April 30, 2012

Qualitative Sociology Review

Volume VIII
Issue 1

Contemporary Issues in Qualitative Research

by

Steven Kleinknecht,
Antony J. Puddephatt
& Carrie B. Sanders

Available Online
www.qualitativesociologyreview.org

QSR Qualitative Sociology Review

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Volume VIII
Issue 1

Contemporary Issues in Qualitative Research

Editors of special issue:

Steven Kleinknecht,
Antony J. Puddephatt
& Carrie B. Sanders

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INTRODUCTION TO THE SPECIAL ISSUE

QUALITATIVE ANALYSIS CONFERENCE 2011: CONTEMPORARY ISSUES IN QUALITATIVE RESEARCH

The community of scholars that attends the Qualitative Analysis Conference in Canada each year has grown considerably since the event's inception in 1984. The inaugural meeting at the University of Waterloo in Ontario, Canada saw a small number of committed qualitative researchers gather for a conference that was first called, "Deviance in a Cross-Cultural Context." Over the last 28 years, the Qualitatives has become an annual touchstone for qualitative researchers in Canada and abroad – attracting participants from all regions of Canada, as well as the United States, Mexico, Britain, France, Germany, Norway, Poland, Australia and Hong Kong. Last year, nearly 200 people attended the three-day conference. The theme of the 2011 conference, Contemporary Issues in Qualitative Research, was established to focus attention on both the application of qualitative methods and the debates surrounding qualitative inquiries, with a consideration of the implications for interdisciplinary standards for systematic qualitative research. The theme was broad enough to ensure a spectrum of papers from different disciplinary, theoretical, and methodological orientations. We encouraged presenters to focus their attention on current issues facing qualitative research, and to explore new and enduring challenges to qualitative methodologies, such as research standards, the integration of technology, the role and influence of emotionality, the researcher's place in field research, ethical regulations and boundaries in the field, and team-based qualitative approaches.

We had the privilege of hosting Patricia and Peter Adler as Keynote Speakers, who spoke about the lessons learned through their many experiences conducting field research across a range of social contexts. We were also fortunate to have a number of excellent featured speakers on hand: Adele E. Clarke spoke about the need to take the non-human into account in qualitative research; Christine Hine discussed dilemmas in conducting online research; and Robert Prus argued for the use of generic social processes to enable lasting, trans-historical, comparative-based knowledge through the practice of ethnographic research. To further stimulate a discussion around standards and contemporary advancements in qualitative methodologies, we held a panel to discuss funding priorities in qualitative research with representatives from major Canadian and American granting agencies. Finally, we were fortunate to have a number of journal editors serve on a panel to offer advice on the process of publishing qualitative research. The objective of the Canadian Qualitatives Conference has been threefold: (1) to encourage and nurture novice researchers; (2) to foster a culture that celebrates, values, and rewards the utility of qualitative inquiry; and, (3) to encourage and highlight inter-professional and multi-disciplinary qualitative research. We hope these goals seem worthwhile to the readership of Qualitative Sociology Review, and we encourage you to participate in the Canadian Qualitatives in the future.

As the organizers for last year's event, we decided to pursue an outlet for some of the more insightful papers that were presented in 2011. The editors of the Qualitative Sociology Review were graciously willing to lend the journal's voice to our efforts. As editors, we had the task of vetting papers and putting those selected through a blind peer-review process with the generous help of leading international experts. The process led to three excellent articles. We also had the pleasure of including the Adler's impassioned and timely Keynote Address.

The issue begins with Patti and Peter Adler's Keynote Address from the Qualitatives, "Tales from the Field. Reflections on four decades of ethnography." Four decades of combined research efforts as a highly successful husband-and--wife team provides the authors with a unique perspective on the current state and future of sociological ethnography. Taking us on a journey through each of their major qualitative research projects, they show how their own brand of "classical ethnography" has served to generate rich, detailed, empirical analyses across a host of different settings. From studying the world of drug dealers, to college basketball players, to the social organization of resort hotels, to the interactional power dynamics of student cliques, to the experiences and perspectives of self-injurers, the Adlers have a wealth of insight and wisdom to pass on to future generations of qualitative researchers. They argue that while postmodernist movements of thought have

been creative and critical forces to aid in the development of the qualitative tradition, we must not forget, nor abandon, the core strengths of a classic, Chicago-school style approach to ethnography. No matter how tempting new and trendy methods and conceptual styles may be, there is never any good substitute for the long periods of time required to sufficiently immerse oneself into, and collect rich and rigorous data about, the social worlds of others.

Orlee Hauser's article, "Pushing Daddy Away? A Qualitative Study of Maternal Gatekeeping," questions the role that women play in perpetuating traditional parenting roles. She argues that it is important to consider what both women and men stand to gain and lose through the maintenance of such roles. Hauser notes that her study is not meant to detract from past research regarding men's limited role in the domestic sphere. Instead, she aims to add to the literature by showing how women play a part in restricting men's roles as caretakers of their children and how women benefit from doing so. Her in-depth qualitative approach extends past research on maternal gatekeeping by offering a detailed study of the nuances of each parent's perspective. The result is an insightful analysis of the various ways women limit men's role in the home, their motivations for doing so, and the gains, losses, and feelings of ambivalence mothers and fathers experience as outcomes of their partners' parenting behaviors.

In her article, "A Reflexive Lens: Exploring Dilemmas of Qualitative Methodology Through the Concept of Reflexivity," Suzanne Day develops a comprehensive survey of the ways in which reflexivity is relevant to the practice of qualitative research. Day takes a firm line that improving our approach to reflexivity cannot guarantee greater truth or accuracy in research, but that the concept can be a useful lens with which to understand the numerous complexities and challenges qualitative researchers face in the field. For example, concerns with reflexivity often intertwine with attempts to bolster accuracy, credibility, and validity, or to consider the identity and social position of the researcher and the associated power dynamics in play in the research relationship. Rather than using reflexivity as a tool to try and settle these issues once and for all, Day considers how the concept enables us to think through these thorny issues in new ways, alerting us to the difficult personal, social, political, and epistemological dilemmas qualitative researchers face throughout the process of data collection and analysis.

In their article, "Reflexive Accounts: An intersectional approach to exploring the fluidity of insider/outsider status and the researcher's impact on culturally sensitive post-positivist qualitative research," Amanda Couture, Arshia Zaidi, and Eleanor Maticka-Tyndale engage in a reflexive discussion to understand how their intersecting identities and resulting insider/outsider statuses may have influenced their data collection. Drawing on empirical evidence from their interviews with South Asian youth about sexual intimacy, the authors demonstrate how participants' perceptions of a researcher's insider/outsider status are fluid and how one's perception of the researcher's status shapes research relations and ultimately data collection. The researchers poignantly illustrate how the intersections of their biography (such as ethnicity, race, and gender) as well as their socialization experiences, all influenced the nature of their discussions with participants, their data analysis, and even their own reflexive accounts.

Kleinknecht, Steven, Antony Puddephatt and Carrie B. Sanders. 2012. "Introduction to the Special Issue. Qualitative Analysis Conference 2011: Contemporary Issues in Qualitative Research." *Qualitative Sociology Review* 8(1):6-9. Retrieved Month, Year (http://www.qualitativesociologyreview.org/ENG/archive_eng.php).

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KEYNOTE ADDRESS
TALES FROM THE FIELD: REFLECTIONS ON FOUR DECADES OF ETHNOGRAPHY

Abstract Drawing on careers spanning over 35 years in the field of ethnography, we reflect on the research in which we've engaged and how the practice and epistemology of ethnography has evolved over this period. We begin by addressing the problematic nature of ethical issues in conducting qualitative research, highlighting the non-uniform nature of standards, the difficulty of applying mainstream or medical criteria to field research, and the issues raised by the new area of cyber research, drawing particularly on our recent cyber-ethnography of self-injury. We then discuss the challenge of engagement, highlighting pulls that draw ethnographers between the ideals of involvement and objectivity. Finally, we address the challenges and changing landscapes of qualitative analysis, and how its practice and legitimation are impacted by contemporary trends in sociology. We conclude by discussing how epistemological decisions in the field of qualitative research are framed in political, ethical, and disciplinary struggles over disciplinary hegemony.

Keywords Ethnography; Epistemology; Ethics; Cyber-Ethnography; Qualitative Research; Self-Injury; Deviance; Sport; Socialization; Youth

Patricia A. Adler is a Professor of Sociology at the University of Colorado, Boulder. Her areas of interest are qualitative methods, deviant behavior, and symbolic interactionism. Her book, *Wheeling and Dealing*, a study of upper-level drug dealers and smugglers, is now considered a classic in the field.

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Peter Adler is a Professor of Sociology at the University of Denver. His areas of interest are sociology of sport, sociology of drugs, and qualitative methods. He has written numerous books, most of them with Patti Adler. Their most recent one is *The Tender Cut*, a study of people who self-injure.

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In 2010, the Adlers were the recipients of the George H. Mead Award from the Society for the Study of Symbolic Interaction, the first collaborators to win this honor for lifetime achievement.

We are honored to be standing in front of this group today. It is our hope to galvanize all of the ideas that we have heard in the many sessions here, to reflect back on what has occurred in the twenty-eight years this conference has been held annually, and to provide, through both autobiographical reflection of our nearly forty years in the field as ethnographers and on the youthful exuberance of many of the novice and younger researchers in the audience, an assessment of where we stand today. The history of field work and field workers is a rich one, full of subjectivity, much like qualitative research epistemology itself. People's stories from the field entwine with their lives, as Van Maanen (1988) so brilliantly reminded us in his discussions of "confessional tales," giving a reflexive imprint to their personal and professional histories. We are pleased to take this occasion to reflect back on the way our approach to the field was influenced by our personal biographies in and outside of the academy.

This year, in 2011, we celebrated the 41st anniversary of our relationship. It began on May 5, 1970, a day marked by the tragedy at Kent State when four college students were shot by the Ohio National Guard as they protested against the Vietnam War, and immortalized by the Crosby, Stills, Nash, and Young song, "Four Dead in Ohio." We, too, were protesting at our campus at Washington University in St. Louis, and the force of that collective consciousness cemented the attraction we had for each other into something that has lasted a long time. Thus began a personal and professional career that has spanned four decades, and concurrently, considerable changes in how ethnography is practiced. We were also fortunate to meet our eventual mentor, Jack Douglas, in 1975, when he was in the midst of writing his seminal methodological treatise, *Investigative Social Research: Individual and Team Field Research* (1976), who saw in us a mini-team, perfect for describing the type of team field research he was then advocating.

We began our sociological odyssey at an auspicious location, not only politically but sociologically; within the year prior to our arrival (1968) Laud Humphreys had conducted his field research on "tearoom trades" that would win him a C. Wright Mills Award from the Society for the Study of Social Problems (SSSP), arguably the most prestigious book award given in North American Sociology. The first work to systematically document the nature of impersonal sex encounters at public rest rooms, *Tearoom Trade: A Study of Homosexual Encounters in Public Places* (1970) cast light onto one dimension of the homosexual scene: a venue where men who conceive of and portray themselves as heterosexual can venture, at some considerable risk, into finding impersonal sex with anonymous partners without any emotional connection or obligation. Laud's work was groundbreaking not only for its empirical exploration of this hidden, deviant scene, but for the combination of investigative methods he used to gather the data. This research made him infamous in the

1 This speech was originally delivered as a Keynote Address at the 28th Annual Qualitative Analysis Conference, Wilfrid Laurier University, Brantford, Ontario, Canada, May 2011.

discipline because he used a covert role to gain entrée into these public bathrooms and, taking the role of the "watch queen," systematically recorded the nature of the way his subjects silently approached, signaled, negotiated, carried out, and terminated their transactions, delicately balancing the need to hide their behavior and scene from dangerous outsiders while simultaneously keeping it open for interested participants to locate.

At the same time he surreptitiously recorded the license plate numbers from their cars and, through a friend at the Department of Motor Vehicles, obtained their names and addresses. He later, after changing his appearance, visited their homes and used a short questionnaire he was concurrently administering for an epidemiological survey through the medical school to find out information about their lives and demographic characteristics. This information helped establish the liminal nature of people who perform these homosexual acts in the gay scene and their primary involvement in a middle-class, heterosexual, establishment lifestyle.

Humphreys' research tore apart Washington University's sociology department as Professor Alvin Gouldner, the resident theorist and a known curmudgeon (see Galliher 2004), lambasted Laud (a graduate student at the time) for the ethics of his covert role and misdirection. A graffiti war sprung up around the department with anonymous postings appearing on bulletin boards (real, not cyber!) that took swipes at members of the faculty and graduate students, who were splintered into fractious camps. Eventually Alvin blew up after one inflammatory posting that criticized him. Because of some Latin phraseology in it, he inferred that it had been written by Laud, who had been a member of the clergy prior to entering graduate school. Subsequently, he is alleged to have punched Laud Humphreys in the face, sending him to the hospital. The department then exploded, with most of the people leaving both Washington University and St. Louis. Our sociological careers began, then, at a site of great professional conflict (see also Adler and Adler 1989a).

We also began our journey in the midst of the countercultural revolutions of the 1960s and '70s. This era was marked by great innovations and revolutions in higher education; people were being rewarded for thinking outside the box. When we were in college, the freedom to explore, to create, and to otherwise develop naturally, was part of the new ethos. Any of us who were in school in those days can point to programs and progressive reforms that were designed to enhance student freedom and to encourage greater individualism: schools without walls, open classrooms, open campuses. Although the media sensationalizes the '60s for the rampant use of drugs, for women burning their bras, and for casual sex, those times had a value system that advocated community, that saw an unjust war that needed to be stopped, and that realized that rote regurgitation and memorization in college curricula was not the best way to learn. From that freedom of thought would come revolutionary ideas about education that we take for granted today, such as internships for credit, service learning, pass-fail courses, auditing, practical experience, and courses about all forms of popular culture, much of it controversial. Today's educators claim they want to get back to basics ("no child left behind"), that North America is lagging behind other industrialized countries academically, but they ignore the creativity and autonomy that leads to great ideas and new forms of society.

We extended the unconventionality of this setting and time. Intellectually fascinated by academia, we found ourselves, as sophomores, taking classes and discussing our take-home exams together in great depth. Once we had thoroughly shared our ideas, we had difficulty disentangling them, and so we approached our professors to see if they would permit us to complete our work collaboratively. Testament to the values of the era, they agreed, challenging us to make our work twice as good as we could individually; we since have taken that as a career mandate. Thus we launched a conjoint career (see Adler et al. 1989) that has been unusual in our field, which was met with a great deal of acrimony in our early professional years (the "Lone Ranger" approach to scholarship was strongly advocated), but which has withstood the test of time. In fact, we know of few other couples so closely aligned in any field. We were honored to have been the first collaborators to win the George H. Mead Award for Lifetime Achievement from the Society for the Study of Symbolic Interaction (SSSI) in 2010.

In this essay we would like to both reminisce about our experiences in the field, as well as about the times in which we have written, the settings we have studied, and the ways in which we have gone about doing our ethnographies to reflect more generally on some aspects of the state of qualitative research today. Along the way, we will talk about ethical, methodological, and epistemological issues related to ethnography and the changes that we have seen in the past three to four decades.

WHEELING AND DEALING

Beginning our study of sociology in the shadow of Laud Humphreys' work, criminology and deviance were our first loves in sociology. We were drawn to major in this field by a particularly charismatic professor, Marv Cummins, and one class in particular. Standing up on a demonstration table in the front of a large, sloped lecture hall, Cummins illustrated how professional burglars break into buildings without shattering their glass windows or tripping the alarm systems. The more we heard, the more we wanted to know the finer details of how these people mastered their craft; we became fascinated by occupational criminality. Our first opportunity for research came when we were undergraduates: we were recruited to join a funded research team studying heroin use in the greater St. Louis area. For this project we dug through emergency room records, hung out at methadone clinics, and interviewed heroin users about their experiences with drugs and the law. Although the people we were studying were very different from us and using harder drugs than the students in our classes (who were part of the hippie movement and smoked

or ingested mostly marijuana and psychedelic drugs), we were able to connect with these people through our nonjudgmental fascination with their lives and curiosity about their drugs of choice. Chosen to accompany our professor to the Kennedy School at Harvard University, where members of other teams from around the United States gathered who were also studying heroin use in their own metropolitan areas, it was our first introduction to high-level academics, the power of research, and the impact that our work in the field could have on theory and praxis. We applied to graduate school with the intention of studying and extending Chicago School sociology.

Enrolling in a Master's program at the University of Chicago in 1973, we quickly learned that the Chicago School, save one or two faculty, had emigrated years earlier when Blumer left the Midwest, and was then chiefly practiced on the West Coast (see Vidich and Lyman [1985] for a discussion of this burgeoning "California School of Interactionism" in the 1960s and '70s). After obtaining our first graduate degree, we sought a Ph.D. at the University of California, San Diego (UCSD), a program founded by Joseph Gusfield, a Chicago graduate, for the express purpose of replicating the energy and synergism of the Chicago School, especially the second generation (see Fine 1995). By building the strongest faculty in the country dedicated to qualitative research, in the shadows of California's new lifestyle, more openness to alternative ways of living, and prosperity, Gusfield hoped to create a program, like no other, that would be the centerpiece of American sociology in the ethnographic tradition. Students and colleagues of Howard Becker, Herbert Blumer, and Erving Goffman were gathered there, including not only Gusfield, but Jack Douglas, Fred Davis, Jackie Wiseman, Murray Davis, and Bennett Berger, forming a strong symbolic interactionist base. In addition, out of this fervent group emerged graduate students who later would become key contributors to symbolic interactionism and ethnography: Carol Warren, John Johnson, David Altheide, Andy Fontana, and Joseph Kotarba. They were joined by an ethnomethodological contingent comprised of Aaron Cicourel, Bud Mehan, Bennetta Jules-Rosette, and Reyes Ramos, as well as theorists, such as Randall Collins and César Graña, who were seeking to make the macro-micro connection in sociological thought. It was here that we learned our strong foundation in the history, epistemology, and practice of qualitative and interpretive sociology.

Casting around for our first research project, we became intrigued by our neighbor's "no visible means of support" lifestyle. Familiar with it from our undergraduate subculture and drug research, we enthusiastically accepted the opportunity he offered to understand the lifestyle and practice of his upper-level smuggling and dealing scene. Right away we were enmeshed in a world of the occupational criminals that initially enticed us into sociology and criminology! As we became more deeply involved in the community and its friendship circle, we had our first encounter (in the mid-1970s) with the just-developing university committee designed to regulate research. One of our advisors insisted that we clear this project through the Human Subjects Committee (now more likely called an Institutional Review Board, IRB), a procedure that was, at that time, optional. To gain approval we would have had to require our friends and acquaintances to sign consent forms with their real names, which they would have refused to do, had we even asked. We would also have had to announce to people, upon initial encounter, that we were studying them, which (our closest friends advised) might have been hazardous to our health, let alone the pursuit of science. Thus, we never did get official university approval for the study, something that could never happen today.

In order to get close enough to the members of the scene to learn about their lives, to understand deeply their perspectives, their joys, and their conflicts, it was necessary to hang out with them regularly, to be accepted into their social circle. Spending time with them required our willingness to engage with them in their leisure pursuits, part of which involved smoking pot and snorting cocaine. Since we were children of the '60s and liked these drugs, we were comfortable with this, even considering it a perquisite of the research. If we had refused to participate in this drug use with them, we would not have been accepted or trusted. We never dealt drugs (although we were offered the opportunity many times, and, to the dealers' constant surprise, declined), but we certainly witnessed many drug deals.

In writing about the methods for this research, which we entitled *Wheeling and Dealing* (Adler 1985), we declared our drug use frankly as a critical source of entrée. Throughout our careers, we have never received any professional censure for this admission. In fact, much to our surprise, we were consistently lauded for our honesty, straightforwardness, and courage. We hope that this was one of the precursors for a more frank and open approach to ethnographic methods than had been practiced, which emerged just a few years later, with the birth of the postmodern turn. The only time our stance ever raised eyebrows was in a presentation we made to the National Institute on Drug Abuse (NIDA), where the proceedings editors politely asked us to censor that part of our methods discussion for the government publication. But our verbal admission at the Washington DC conference was seen as courageous by other qualitative (funded) drug researchers. Our work was well received, and we were grateful to have avoided the notoriety that plagued Laud Humphreys.

BACKBOARDS AND BLACKBOARDS

In 1980 we moved to Tulsa, Oklahoma, a region so foreign to us culturally, geographically, and personally, that we found fitting in there difficult, at best. Yet, academia was a "publish or perish" profession then, as it is even more now, so we were eager to find another topic for our next study. We have always been strong proponents of studying "in our own backyards" (experience near, as opposed to the experience far, of most anthropologists of the day). At the time, we found one of the local colleges, Oral

Roberts University (ORU) fascinating, but we knew we were not the people to do this research project. We were New York Jews, precisely the kind of people that these evangelical Christians had been taught all their lives not to trust or befriend (though it should be noted that Alan Peshkin [1984], also a Jew, was able to do ethnographic research in a similar high school setting). We could not forge the subjective connection necessary to do participant-observation research in an unbiased manner. From studying the exciting lives of upper-level drug traffickers, we found ourselves, instead, writing about middle-class parents who carpooled their children to and from school (Adler and Adler 1984).

One day, though, Pete gave a reprint of an article we had written about momentum in sports (Adler and Adler 1978a) to one of his students, an intercollegiate basketball player, who was excited to read about something so close to his experiences. He took the article to his coach to read. The coach liked what he read, because he figured that if this professor knew how to capture momentum, it might help his team win games. He then invited Pete for a meeting, which led to a talk to the players. The interactions went so well that Pete was invited back as often as he wanted. Before long Pete was a regular fixture with the team, hanging around during practices, helping players arrange their academic schedules (before the institution of academic advising became widespread for athletes), sitting behind the bench at home games, and traveling on short road trips with the team. His vast storehouse of athletic trivia and insight into athletics, academia, and life in general forged a strong bond between himself and the coach(es) and players. They gave him the moniker of "Doc." Patti took the role of the coach's wife, and befriended the other wives and players' girlfriends. We fed team members at our house most Sunday nights and socialized with them after practices and on the road.

After a year or so, Pete's role as an academic advisor started getting media attention and he became the subject of considerable print, radio, and television coverage. He was catapulted into the celebrity that the team members shared as their winning seasons increased and they acquired league and national championships (see Adler 1984). He lived as one of the team and shared the experiences and feelings of team members, something that we considered essential to an existential understanding of the scene. There were times, in fact, where he was asked for his autograph in public, and was constantly pressed by fans to give assessments of the team and if they were ready for the next game (or season).

Although this role brought Pete closer to the emotional and lived experiences of the players, there were times when his analytical perspective on the scene got sidetracked. Here, our team approach was especially valuable because Patti would debrief with him into a tape recorder after particularly important experiences, would remind him to write field notes, and would brainstorm with him about the development and modification of important analytical concepts. During this research we turned an oft-repeated phrase from coaches to players that they should "get with the program" into an article about the concept of organizational loyalty (Adler and Adler 1988). Our longitudinal, in-depth involvement with individuals and the team enabled us to trace and write about the identity careers of college athletes as they progressed through college, dealing with all of the allures and pressures. We wrote about the role conflict they encountered between their athletic, social, and academic roles, and how they resolved it. As time wore on, we wrote the story of their lives (Adler and Adler 1991).

But we also thought long and hard about what we should not write about, in this research as well as the one on drug trafficking. It is a maxim in sociology that people only write about the second-worst thing that happens to them, and we probably held back in similar ways. After thinking about this and wrestling with it, we wrote an article about self-censorship in field research (Adler and Adler 1989b), discussing this practical and ethical dilemma.

MEMBERSHIP ROLES IN FIELD RESEARCH

After six long and personally arduous (but academically productive) years we left Tulsa in 1986. We returned to a town and school we loved, taking one-year teaching appointments at our alma mater, Washington University in St. Louis. At around the same time, we were asked to become journal editors, taking over *Urban Life* and changing its name, in concert with Mitch Allen, the editor for Sage, and John Lofland, the journal's founding editor, to *Journal of Contemporary Ethnography.* This was a labor of love for us, the first journal to which we had unfettered allegiance and admiration (we published our second peer-reviewed article there). Working before the days of electronic submission, review, and correspondence, we enjoyed editing others' manuscripts, meeting with authors at conferences to discuss their work, and to some degree, shaping the direction of ethnography at the time.

We continued to write about our basketball research and reflected on epistemological issues we were encountering in the field. We thought about the similarities between the drug dealing and basketball projects and our approaches to them. Schooled by Jack Douglas' approach (Douglas 1976), we had a strong commitment to in-depth, participatory research. We contrasted Pete's coaching and advising role in the basketball research, as a coach on the team, and our role in the drug dealing research as friends, neighbors, and roommates of drug dealers. These both differed in significant ways from what we had been taught in our graduate school books espousing second-generation Chicago School epistemology. The Chicago School approach from the 1950s and '60s advocated a "fly on the wall" position. In writings by Gold (1958) and Junker (1960) that outlined the range of appropriate research roles, we were advised to tread a fine line between involvement and detachment, between subjectivity and objectivity. We could be observers-as-participants or participants-as-observers, but there was a lot of negative rhetoric about "going native."

We felt in our guts that we (sociologists) were being taught wrong. What the literature defined as going native seemed, to us, a necessary field research experience. How else were we to truly understand the existential reality of how people felt? If we didn't understand how they felt, how could we understand how and why they acted? Symbolic interactionism put a lot of emphasis on rational cognition, on taking the role of the other and assessing possible outcomes of behavior, and on aligning joint actions. But in *American Social Order* (Douglas 1971) and *Existential Sociology* (1975), Jack Douglas and John Johnson had written about the existential reality of life, the fundamental importance of feelings ("brute being," as they called it) over rational thought, and this resonated with our experience in both field settings.

It was not the detachment, the distance, or the objectivity, we believed, that made a research project great, it was the involvement, the closeness, and the subjectivity. We never heard anyone praise an ethnography by saying, "Wow, you really kept your distance from the participants." Rather, research generated credibility by the closeness of researchers to their respondents and by how well they captured the essence of the lives and perspectives of the people they studied. Drawing on *Investigative Social Research,* in *Membership Roles in Field Research* (Adler and Adler 1987) we had called on researchers to embrace subjectivity, to recognize that all people and groups had insiders' and outsiders' knowledge, and to place critical import on penetrating the outer (and inner) layers of front work. Unbeknownst to us at the time, there were similar murmurings in anthropology (see Clifford and Marcus 1986) and among a small, but rapidly growing cadre of sociologists led by Norman Denzin (1989), who were advocating comparable epistemological changes in ethnographic practice.

In our treatise, we went beyond Douglas to argue that all researchers needed to take membership roles in their research. In our drug dealing research we had taken a *peripheral* membership role: we became members of the social setting, but did not engage in the core activities of the group (dealing). Yet, we got closer to this upper-level group of dealers than researchers previously were able to penetrate. They became our closest friends and we socialized primarily with them, worked with them in their legitimate front businesses, babysat their children, traveled with them, visited them in jail, testified for them in court, and invited our closest friend to move in with us when he got released from prison. We are proud to say that these friendships still endure, and that we visit and speak with our key friends from this research on a regular basis, more than 35 years later.

In the basketball research Pete took an *active* membership role, participating in the work for the team as an academic coach and as an advisor to the players and coaches. He planned players' schedules, helped them interact with their professors, guided them in life, and served as a friend and role model. He consulted with the coaches and helped them understand the way the university operated and the place of athletics within the political realm of the academy.

His highly visible position on the bench and in the media engendered considerable jealousy among his faculty peers, and he was explicitly told after a few years to pull back from such a public role or it would jeopardize his chances for tenure.

There were also times when we worried that such an active role in the setting might contaminate the data, because Pete worked hard to counteract how the athletic realm had an insidious effect on the players. He urged players not to neglect their coursework, to pursue their degree, even if it seemed unimportant to them at the time. He tried to put their chances of making it in the NBA in perspective, so that they would recognize what their life options were more realistically. But in affecting the data, we learned the hard way about the obdurate reality of the setting: no matter how hard we tried to change it, we could not. Coaches dangled the NBA in players' faces to rivet their focus on their athletics, despite their genuine concern for them as individuals with non-athletic futures. Players ate, slept, and dreamed about making it in the big leagues, despite Pete's admonitions. And it wasn't until years later, when we returned to Tulsa to participate in the wedding of one of the players that several of those who had never graduated reflected on their lives, thanked him for trying to wake them up to the fantasy that held them entrapped. "You were right, Doc," they said. "You told it like it was, but we wouldn't listen." We remain friends with a handful of people from this setting today.

Little did we know that our next project would fall into our third research category: the *complete* membership role.

PEER POWER

After a year in St. Louis, we moved to Boulder, Colorado in 1987. *Membership Roles* had just come out and we were writing *Backboards and Blackboards.* It has always been our practice to overlap the last few years of a research project, when the data were mostly gathered and we were spending more of our time writing, to begin our next study. That way, by the time the research was published, we would be a few years into the next setting and adequately immersed in it to begin writing. From start to finish, in a career of forty years, we have spent nearly ten years on each of our five major ethnographies (with assorted projects in between).

As usual, we turned to our backyards, this time literally. As we progressed through our careers, we continued to believe, epistemologically, that we should overlap our research lives with our private lives. That way, we could participate fully in our research settings. It was not possible, we thought, to understand a scene and its people without being there on the weekends as well as the weekdays, in the evenings as well as the daytimes, during periods of crisis as well as times of calm and routine. We sat back and let something interesting drift toward us, keeping our sociological imaginations and curiosity engaged.

In the drug dealing research we had started by studying our neighbors. The basketball research was launched by our knowledge of our student(s). This time it was the lives of our children that captured our sociological interest. With our theoretical orientation toward symbolic interactionism, we had long been fascinated by children and socialization. We thought about famous scholars, such as Charles H. Cooley, Erik Erikson, and Jean Piaget, who studied their own children, seeing in them the laboratories of human nature. In San Diego we wrote about the intergenerational socialization to deviance that we saw in "tinydopers:" the children of pot smokers who smoked pot (Adler and Adler 1978b). While our Tulsa years saw us writing about carpooling, by the time we moved to Colorado, our children were older and their lives were becoming more engaging. Our children's social worlds enticed us as an object of study, not only because they were fresh, challenging, important, and unbelievably complex, but because studying them offered us the ancillary benefit of spending more time with our children during their important and formative years.

Our daughter, who was nine-years-old and in fourth grade when we arrived, seemed to have a nice life: she had friends, made dates, danced, and enjoyed school. But at the end of that first year something happened to disabuse us of our complacency. Our first glimpse behind the scenes of this happy front came at an elementary school end-of-year party when the mother of another girl said she wanted to scratch our daughter's eyes out. "What had she done?," we worried. This mother told us that she was transferring her daughter to another school because of our child. We asked our daughter, but got inadequate responses.

At the start of the following school year, we discovered that our daughter's best friend had been banned (by her mother) from playing with her. Separated from her best friend and shuffled into a new class, she had to make friends. She was drawn into a group of popular girls dominated by a manipulative clique leader. With our daughter now at the receiving end of trouble, we became aware of the complex drama of these girls' interactional clique dynamics and their cruelty. She had apparently been mean to girls the year before, and now when she was getting emotionally beaten up by a more skilled alpha leader, she had few places to turn. She experienced the drama of the ups and downs, the inclusion and exclusion, the vicissitudes of leadership and followership.

Our son went through some similar dynamics. Although he was originally accepted socially for his athletic skills, by fourth and fifth grade he was dropped by his former friends and became shunned as a pariah. He was tormented by clique leaders and bullied by those who would curry favor with them, and beaten up. In parent-teacher conferences we were told that his life was a daily hell. This was altogether too much drama to ignore. What made kids so popular, we wondered, that people could rise and fall like this? What gave clique leaders so much power that they could command such heinous behavior from their followers and make others' lives so miserable? How did kids this age learn to read the subtle and shifting currents so they could go with the flow and not get cut to shreds in the crossfire? We have always felt that the answers to these questions, published in *Peer Power*: *Preadolescent Culture and Identity,* (Adler and Adler 1998), offered the most generically applicable models of the social world of our careers, as the clique dynamics we described there pertain just as well to the micro and macro politics in all forms of everyday and organizational life as they do to children's worlds.

Entering into children's worlds is not always easy for adults, as children spend some time in the private company of their peers and other time in institutional settings to which access is restricted. By taking the role of the "parent-as-researcher" (see Adler and Adler 1996), we capitalized on a naturally occurring membership role where our presence was less artificial and unwieldy, where we already had role immersion, and where the need for role pretense was diminished.

In this research we occupied several parental roles in different settings. We interacted with children, parents, teachers, and school administrators as parents-in-the-school, volunteering in classrooms, accompanying field trips, organizing and running school carnivals and other events, driving carpools, and serving on school committees. We interacted with children, parents, other adults, and city administrators as parents-in-the-community, coaching and refereeing youth sports teams, serving as team parents, being the team photographer, organizing and running the concession stand, and founding and administrating our own youth baseball league. We interacted with children, their parents, neighborhood adults and children, and adult friends and their children as parents-in-the-home, being a part of our neighborhood, having friends in the community, interacting with the neighborhood and friendship groups of our children, offering food and restroom facilities (our house bordered the neighborhood's playing field), nursing children through illnesses, injuries, and substances abuses, helping them with their school decisions and schoolwork, functioning as mentors and role models, serving as friends and confidantes, bailing them out of jail and other troubles, and helping them talk to their own parents.

One of the key perquisites of this research was that we did spend a lot of time with our children. But there may have been ethical issues that we did not consider at the time. At a small conference on ethnographic studies of children another presenter challenged our research role: "I'd hate to be the Adlers' children," she said. Was there something we hadn't thought about, some abuse of power we had inadvertently taken into the relationship? Would they hate us forever for that? These dilemmas illustrate some of the difficulties of the *complete* membership role in research, showing the way any epistemological perspective engenders trade-offs.

PARADISE LABORERS

Our first foray into doing distance ethnography, beyond our own backyard, came with our

study of the occupational culture of the Hawaiian hospitality industry, *Paradise Laborers. Hotel Work in the Global Economy* (Adler and Adler 2004). We visited Hawaii in 1992 and fell in love with it. Each time we returned, it spoke to our souls more profoundly. Looking for some way to facilitate regular travel there, we seized on research. We were fascinated by the complexity of resort hotels, the management philosophies guiding their operation, their multi-cultural workforce, and the ironic juxtaposition of people working to facilitate the leisure of others. As we delved deeper into the arena, we found an enormous richness of language, culture, and social stratification. Although we began the project as tourists, we eventually managed, over the course of several years, to make it into our backyard, getting teaching jobs there at the local college and eventually building a house. Once again, we joined our research and personal lives.

Living in one place and doing research in another, part-time, presented us with the difficulty of traveling back and forth to the field and not having the research setting continuously available. Establishing the kind of membership role we had previously used was harder. We rented a cheap condo, checked in and out of various hotels along one particularly desirable strip of property, joined their membership programs (and even had one modeled on us), and proceeded to hang out with the employees. We infiltrated the four different worker groups we found there (locals, new immigrants, management, and seekers) through various routes, as guests who took their work and lives seriously, as professors at the college, through our participation in local and hotel activities, and ultimately as friends. Eventually we applied to one hotel for permission to study it.

Dealing with an organized group proved rather different from negotiating entrée with unorganized individuals: we had to navigate relations with the gatekeeper (the general manager). The transience in the hospitality industry, and particularly in the hotel we had chosen, stymied us because each time we thought we had forged a relationship with the current GM and gotten permission to study the property, he was fired and replaced; we had to start all over again. We experienced several heartbreaking disappointments as we arrived, ready to begin our formal research, only to find someone new at the helm who did not know us. Eventually, however, our perseverance paid off and we achieved entrée. We then experienced the benefit of studying an organization, as, once cleared by the GM, we were accepted by most employees and could observe backstage behavior, schedule interviews with management, and wander around the property taking notes openly. Some workers admitted to us that they had heard rumors that we were management spies, but once they got to know us, we easily dispelled that impression.

After several years we branched out beyond our first hotel to do a more comprehensive ethnography of all the hotels on the strip. Yet, rather than seeking formal, organizational entrée to the other resorts, we decided to reach out to individual employees in their leisure time; we

had plumbed the management category adequately. By this phase of the project, we knew enough people to snowball from one contact to another, and saturated our penetration into each of the four groups of workers through our connections to students, exercise partners, neighbors, people we had already interviewed, and friends.

This research project took us into literatures that were far afield from those we had previously encountered, a feature much more common to qualitative than quantitative research enterprises and careers. We wrote about organizational and ethnic stratification, labor relations, the economics of development, the postmodern self, and work/leisure. Aside from the obvious perquisites of spending time in a beautiful and romantic paradise, this research had the benefit of acquainting us with some fascinating new literature. And, once again, doing this longitudinal, in-depth research project impacted our lives and those of the people we studied in profound, reciprocal ways.

THE TENDER CUT

Our most recent research, *The Tender Cut: Inside the Hidden World of Self-Injury* (Adler and Adler 2011), also called to us, but in a different way. This was the first time we moved away from our long-time commitment to in-depth participant-observation and researching in our backyards. We first heard about self-injury (although not by any name) in 1982 when a student of Pete's, in Tulsa, confided in him about the myriad cuts on her arms. Over subsequent years we both caught further glimpses of similar behavior. As interested and "cool" professors who taught courses on deviance, popular culture, drugs, and sport, we often found ourselves the adults to whom college students turned as sounding boards. Our next encounters with cutting were rare at first, but took on greater frequency during the late 1980s and early 1990s. By the mid '90s we knew or had heard about enough people who cut themselves intentionally that we felt surrounded by it. Yet, during the occasional times when we discussed this with friends or colleagues, we found it fundamentally unknown. Then, in the spring of 1996, a young high school-aged friend of ours, the daughter of close friends, confided in Pete about her cutting. She had never mentioned it to her parents, but she needed someone to talk to about it. Pete was her college advisor (one of his side avocations), and they had a close relationship. This very detailed, intimate conversation caught our attention. We felt the behavior was calling to us to study it, but we were squarely in the middle of another major research project and did not have the time.

We were attracted to the project because it meant a return to deviance, our first love, and because we believed we could be nonjudgmental about the topic. In contrast to the difficulties we had in trying to get clearance for studying drug dealers, we naively thought IRB approval for this topic would be easy: the behavior was deviant, but not criminal and people were only harming themselves, not others. We also thought that since our early conversations with people about the topic brought shock and surpri-

se, with no recognition, we would have great difficulty locating people to study. We couldn't have been more wrong on both counts.

Our first shock came when the IRB told us that they believed self-injury was associated with suicidality, meaning those who cut were a vulnerable population. Next, we were required to use and adopt the psycho-medical perspective in defining this behavior, in reviewing the literature, and in accepting the causes, effects, and general demographics of the population. This was the first sign of the hegemony of the psycho-medical perspective and their "ownership" of the domain. After our first set of revisions, we were then required to provide subjects with referrals to clinicians who provided psychotherapy or counseling on self-injury cessation, something we suspected our subjects might not appreciate. This was not the value neutrality of Max Weber, and not the nonjudgmental way we wanted to start our conversations with these people. In interviewing minors (a potentially significant percentage of the population) we were required to obtain minor assent and parental consent. That was a really big impediment, since all of the subjects we had talked to personally had kept their injuring hidden from their parents and nearly everyone else. Being limited to only minors "out" to their parents would involve a significantly biased population. But we pressed on. After another round of revisions we were told that we could not directly solicit interviews from people, but only "put it out there" that we were interested and invite those who wanted to participate in our research to contact us. After nearly two years of revisions we were ready to begin the study.

As an unorganized group of non-affiliated individuals, self-injurers could not be studied through participant-observation, the usual means we had employed. They were a highly hidden population. It was hard to find subjects at first, but through word-of-mouth and via media interviews, people began to learn of our interest. Surprisingly, they came to us to be interviewed. When we asked them (at the end of the interview) why they had come forward, they said that they hoped we would write about the behavior so that others could read it and learn that they were neither alone nor crazy. Many recounted horrible experiences at the hands of parents, high school counselors, primary care doctors and pediatricians, and emergency room physicians, from which they hoped to spare others. This moved us deeply, and we became committed to represent their voices and their perspectives.

As we continued to interview people in our offices face-to-face, we began to be aware, in the early 2000s, of the rise of self-injury being discussed on the Internet. Websites, blogs, diaries, listservs, and bulletin boards were cropping up where people wrote about their experiences and posted photos, poems, and artwork. Since these were public sites, we visited these and recorded the data. But could these data be used? At that time the practical and ethical standards for Internet research were unclear and conflicted. Not much was published on it, as it was a nascent field. We wanted to expand our research there, because, with nearly 40 interviews completed, we had become somewhat empirically saturated. A slippery epistemological slope, there were no standard norms guiding qualitative Internet researchers. We had to "wing" it, therefore, to the best of our ability. We read public postings. We joined several groups as overt researchers for the simple ease of having postings delivered into our boxes, even though the sites or boards were still publicly accessible. We participated in online conversations and made online friends in various communities. But it was difficult if not impossible to make our research interests known every time we visited a site or read emails or postings. We renewed our protocol, gaining permission to use this material.

In our next renewal we applied for permission to solicit people online for interviews that we could conduct over the phone. Again, the IRB presented us with problems. How would we ascertain the age of subjects? Although we specified that we were only interested in talking to people 18 or older, we had to trust what they told us and try to cross-check that against what they wrote in their postings. The IRB required that when studying minors (a trickle of people at most) we needed to further "verify" that the parents were who they said they were. How were we supposed to do this? We arranged to telephone a parent of potential subjects to verify their age.

The subsequent year, in renewing our protocol we were told we had to expand our parental permission of minors by having written permission of both parents or a parent and a legal guardian. In actual fact, however, many minors who self-injured did not have two parents in the home. We had only interviewed two minors to date, and we did not recruit any more that year.

In our next renewal we were told that our consent form had to include a warning to parents that if they knew about their child's self-injuring and did not "do something about it," we would be forced to "report them." What did that mean? What would constitute an acceptable threshold of doing something about it on the parents' part: having a conversation with their child; sending the child to a therapist; putting the child on medication; taking their child to the doctor, or checking their child into a psychiatric hospital? This was pretty unclear. Further, to whom should we report recalcitrant parents? The police? Social workers? The IRB? The self-injury police? Epistemologically this felt all wrong. How could we live with thinking about turning in someone who was trying to help us with our research? At this point we officially dropped minors from subject recruitment.

We were able to use the Internet to successfully recruit subjects from all over the world. We conducted telephone interviews with people in Europe, the South Pacific, and North America. At the same time we continued to interview people face-to-face, but only after screening them to see if their experiences advanced our knowledge empirically or theoretically. By this point we were turning down interviews in person with a high degree of frequency. The completed study draws on over 135 in-depth, life-hi-

story interviews, conducted in person and on the telephone, constituting what we believe to be the largest sample of qualitative interviews with non-institutionalized self-injurers ever gathered. Participants ranged in age from 16 to their mid-fifties, with many more women than men (85 percent women and 15 percent men), nearly all Caucasian. Over the course of our research we also collected tens of thousands (in the range of 30,000–40,000) of Internet messages and emails including those posted publicly and those written to and by us.

But when we were finishing the book in 2009–10, more epistemological and ethical questions arose. Revisiting sites we had not carefully examined for years, other than occasionally posting research solicitations, we noticed that several of them had gone "membership only." What did that mean? What about the data we had gathered when they were publicly accessible? When did that change? The ethical issues seemed even murkier than they had originally and were fraught with problematic possibilities. We had three or four chapters outlined and filled with quotes and field notes that might possibly have come all or in part from these sources. Could we use them? Should we use them? What were other people doing? Again, there was no real consistency in ethical standards. We decided to try to find a middle ground by working with data from primarily publicly accessible sites and using email messages or postings that would not identify the posters or sites. This resulted in our eliminating three chapters from our manuscript.

We are pleased to say that the book has been published and we have received numerous emails from people we interviewed who bought it, read it, and thanked us for the way we portrayed them and their behavior (for "giving them voice" in a world in which they were mostly unheard). As in our other research projects, we still correspond with several of our closest online friends about all aspects of their lives.

STATE OF THE FIELD TODAY

We end this Address by assessing the contemporary state of ethnography today. In so doing, we celebrate the success of the efflorescence and spread of ethnography. From sociology to anthropology, from urban studies, ethnic studies, cultural studies, to feminist studies, from education to medicine, law, business, journalism, communication, ethnomusicology, history, literature, and more, we have seen the rise and growth of field research. Ethnography in contemporary academia ranges in character from anecdotal to narrative, formal, partial, experimental, textual, and all types of other forms and genres. Ethnography remains a field that may claim to be the "most scientific of the humanities and the most humanistic of the sciences" (Van Maanen 2011:151). In our pluralistic world, subcultures have flourished, and with them the opportunities for describing and analyzing them. Writings about ethnography have become a huge industry, stretching beyond ethnographies themselves to numerous encyclopedias, handbooks, manuals, anthologies, literature reviews, talks and presentations, journal articles, monographs, blogs, message boards, social networking sites, online publications, listservs, and chat rooms. The good news is that ethnography has gone from being the primary approach of anthropology and a small portion of the sociological discipline, to becoming used, accepted, and legitimated within a huge range of social scientific and other approaches.

At the same time, this spread has also occasioned the dispersion and diversification of the approach. This segmentation raises an issue of concern: the evolution and splintering of the field. In a sub-discipline where we should all be related, as kin of sorts, working together in harmony, there is fragmentation. Some of this may attest to the success of the interpretive movement more broadly, but some of it may portend its dissolution and decline.

We first introduced our idea of the "Four Faces of Ethnography" in our Presidential Address before the Midwest Sociological Society (see Adler and Adler 2008) to talk about some of the different genres in ethnographic work and representation. Building on the literature, analyzing the rhetoric and representation in ethnography (i.e., Geertz 1988; Van Maanen 1988, 1995, 2011; Atkinson 1990, 1992; Hammersley 1991; Denzin and Lincoln 1994 are some of the earliest progenitors), we proposed four styles of representing ethnographic research that are geared toward four different audiences: Classical, Mainstream, Postmodernist, and Public. While no one typology can adequately address the range and breadth of ethnography, we revisit this concept to analyze where the progenitors of these original representations appear.

MAINSTREAM ETHNOGRAPHY

The hegemony of the discipline still resides in the mainstream journals. These accord a small amount of space to qualitative research. Even when the journal, *Social Psychology Quarterly* was in the hands, first, of Spencer Cahill, and then Gary Alan Fine, two editors who should have been able to entice more ethnographers among their submitters, the number of qualitative works that were published under their tenures did not increase significantly. Field researchers who want to place their work in these more highly ranked outlets need to understand how to translate their ideas from the lexicon of classical ethnography to that of mainstream sociology. Mainstream reviewers and editors are often confused about how to evaluate ethnographic work because there is not as great a consensus about standards as there is for quantitative work. They assume a hypothetico-deductive model of research, into which ethnographers may have to try to fit themselves. This is most particularly evident in certain sections of an article, such as the Introduction and the Methods discussion. Validity and reliability are core concerns. To attain publication in these outlets with the prestige and widespread audience that they offer, qualitative researchers must justify their use of field research to a mainstream audience, to rationalize an often intuitive research process, and to sterilize subjective elements of the research. Although there have been some attempts to publish in these venues, most ethnographers reject the mainstream concept that a rigorous methodological blueprint, pre-de-

termined before the research begins, accompanied by a rhetoric that requires legitimation in positivistic terms, adheres to a terse and obtuse writing style, and revolves around obdurately fixed and flat definitions and formal analysis is necessary.

PUBLIC ETHNOGRAPHY

Over the past decade or two we have seen a rise in the prominence of public ethnography, the presentation of qualitative field research in a form accessible to the intelligent lay reader. First-generation exemplars of the field include people such as Elijah Anderson, Mitch Duneier, Katherine Newman, and Philippe Bourgois. Public ethnographers favor engaging in in-depth participant-observation. They critique qualitative researchers who use in-depth, life-history interviews as data rather than living among the people they represent. They use lengthy, verbatim transcriptions of naturally occurring conversations, often presenting them devoid of much framing. Yet, public ethnography generally lacks the kind of epistemological discussion, theoretical development, or conceptual organization of the classical, realist ethnographies that we see presented at this Qualitative Analysis Conference. Yet, it is *in vogue*, especially among Ivy League and other elite university ethnographers. Although public ethnographers use a methodology similar to our own and trace their roots to leaders in our field, they circulate in a more rarified ambit. Some of their conferences are by invitation only and fete participants with generous expense allowances. At this time, public ethnography is enjoying a zenith of prestige and influence, and is even attaining publication in some mainstream venues (see Goffman 2009).

POSTMODERN ETHNOGRAPHY

Interestingly, taking place within one week of this conference (in May 2011) is the Seventh International Congress of Qualitative Inquiry, in Urbana, IL. This collection of postmodern (or post-structural) ethnographers rejects the ideal of value-free (Weberian) inquiry based on a "God's eye view of reality," as dated. Instead, like feminists, they privilege politically based inquiry. They also espouse moving beyond the experimental, reflexive ways of writing first-person ethnographic texts to creating critical personal narratives of counter-hegemonic, decolonizing methodologies. They describe the field of qualitative research as defined primarily by a series of essential tensions, contradictions, and hesitations between competing definitions of the field. Some of this can be seen in the debate over the definition and ownership of the term "autoethnography" (see the JCE special issue on "Analytic Autoethnography," edited by Leon Anderson in 2006, especially Anderson 2006, and Ellis and Bochner 2006).

It is useful to compare the state of postmodern ethnography with the kind of classical ethnography practiced by participants at this meeting by contrasting the themes of the Denzin Congress of Qualitative Inquiry with the 28th Annual Qualitative Analysis Conference. The theme of the present conference, "Contemporary Issues in Qualitative Research," focuses on the application of qualitative methods and the debates surrounding qualitative inquiry. The stated goal is to explore new and enduring challenges to qualitative methodologies such as: research standards, the integration of technology, the role and influence of emotionality, the researcher's place in the field, ethical regulations and boundaries in the field, and team-based qualitative approaches. According to their program, the Congress's theme is the "Politics of Advocacy." Sessions take up critiques of value-free inquiry; issues of partisanship and bias; the politics of evidence; alternatives to evidence-based models; indigenous research ethics; and decolonizing inquiry. Contributors are invited to experiment with traditional and new methodologies and with new presentational formats such as ethno-dramas, performance, poetry, autoethnography, and just plain fiction (see Congress of Qualitative Inquiry 2011).

As a result, their program features multiple sessions on autoethnography (using their definition of the concept as the study of one's own self) including 11 autoethnographic sessions on such topics as: identity, resistance, and the academy; locating sites; gender; physician autoethnographies; the family; decolonizing; the arts; violence, the nation; joy; and three sessions on autoethnographic potpourri. Other sessions feature performance ethnography, ethno-dramas, fiction, stories, ethno-theater, playing cards, poetry, advocacy, indigenous research methods, writing, representation, and duoethnographies.

Postmodernism, born in a critique of both positivist and post-positivist sociology, casts realist ethnography as "merely modernist," practiced by field researchers who are politically naïve, chained to some "God's eye" fallacy, and inadequately evolved to recognize the true epistemological and representational callings. Both of these ethnographic "faces" have sprung from the foundation of classical ethnography. Yet, despite these differences, we would rather see a convergence of these approaches, with subfields and lines of inquiry all housed under one rubric. There has been an explosion of new qualitative/interpretive journals. This could be a good thing for our collective enterprise, giving us more outlets and fostering our prosperity. Let us focus our enterprise to widen our common ground, not narrow it.

CLASSICAL ETHNOGRAPHY

The classical genre stands as the original version of Chicago School ethnography, bending and swaying with ongoing movements in the subfield. Its mission has always been to "bring back the news," to rhetorically convince readers that it conveys an authentic and verifiable tale that has been gathered by people who left the ivory tower to enter the field, returning with accurate knowledge about the trends and patterns of the world from its everyday nature to its obscure and hidden nooks and crannies. It has the power to critique, to theorize, to edify, to surprise, to amuse, to annoy, or to comfort (Van Maanen 2011). This conference represents a site of classical ethnography.

Ethnography is the only method that allows us unfettered access to the lives of others. Although we certainly have issues with which to grapple, such as representation, authenticity, voice, ethics, and research bias, we continue to experiment with them. We do not shut the door to new, emergent, issues. Finding a "cutting edge" does not require eschewing the history or tradition whence we sprang. Let us continue to place ourselves where we belong, with our participants, with the people in their naturalistic settings, treading that thin line between the everyday life member and the analytical observer.

We should all realize that we live in a forest of diverse ethnographic work, and it benefits us to recognize the subtle nuances that distinguish these genres. There is no longer one standard model of ethnography. But, at the same time, as we innovate and forge new streams, let us still join hands and not overlook our common ground: that we are representing the lives of people we study based on participant-observation and in-depth interviewing, done longitudinally over years in the field, overlaid with the type of critical, theoretical analysis that only the sociological imagination can produce.

Let us stop widening and, rather, bridge the chasm we have with our brethren. Let us incorporate some of the creative insights and sound advice from each new offshoot. One thing that has always characterized classical ethnography is its malleable nature, as it evolves in response to the foment of new ideas, new approaches, new forms. We should continue to incorporate the best features of postmodernism, of public ethnography, of mainstream ethnography and meld them with the excellent work we are doing in our tradition, which we are teaching to new generations of ethnographic practitioners and scholars. We implore you to never wander too far from the field, to make sure you gain access and build rapport with the people you study, and to remain at an analytical distance at the same time as you make sure you get close enough to the data that you can honestly and accurately report them. Then we can get back to the work that our Chicago School forefathers and mothers asked us to do in the first place: to get our hands dirty in real research.

We close by saying, "Give me some of that good old time realist ethnography, it's good enough for me." Let us keep on doing what we do well: getting deep into our fieldwork; giving voice to the members' perspectives. We should continue to spend time using the contours of subjectivity to understand the sociological world and enhance it with the creativity of our conceptual and theoretical analysis. Let's encourage fieldworkers to go to the field rather than to reflect solely on texts of the field, so that they can become deeply ensconced in the life worlds of their participants. Give us ethnography where researchers know the everyday nuances of the people's lives they study, where real problems of conflict sometimes occur between the researcher and the researched that need to be ironed out, and where our empirical data go beyond portraying the individual experiences of the researcher to depicting the generic experiences of the group or subculture.

We urge you to stay with the program while at the same time getting on with the show. Change is necessary, but wholesale revolution may not be. What we have is not broken, but it may benefit from regular re-examination, creative innovation, experimentation, and fun. Let us continue to embrace what Dietz, Prus, and Shaffir (1994) have called "ethnography as human lived experience." As Robert Park admonished us, go live among these people, be kind to them, understand their worlds and the way they live in them. Use *verstehen* and produce analytically sophisticated documents for social scientists and intelligent lay people to read throughout the next millennium. As we see it, participants who come to this conference are on the right track. There will be detours along the way, but we believe that when you arrive at the station, ethnography will best be served by ensconcing yourselves in the world as completely and as humanely possible, and by extending knowledge in a way that can only be done by scholars who have held the hands and walked in the shoes of the people they study. GO FOR IT!

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PUSHING DADDY AWAY? A QUALITATIVE STUDY OF MATERNAL GATEKEEPING

Abstract There is a great deal of literature regarding the differences between motherhood and fatherhood. The literature suggests that parenthood is deeply gendered and takes on different meanings for men and for women. This paper examines parenthood from an angle not typically addressed in feminist discussion. Missing from recent scholarship is any in-depth examination of the role that women may play in limiting the involvement of their male partners. What part do women play in the maintenance of traditional parenting roles? This paper, based on a series of interviews with parents of young children as well as field research in child centered locations, concentrates on the reasons behind the emergence of gendered parenting approaches. The qualitative nature of this study makes it possible to note the complexity of this issue and examine the role of both mother and father in gatekeeping. Overall, I argue that many mothers are involved in maternal gatekeeping through taking control over both major and minor parenting decisions, through controlling access to parenting information and through implementing control mechanisms during their absence. I discuss the means by which some mothers attempt to limit their partners' involvement and the ambivalence that this creates for both parents involved.

Keywords Maternal Gatekeeping; Gender; Parenting; Paternal Involvement

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The home is clearly the newest battlefront in the struggle for gender equality. When it comes to parenting, however, it often seems that the more things change, the more they stay the same. The literature on gender and childcare is tediously consistent. Women continue to take on the lion's share of childcare duties and this finding remains constant despite women's participation in the paid workforce (Hochschild 1989, 1997; Shelton and John 1996; Coltrane 2000; Sayer 2004; Craig 2006; Poortman and van der Lippe 2009). Furthermore, men tend to view their roles as fathers as mediated by their wives. In fact, men tend to see children as an extension of their marriage and often it is their female partners who decide when to have children and in which way to raise them (Hochschild 1989; di Leonardo 2005; Lorber 2005; Townsend 2005). Even when men are given the opportunity to relocate their paid labor to the domestic realm and increase their childcare activities, this involvement is often viewed as extra and their wives and partners are called on to police these new boundaries (Halford 2006).

There is, of course, evidence of the increased accessibility and involvement of fathers in the lives of their children in the last several decades. There has been a steady increase in the time that men have been spending with their children as well as an increase in the practice of coparenting (Furstenberg 1988; Pleck 1997; Deutsch 1999; Yeung et al. 2001; Turner and Welch 2012). There have also been several notable works that advance gender-neutral parenting, calling for a degendering of this domain and promoting equality in the area of parenting (see, for instance, Kimball 1988; Lorber 2005; Mannino and Deutsch 2007). However, there still remains a large gap between fathers' levels of involvement and those of mothers. Once children are born, they are often viewed to be their mother's priority. Barrett and McIntosh (1982) point out that the fact that women can bear children as well as lactate often leads to the assumption that all childcare (and by extension, housework) must be carried out by mothers. While they term this, "the tyranny of motherhood" (Barrett and McIntosh 1982:61), other scholars reveal how motherhood becomes a romanticized identity, and mothers tend to feel "morally transformed" (McMahon 1995). Fathers, on the other hand, are often viewed as "helping out" or as "babysitters" when it comes to their own children. They often choose how and how much they will involve themselves with childcare, leaving the burden of responsibility of emotional and physical childcare on the shoulders of mothers (Barrett and McIntosh 1982; LaRossa 1997). Indeed, when a man is the primary care-giver to a child, it is deemed to be an extraordinary situation, sometimes even newsworthy (Lorber 2005). Many scholars point out the inequities inherent in the rigid allocation of traditional parenting roles, pointing out negative outcomes for women in terms of workload in the home and career limitations (see, for example, Williams 2000; Crittenden 2001; Hirshman 2006 among others).

Typically, societal structures, as well as gendered power relations, are held responsible for the endurance of traditional gendered parenting roles. Studies look, quite rightly, to broader social structures to explain the disparity. They concentrate on labor force characteristics, workplace policies, parental leave, relative earnings of men and women, et cetera (see, for instance, Deutsch, Lussier and Servis 1993; Deutsch 1999; Cowan and Cowan 2000; Brandth and Kvande 2009 among others). Other studies explore gen-

der ideologies as the roots of this problem (see Deutsch et al. 1993; Bulanda 2004; Gaunt 2006). Lastly, many studies look to couples' conception of gender roles as well as their everyday negotiations concerning family and work to explain why men are less involved in parenting than women (see Gerson 1985; Thompson 1991; Hochschild 1997 among others).

Missing: A Deeper Analysis of Gendered Parenting Patterns

Many feminist scholars call on families to begin defining motherhood and fatherhood in the same fashion and, thus, begin the process of degendering parenting (Hooks 1984; Lorber 2005; Wharton 2005). What is missing from recent scholarship is any in-depth examination of the role that women may play in limiting the involvement of their male partners. As a gender scholar I have read study after study explaining what men may have to gain from the unequal distribution of parenting labor in the home. There is, however, a growing body of literature on maternal gatekeeping and on what women may achieve by this practice that is deserving of attention. This study is situated in this growing area of research.

I examine parenthood from an angle not typically addressed in feminist discussion, challenging the assumption that traditional gendered parenting roles are being perpetuated by, and for the overall benefit of fathers. Instead, I address the somewhat controversial question of what both women and men stand to gain or to lose by maintaining traditional parenting roles. While providing fresh insight into the roles that women play in sustaining traditional family gender roles, I call into question ideas of where the knowledge of gender inequality is situated and challenge the perception that women respondents hold the only set of keys to unlocking inquiry on this matter.

The study presented in this paper expands the research in the area of maternal gatekeeping in several ways. To this point, the majority of research carried out in this area has been conducted through the use of survey questionnaires and has been quantitative in nature. This study provides a deeper, more detailed examination and is one of few qualitative studies on maternal gatekeeping to be conducted (other qualitative studies have focused on maternal gatekeeping in families that have experienced parental separation or divorce – see, for instance, Sano, Richards and Zvonkovic 2008; Trinder 2008). It is also one of few studies that does not focus on the psychological characteristics of mothers involved in gatekeeping, but, rather, explores the gatekeeping methods employed by mothers as well as their motivations. Moreover, it is one of the only studies to give voice on this issue to fathers, outlining how they feel when gatekeeping takes place as well as the gains and losses that it provides for men. While previous studies on maternal gatekeeping do not explain the causal direction of this activity making it unclear as to whether gatekeeping is a product of low paternal involvement in parenting or a source of it, this study argues that it is a bit of both. The qualitative nature of this study makes it possible to note the complexity of this issue and examine the role of both mother and father in gatekeeping.

The findings of this study do not challenge those established in previous research. Rather, they build upon and intensify existing research findings through a thicker and more detailed analysis. Overall, I argue that many mothers are involved in maternal gatekeeping through taking control over both major and minor parenting decisions, through controlling access to parenting information and through implementing control mechanisms during their absence. I discuss the means by which some mothers attempt to limit their partners' involvement and the ambivalence that this creates for both parents involved.

THEORETICAL BACKGROUND: MATERNAL GATEKEEPING

Evidence suggests that women generally value the involvement of fathers in their children's upbringing (Pleck 1985; Thompson 1991). Indeed, the positive impact on both children and on the parental unit achieved by paternal involvement with children has been well documented (see Hochschild 1989; Coltrane 1996; Glass 1998; Pleck and Masciadrelli 2004). Still, many women limit the involvement of their children's fathers through behavior that has been termed "maternal gatekeeping." Allen and Hawkins define maternal gatekeeping as, "a collection of beliefs and behaviors that ultimately inhibit a collaborative effort between men and women in family work by limiting men's opportunities for learning and growing through caring for home and children" (1999:200). While clarifying that maternal gatekeeping is not the primary barrier to father's involvement in childrearing, they suggest that this may be a factor and that over twenty percent of mothers engage in this behavior. Allen and Hawkins (1999) define this behavior as having three dimensions: mother's reluctance to surrender responsibility for family matters by being rigid in their standards, mother's receiving external validation of their identities as mothers, and differentiated conceptions of parental roles.

Cannon, Schoppe-Sullivan, Mangelsdorf, Brown and Szewczyk-Sokolowski (2008) posit that maternal gatekeeping may be either facilitative (encouraging paternal involvement and creating opportunities for fathers to gain experience) or inhibitory (behaviors meant to regulate father involvement such as criticizing the father's behavior). Typically, however, the existing research concentrates on this second category, pointing out that gatekeeping limits fathers' opportunities to develop relevant parenting skills and to experience childcare (Allen and Hawkins 1999; Fagan and Barnett 2003; McBride et al. 2005).

Many studies demonstrate the relationship between mothers' beliefs concerning the role of fathers and their gender role identities and gatekeeping (Fagan and Barnett 2003; McBride et al. 2005; Cannon et al. 2008; Gaunt 2008; Schoppe-Sullivan et al. 2008; Kulik and Tsoref 2010). McBride, Brown, Bost, Shin, Vaughn and Korth (2005) found that mothers' feelings concerning the roles of fathers played a role in moderating

the relationship between fathers' perceived investments in parenting and their actual levels of involvement. Fagan and Barnett (2003) operationalized maternal gatekeeping behaviorally, examining the relationships between women's reports of gatekeeping, their perceptions of their children fathers' competence, attitudes concerning the role of fathers and actual father involvement. They found that mothers who placed greater value on the role of fathers also reported that their children's fathers were more involved. Moreover, mothers who viewed their children's fathers as competent parents were less likely to gatekeep. Overall, research points to feelings of ambivalence on the part of mothers towards fathers' increased involvement in parenting and suggests that mothers may, in fact, be unaware that they are engaging in this practice (Gaunt 2008).

Doucet (2009) points out that when women make space for fathers to enter the parenting realm, fathers come to take on responsibility for children both in terms of emotion and community. Doucet (2006) questions the constant comparison of women and men in terms of parenting skills and urges scholars and policy makers alike to note the unique abilities and parenting approaches that fathers bring to their families. She points out that much of the retention of traditional gendered parenting roles stems from the marginalization that fathers often feel in female dominated early childhood settings, such as parenting groups, and notes that women and men experience different pressures when displaying childcare in community settings (Doucet 2006, 2009, 2011).

While not discussing gatekeeping *per se*, Townsend's (2002, 2005) work on fatherhood and the mediating role of women also sheds light on gatekeeping activity. Townsend describes how the men that he studied viewed, "«marriage and children» as elements of a «package deal» which cannot be easily separated" (2005:105). He argues that women are often the decision makers when it comes to having children and that women take on the roles of "default parents." Furthermore, he argues that women play the role of mediator when it comes to fathers' involvement, outlining the conditions that surround fathering behavior. Townsend (2002) argues that men's mediated roles are a result of paid employment and that it is their identity as family providers that is used to express closeness to their children. Indeed, the emphasis placed on provision limits men's time within the home and, thus, men's roles come to be mediated by their wives.

RESEARCH QUESTIONS

While the available research on maternal gatekeeping is captivating, it leaves gender scholars hungering for expanded narrative. What, for instance, does maternal gatekeeping look like? How does this behavior take shape? How do fathers feel about gatekeeping and how do they react to it? Moreover, what do both women and men have to gain from gatekeeping behavior and, more importantly, what do they have to lose?

My research adds to the existing literature on parenting by asking what part women may play in the maintenance of traditional parenting roles. There is a growing discussion, in a society that is moving towards gender neutrality, as to why gender takes such a strong role in shaping parenting. My work contributes to this dialogue, adding to the puzzle, the piece fashioned by the role of mothers.

This study treads uncomfortable waters. The very notion that women engage in gatekeeping activity is, itself, controversial as it removes this behavior from the broader context of the patriarchal family structure. Furthermore, discussion of gatekeeping seems to underestimate the strength of fathers' decisions concerning their own parenting behavior and almost seems to suggest that the gates to paternal involvement are not merely closed, but bolted shut by mothers (see Walker and McGraw [2000] for an excellent example of such criticism). In light of this, it is important to clarify that my research findings are not meant to weaken broader arguments concerning the roles of societal institutions and structures, especially the paid labor market and the male-dominated family structure. It does, however, suggest that mothers sometimes contribute to inequalities in the area of parenting and that women may, indeed, have something to gain from this practice.

METHODOLOGICAL APPROACH

The bulk of my data was obtained from a series of forty semi-structured interviews with respondents who have young children living in their household. In all cases the mother and father lived together and all but three couples were married. In most cases, the mother and the father were interviewed separately so that they would feel more comfortable speaking freely about their parenting relationship. Each interview was based on a series of prepared questions pertaining to the respondent's parenting experience. Thus, I explored the same group of central themes with respondents while allowing for flexibility in terms of probing any comments that seem especially interesting. Interviews lasted from between forty-five minutes to over two hours and were transcribed verbatim.

While approximately one quarter of my interviews were conducted in the state of Indiana and several interviews took place in Ohio, the bulk of my data was collected throughout the state of Wisconsin. My findings, then, are typical of the American Midwest. More specifically, research was conducted in an area of the Midwest known for having a high standard of living, high safety ratings, and as being a "good place to raise children." The median age of the research area is in the mid thirties and average household income rests just above sixty thousand dollars. Approximately eighty percent of the population in the area is Caucasian and this was evident in my sample, which was also approximately eighty percent Caucasian. For a list of respondents and their biographical data, see Appendix A. All names used in this article are pseudonyms. [Pseudonyms were chosen with care as to reflect the ethnic origin of respondents' real names. Agbenyaga, for instance, was chosen to reflect the respondent's real name, which is West African in origin while Hilda

was chosen to reflect a name that is Swedish in its origin.]

Using the methodological framework of grounded theory, I was able to generate theory from the data throughout the research process. This approach is valuable as it allows researchers to let their data dictate their findings and, thus, also uncovers what aspects of the research are most significant to their research subjects. It allows respondents to inform, and the researcher to convey the local meaning that respondents create in a situation. This method was especially useful for this study as grounded theory is both detailed and rigorous, yet also permits the flexibility and freedom required to gain new perspectives on common situations. That is, grounded theory allows for layers of analysis that break down assumptions (in this case, the assumption that all patriarchal structures work in a similar fashion) and allow for greater diversity in findings, especially when investigating multifaceted social phenomena (for a more detailed account of grounded theory see Glaser and Strauss 1967; Strauss and Corbin 1990; Charmaz 2006; Bryant 2007 among others). Using open ended questioning was particularly constructive for my research as it facilitated the kind of flexibility that allowed my respondents to concentrate on the aspects of parenting that they found most meaningful, thus giving my subjects voice in my research. Often, respondents' comments led me to add extra questions in subsequent interviews, often taking me down unexpected paths. For instance, I did not initially ask parents about having travelled with and without their children and the planning that often accompanied these trips. Responses from early respondents caused me to add these questions, which proved to hold an abundance of data relevant to the topic of gatekeeping.

I used a snowball sampling technique to carry out this research. This technique consists of gathering informants who, after the interview, refer you to other informants. Qualitative research of this nature typically consists of a number of small snowball groups. Several interviews were carried out by research assistants under my supervision. Initial respondents were obtained while observing parenting *in situ* at places where parents and children can be found, including parks, child-themed cafes, libraries and restaurants. I would simply approach strangers in these settings, introduce myself, obtain their phone numbers, and then set up an appointment for an interview at a later date. While this sampling technique was non-random and purposive, it allowed me to explore the narratives of my respondents in rich detail. In general, this approach yielded a relatively diverse sample, consisting of several different religious, ethnic, and socioeconomic groups. The sample also consisted of families that were formed by adoption and step-parenting. It did not, unfortunately, result in a sample that was diverse in terms of sexual orientation and this is a limitation of my study.

Typically, parents were very eager to speak about their parenting roles and rejections were few and far between. On several occasions, women respondents explained that they would be happy to be interviewed, but that their husbands or live-in boyfriends were very busy and would most likely refuse. Indeed, this was the case with two respondents who were interviewed without their partners included in the sample. However, it was more often the case that, once the male partner was asked, he agreed to the interview and often spent longer speaking than his female counterpart. In general, respondents mentioned that they felt special to be included in a study of an academic nature as, for them, this was an exceptional experience.

Once complete, my data was openly coded for analysis using codes such as: BP (baby preparation), U@N (up at night), BT (bedtime), PT (play time), G (gain), R (rigidity), COD (control over decision), COI (control over information), as well as others. I then organized relationships between the codes used, combining them into common themes. Thus, concepts became categories of analysis. Once this conceptualizing was complete, I began my second coding (axial coding) of the material, recoding the data according to the properties and dimensions of the categories and subcategories that I had formed. I completed my data analysis with a third, selective coding, creating substantive theory from the categories of axial codes. At this point in analysis the core narrative of my research emerged and I began answering the broader questions of how women limit their male partner's parenting, how men respond to this and what women have to gain from maternal gatekeeping.

FINDINGS

In the following sections I outline how a number of mothers partake in maternal gatekeeping through asserting control over both major and minor parenting decisions. Gatekeeping behavior also takes shape through the assertion of control over parenting information and by controlling the home environment even during absence. Several of my respondents engaged in archetypal gatekeeping behavior by limiting the involvement of their male partners. These behaviors are fraught with both gains and losses for both mothers and fathers and, thus, create a sense of ambivalence in parents as they simultaneously accept and reject gatekeeping activity.

Control over Decisions

Maternal gatekeeping is, in itself, an instrument of control that women sometimes use to assert influence in the domestic domain. Many of my women respondents explained to me that they prefer to have control over the majority of parenting decisions. These decisions varied from minor decisions such as what color to paint the nursery, which baby furniture to buy and which hobbies their children would adopt to more major decisions such as what day care their child would attend, what their children's diet should consist of and what type of parenting style would be adopted. Thus, mothers actively managed their children's upbringing and, by extension, their male partners' parenting styles and choices. Women often addressed this taking control over decisions outright. Victoria, a professor with two young children who refers to herself as a "control person," explained:

> I plan everything. I do all the doctors' appointments. I schedule haircuts. I schedule their pictures. You know, I pay the sitters. I pay the pre-

> school. I make the schedule for year...I'm kind of managing their lives a lot more than he probably does... I think, it's not even a, "Oh, my husband doesn't do anything," it's just a, "This is just what I did"...those are just the things that I chose to take over.

It is interesting to note that Victoria goes out of her way to explain that this "taking over" was her doing and not necessarily a result of her husband's reluctance to fill this role. Sharon, a mother of two, echoes this sentiment when speaking about making preparations for a family trip, explaining: "Yeah, I do all the work...I've never even thought of letting Tod do it or even asking him to do it." Indeed, many women limited their partners' involvement with their children by taking on tasks and making decisions as if their partner's roles as parents were nonexistent. That is, decisions were made without soliciting input from fathers whatsoever. While most fathers did not actively fight their partners' managing decisions, they often expressed regret for having conceded control over this domain. For instance, Mathew, a currently unemployed stay-at-home father, during a discussion over potty training decisions, is asked: "How did you come up with that idea?" His answer: "She did. I won't ever do that again."

Often control over decisions was taken more forcefully. Mathew reports being, "yelled at" when he makes decisions that differ from his partner's. Amber, a graduate student with a toddler at home, does not go so far as to yell when her boyfriend disagrees with her, but tells me that her boyfriend often jokes that she's "the boss" and explains that she often controls both insignificant elements of her family's life (such as "dragging" her boyfriend to have a family picture taken) as well as several important elements. For instance, her boyfriend did not want their son to attend daycare; he preferred him to stay at home with him. Amber, however, made clear during her interview that she did not want her boyfriend to take on the role of a stay-at-home father. When speaking about the daycare that her son used to attend she says: "but he [her boyfriend] was not fond of the daycare; it was more like I forced him to be okay with it..." Her boyfriend, Hector, admits to giving in when a disagreement over their son arises, "I won't say nothing even though I probably won't agree..." In fact, when I ask how they come to a decision over how to do things, he interrupts the question and inserts, "she's right... When she wants me to do it a certain way, then I have to do it a certain way."

Many women expressed their control more subtly, by answering questions concerning childcare decisions with the word "I" (as in "I wanted," "the approach I want to take" or "I picked," etc.). This was often mirrored by their male partners' statements (such as: "we painted the room the colors that she wanted," "she was really picky about..."). Sometimes mothers were less subtle in their control over decisions. Wendy, a mother of two young children, expressed: "But, I think my role falls into the decision making and his role falls into 'I do what she says'..." In a more extreme case, Mathew explained how decisions concerning his son's clothing are made:

> M: Um, today Maria picked his clothes out. She doesn't like how I dress him...sometimes I just put some clothes on him in the morning and then Maria goes, "Hey, I don't want that on my son!"
> Interviewer: What happens then?
> M: She takes off his clothes and puts something else on.

It makes a great deal of sense that women would attempt to control elements of the domestic realm. Increased paternal involvement in decision making intrudes on an area that has historically been a source of power for women. Moreover, regardless of actual distribution of work in the home, women continue to be viewed as those responsible for care of and decisions surrounding their children. This places a great deal of pressure on women who often feel judged by the condition of their homes and children, a judgment from which their male partners seem to be exempt. Women in my sample were very much aware of this. Mary, a mother of two children under the age of two, makes comments that express this connection well when she blends her identity of being a caretaker to her children and husband with the impression that she feels that she makes when her home is not perfect:

> Like, I love being a wife and caretaker and people come to my house and it's messy and I feel that reflects on me, it doesn't reflect on Steve. And so I put pressure on myself that way.

Because women are so often associated with childcare and housework, failure to make a good impression in these areas does not simply reflect on mothers as individuals, but has a large impact on their sense of self-worth as women. This connection between feminine identity and childcare may provide insight as to why many women feel that the weight of childcare responsibility is theirs alone. Indeed, many of my women respondents seemed to be thrown into the role of decision maker during pregnancy (with registering for baby showers, which for all of my respondents that had them, centered around the mother) and even before pregnancy. The largest, and most powerful decision to be made surrounding children is whether or not to have any. This was not lost on my respondents, many of whom made it clear that this was an area in which they would have the final say:

> [When asked about having more children] No! No, it is not in my plans. Apparently it is in Jim's and my daughter's plans... (Wendy)

[It is interesting to note here that her husband's decision making power in this area is equated with that of her daughter.]

> Then the problem came into how many and I only wanted two more tops and he could go forever, he could have a dozen and he'd be happy... (Tina)

> And, I've always wanted one and Tod's always not. And finally it came, I want one, he doesn't. I wanted it more. (Sharon)

> She stopped taking her birth control is what it was. I mean, it wasn't something we decided on or anything. I don't know why she did it. (Tod – talking about his first wife, not Sharon)

> She [wants more]. I just do whatever she says, but otherwise I'm OK with just the one, but she wants more. (Hector)

> [He did not want more children, but] I was on my own plan, I got pregnant. (Kaitlin – met during field research)

> I told Maria – I said, "I want like 15 more." She like, "It ain't going to happen." If I could, I would. (Mathew)

It is both noteworthy as well as heartening to see that the women in my study had a strong sense of control when it came to decisions concerning their own bodies, a human rights issue that women have been tackling for years. Nevertheless, controlling major decisions surrounding pregnancy discounts the feelings of their partners. This flows over even into cases where the pregnancy is unplanned as women seem to control the "feeling environment" concerning the pregnancy. In the case of John and Tanya, for example, Tanya reported that she had been "freaking out" about her unplanned pregnancy, while John states:

> ...I was thrilled, but she didn't know it (laughs) until it was OK for her to be happy about it... I knew better than to go: "Oh, this is a good thing" when she's freaking out.

It seems clear that feelings and decisions surrounding pregnancy fall into the women's domain. It is not surprising then that feelings and decisions surrounding childcare would also come to be seen as the women's domain. Indeed, in most cases, the respondents who reported making solo decisions concerning pregnancy, were more likely to control other decisions concerning their children's upbringing as well.

Control Over Information

Linked to control over parenting decisions, many of my respondent mothers reported that they took steps to control the information that their partners received concerning their child's routine, health and care. This control often began as early as pregnancy – as women began to gather information on their changing bodies. Far more women respondents read books on pregnancy than male respondents and when both parents were engaged in research, the mother often did more reading. Several women reported selectively sharing information with the father, telling him only the parts that they felt would interest him or that he should find important. Maria, for instance, told me: "mostly, I just did all the reading and dictated to him [a filtered version of] what I read." This pre-parent information was not limited to couples that conceived naturally. When Reuben and Katie decided to adopt children (after having had two children through pregnancy), it was Katie who read and researched on both the adoption procedure and the issues concerning raising children who have been adopted. She then shared with Reuben only the parts that she felt he needed to know.

It is important to note that controlling information is not something that is done purposefully by mothers, but, rather, is an extension of controlling decisions and speaks to a lack of communication between parents. That is, women in my sample reported that they did not intentionally hide information from their children's fathers, but they did choose not to share information with them for various reasons including a belief that holding this information was part of a mother's realm, a belief that this information should come naturally to a parent, or simply the source of pride that holding limited information awards. Victoria articulates this important point well when she reports:

> I would leave the house and he would call me, "What does this kid want? Why is he crying? Where is his food? What do you feed him?" And I was like, "Seriously, you don't know what he eats?" Well he probably wouldn't because I just do it without explaining what I'm doing...

Victoria later adds that she feels that her husband "would be completely lost if he had to figure out" all of the things that her children need to have done for them. At the same time, the above quote illustrates how she understands that she has never shared this information with him. Controlling his access to information, then, creates in her husband a sense of dependence on her when it comes to carrying out basic parenting tasks. This is very much in line with Allen and Hawkins definition of maternal gatekeeping as, "a collection of beliefs and behaviors that ultimately inhibit a collaborative effort between men and women in family work by limiting men's opportunities for learning and growing through caring for home and children" (1999:200).

Sharon makes similar statements to Victoria when she discussed Tod's role in choosing a daycare:

> And I think it's probably not that he doesn't care, it's probably that he's never went and seen it to know that there's a difference... I'm guessing he's probably just thinking, "You know, a daycare's a daycare."

In general, women reported that they were more in tune with their children's needs than their partners and that they were more on top of their children's medical information because they were the ones to take their children to the doctor. This was often a result of simply being the primary parent and is not surprising in light of the findings of most parenting research. However, women's reluctance to take the extra step of sharing information with their partners is puzzling. In doing so, they set themselves up as the only people capable of administering medicines and taking care of their children's health care needs. This, of course, awards mothers a great deal of power and a real sense of indispensability. At the same time, women often reported feeling ambivalent about their roles as information custodians. On one hand, they gained pride from having exclusive knowledge and, yet, they felt overwhelmed by the responsibility that this entails. This will be discussed further in subsequent sections.

Control When Away

Surprisingly, maternal gatekeeping is not limited to periods when mothers are at home. Many mothers in my sample explained that

when they went away overnight they would leave ready made meals and ensure that there were enough clothes, diapers, et cetera for their children. Agbenyaga, a homemaker with a two year old at home, explained that before leaving overnight she would make, "sure her [daughter's] laundries were made (sic!) and...[go] grocery shopping for her food, drinks. Even though, daddy is around I still make sure that she has enough of that, and diapers..." Mary, a mother of two, explains how she prepared for an overnight trip by planning meals and providing instructions for her husband:

> I planned two meals that they – that he could make. I went grocery shopping and got all the ingredients and set the recipe cards out for him... normally I would have made it, but I decided to go last minute, I think. So, I didn't have time to make it... I knew they'd have a great time. I did worry if she – I made a comment to him like, "If she pees by her diaper, change the sheets. Don't just be like 'it will be dry by nap time'..."

While research on maternal gatekeeping suggests that mothers who engage in the practice are more likely to have little faith in their partner's parenting ability, both Mary and Agbenyaga expressed that they had a great deal of trust in their husbands. The extra planning work that they take upon themselves before leaving seems to be more about control and, also, simply being in the habit of doing this work. On the other hand, many women (including Agbenyaga) reported that when they left for an overnight trip, they would leave their children with their mothers or their mothers-in-law, thereby placing their children under the supervision of another woman rather than leaving them with their fathers. This suggests that women's faith in their partner's parenting abilities may be more limited than they are willing to admit and that they feel more secure having another woman carry out the tasks that society has allocated as a female domain.

Most surprising, was the discovery that many of my women respondents who worked outside the home and had an opportunity to leave their children with their partners (who had a day off from their jobs, had flexible work schedules that allowed them to be home with their children or who were unemployed and acting as primary caregivers) would use the morning hours or the night before as a means of controlling the time their partners spent during the day alone with their children. That is, they would spend time preparing for their partner's day. The most extreme example of this can be found in the case of Ashley who would spend half an hour every morning getting the house ready for her partner Robert. During this time she would prepare sippy cups for her son, filling them with milk and placing them in the fridge, prepare food for her son that day, turn on the TV for them – ready for when they wake up, go through the house and place specific toys in places where she knew her son would find them and play with them, et cetera. She would then let Robert know that he was "on duty" and leave for work. Amazingly, Robert seemed, during his interview, to be completely oblivious to all of the work that Ashley carried out. Moreover, Ashley did not seem to recognize that she was micro-managing her partner's day by doing work that is typically considered the responsibility of a child's immediate care-taker.

While Ashley's example may be excessive, less extreme examples of this managing a partner's parenting during absence abound. Both men and women reported that mothers would often engage in preparations for childcare the night before, readying clothes and preparing food. Several women reported making requests or lists for their partners to carry out with their children such as specifying the activities that fathers were to engage in with their children for the day. For instance, women would often arrange fathers' schedules with their children, detailing how much time should be spent outdoors, how much time spent watching television, et cetera. Often women would stipulate how certain parenting tasks should be carried out during their absence. Explicit instructions were often given surrounding what to feed children and how to put them to bed (or, often, what methods of putting them to bed were unacceptable). Some mothers even indicated what toys were to be played with (with emphasis typically placed on educational toys). Diaper bags provide an especially significant example. Several men reported that they had never filled a diaper bag. Hector, a stay-at-home father with one child, explained that his child's diaper bag is always full and ready to go. Indeed, he jokingly referred to this as well as other housework that his partner would do, as part of his "magic stuff," which always stays clean and ready for use.

It is difficult to access the reasoning behind mothers carrying out the work of managing childcare during their absence. While they report having trust in the fathers of their children, they make it clear that they are not always willing to hand over complete control of this domain. One possible explanation rests on the fact that leaving children with fathers compromises the power that women hold in the family. After all, if fathers can do alright without mothers' help, what distinctive contributions to family life can mothers lay claim to? This is an important question as, all too often, the power that awaits women who work or volunteer outside the home does not offer an adequate power alternative. What is clear is that the work of maintaining parental influence when absent takes time and effort, a fact which many of my respondents found problematic. This, again, speaks to the ambivalence that women feel over maternal gatekeeping behavior.

Limiting Paternal Involvement

The most basic form of maternal gatekeeping, limiting fathers' involvement, took a central role for some of the couples in my sample. For some women this behavior was not understated, nor was it articulated subtly during their interviews. Sharon, for instance, expressed early in the interview that she functions as a "single parent" and that Tod only "fills in" when he can. Tod's comments mirror Sharon's, yet illuminate his feeling that he has very little control over the amount of his involvement with his children:

> I never had to do anything. I didn't have a choice. It wasn't that I didn't want to do the stuff, it was that she got up in the morning, got Anita clothed and took her to her mother's or grandpa's and that was just the way it was. If I actually said, "Well, I really don't have a lot going on today, leave her home with me, we'll go to the parks and play" she'd say, "No, she's going to grandma's and grandpa's." And that's the way it was and then, finally, I was like, "OK, I won't even ask." So, then it was like, "Do what you want with her, I guess I've got no say."

David, tells a similar story:

> It was really, actually weird because I had a lot of experience with children so I did everything – well, not everything, but I did a lot of stuff and tried to help her, but then, after a couple of weeks, it was opposite. She did everything.

When asked to explain how this division of parenting labor came about, David responds with confusion, at first saying, "She was – I don't know" and then going back and forth between talking about his prior experience with children, her ability to lactate, and his work commitments. His wife, Hilda, interviewed earlier, mentions that David was once a better caretaker than her, but quickly explains that she has taken over this domain due to women's maternal advantage. In fact, while David reported that they were both equally good at parenting tasks, Hilda reported: "I just feel that I'm almost better at everything." When David is asked for a second time how the "switch" in roles came about, David continues talking in circles and then finally tells me:

> To me, I felt that she was really nervous when I brought him home, which is my own perspective, and I just kind of helped her out – showed her the diapers, cleaning the umbilical cord and stuff like that. And I might be blowing it out of proportion because I'm a guy and that's what we do, but it just seemed to – I was doing a little bit more and, then, now not as much. It has kind of reversed... And, it's not because I don't want to help her, it's just, you know, trying to find that medium so I can concentrate on other things...but, yet still be part of the family and enjoy things.

It is remarkable that David simply cannot explain why he went from principal caregiver to secondary caregiver. The manner in which he ends his explanation, however, is telling. One must wonder why he feels that he needs to find a way to be part of the family now, when at the time of the baby's arrival, this was not a concern. Indeed, the mothers in my sample who employed maternal gatekeeping, whether obvious or subtle, did not seem to understand that their partners felt limited by these behaviors and, often, felt pushed out of their fathering roles. Many couples told me about times that the mother had to leave the house for one reason or another feeling that the father would have a hard time alone with their child. This sense of indispensability was shattered, however, when they would come home to find that the father did quite well on his own. Fathers were quick to communicate the feelings of joy that they had concerning these times alone with their children. For example, Jerry, the father of a one year old, told me that he had some trouble connecting with his daughter when

his wife was home as the baby always went to her mother for things first, but, "when mom's not around, she's my girl!" Interestingly, Ruth, a nurse and mother of two, explained to me how she resisted the temptation to gatekeep so as not to create a family environment in which her spouse would feel like a secondary parent. She told me:

> I think with my first I was a bit, "It's my way, it's my way." But, then you realize that if you do that, you kind of push the other [parent] to the side... It's always you making sure everything's done and...once you let that go you realize, "Oh, he's fine."

In general, women respondents, as well as those met during field research, tended to make comments concerning their partners' inattentiveness to their children's needs. One woman, for instance, told me that she never left her child alone with her husband because he "doesn't hear" the baby monitor. However, not all of the couples in my sample that were involved with limiting fathers' involvement were concerned with safety/needs issues. Rather, the limitation seemed to stem from differing parenting styles. Hector's comments provide an especially good example:

> ...if I had a choice, I'd rather be with him then not with him on the days that she's gone. I tell her all the time, "You can leave whenever you want – go vacation with your mom somewhere..." She says, "No, I don't want to, you just want to keep Juan," you know, and she's like, "You play with him too much..."

In general, the men in my sample tended to take a more "laid back" approach to parenting (such as we see with Hector who preferred to play with his toddler instead of teaching him things). This makes sense when we consider that men do not place a great deal of their identity on their roles as fathers and may not be attempting to be "superdads." At the same time, I found that the men in my sample were more lenient concerning things such as bed-time (letting their children fall asleep on them rather than in their beds), meal time (for instance, not insisting on only organic food or being less strict with meal timing and nutrition), and play time (allowing their children to take more risks, to play for longer periods, and to watch more television than mothers would like). These things were often viewed by mothers as lesser parenting practices and in need of limitation whereas fathers did not view things in this manner. They simply explained to me that their parenting priorities lay elsewhere.

The most obvious limiting of paternal involvement stems from the choices made by couples concerning men carrying out more work outside the home. While it would be absurd to downplay the role of sexism and economic inequality in the paid labor market as driving forces behind such family decisions, the comments that several of my respondents made about their thoughts on these roles are telling. When asked about the possibility of "switching roles" many women responded that their husbands would be fine with this, but that they would not be. Klara, for instance, mentions economic factors

as weighing in her decision to be the stay-at-home parent, but quickly also clarifies:

> Oh, I think he'd be willing to...but it was always my dream to stay home with the kids...my dream that I would be there for the first steps or the first word, first wave, first smile, you know?

Several husbands' comments mirror these sentiments. Adam, for example, when asked how the decision was made for Ruth to stay home, answers: "Well, she wanted to. And, I...made more money than her…but mainly she really desired to do that so..." Maria, whose husband is currently unemployed and on disability, told me:

> ...I don't have as much time [with my son] cause he wants to be with his daddy. I don't know – I would prefer him to go back to work, that'd be great. I just miss being home with my kid without him.

While Maria shares that she'd like her husband to return to work so that she can spend more alone time with her child, the desire for fathers to curb their at home time with their children was not limited to women who worked only within the home. Ashley, for instance, states that she'd prefer Robert not to stay home with her son because daycare would be good in developing her son's social skills. At the same time she explains that if she could stay home, she would. Amber does not wish to stay home with her son, but did insist on her son going to daycare (until they could no longer afford it). She tells me outright that she does not want her boyfriend to be a full-time homemaker while clarifying: "my boyfriend wishes that he could just stay home and take care of him..." Sharon, as well, is very straightforward in her explanation of how she and Tod chose their work roles:

> He says that all the time, "You go to work and make the money and then I'll do the…"...And I'm just like, "No, no, how would I ever...that's why you make the big money and I don't, because when I want to just leave for a day, I can. If it's nice out and I want to go pick my kids up, I can. If she's got a Christmas concert, you know, and it's during the day, yep, I have to leave [work] at noon today." OK. So, it's like my trade-off.

Sharon, thus, limits her partner's involvement with her children in order to afford herself a more flexible work schedule, assuring that she will never need to limit her own involvement with her children. This model, which limits fatherhood in order to allow women to pursue involved motherhood is by no means novel. Indeed, it is rooted in the long standing cult of domesticity. My respondents simply modify the model, sewing in patches of female paid labor to give a modern look to a notion that is no longer fashionable as is.

Gain, Loss and Ambivalence

Both mothers and fathers involved in maternal gatekeeping seemed to simultaneously accept and reject this practice and, in fact, tended to feel a sense of ambivalence towards it that suggested that it provides both gains and losses. As previously mentioned, the research on maternal gatekeeping does not clarify the causal direction of this activity. Is gatekeeping the source of lower paternal involvement, or is it, actually, a product of fathers' low involvement to begin with? My research suggests that the causality flows both ways, and that gatekeeping is both the source of and the product of low levels of paternal involvement at the same time. In order to better understand this assertion, we must first explore what both women and men have to gain and to lose from maternal gatekeeping.

Gain

Among the positive features of maternal gatekeeping for women is the sense of indispensability that it creates. This was often suggested by some of the first comments that women made during interviews. For example, when I asked Sharon if she minded being interviewed she responded by telling me, "I run the show." Ashley explained to me that after the birth of their child she was scared because she didn't know how Robert, "was going to make it through the month" and added that she'd, "never seen him so stressed out before." This is noteworthy considering that Robert reported that it, "wasn't as difficult as...[he] thought it would be" and that the first month after the baby's arrival went, "amazingly smoothly." Women often reported that their male partners were simply not as good at parenting as they were and men often reiterated this view. Often, respondents would link this to the young ages of their children telling me that the child simply needs his/her mother more at this point yet they failed to provide evidence to back up this contention.

Some women indicated that they gained a sense of domestic power through gatekeeping behavior and many attempted to rationalize this by suggesting that they simply had controlling personalities. Maria, for instance, told me that she had a, "dominating personality," and then continued to explain that she desired to keep the current division of parenting labor as it is because her son may not need her as much in the future. She explains how she feels when her son chooses her over his father: "part of my heart breaks when he says, 'I want my mommy' and the other part of me is going, 'Ha ha!'" Sharon's comments are quite similar as she explains that she cannot bring herself to give up the power that being the primary parent brings in terms of her maternal identity:

> I love being a mom. So, I don't mind doing it all. Like doctor's appointments, I couldn't imagine not going. I couldn't, "You take her and you tell me what they said"...It's just the control issue in me.

For some women, the power gained from being the essential parent compensated for power lost after childbirth. This was most evident in respondents who cut down on hours or quit their jobs in the paid work force. However, it sometimes extended to other realms of women's identity. Samantha, for instance, shared that her husband was distant and unhappy after her son was born and seemed upset that they could not have sex for a long period of time after the birth (which involved complications). She seemed to use her indispensability as the care-giver of his child (and the child's major food

source) as a way of restoring the security that she once felt in her marriage.

Loss

Maternal gatekeeping also seems to be fraught with loss. The most significant loss for mothers was undoubtedly the loss of time and energy that carrying out the majority of parenting work entails. Every mother respondent who limited her partner's involvement (even if only to a small degree) expressed a sincere desire for help with childcare and a feeling of being overwhelmed with parenting responsibilities. At the same time, they were keenly aware of the contradiction between their chosen parenting roles and their feelings of exhaustion. Respondents expressed this incongruity themselves:

> So, it's not always balanced in a way where I would like, but I do feel that, um, you know, at the same time I have the role that I want. (Wendy)

> I would like to say that I'm not gender biased... I would like to say that, but...I chose to be, you know, an active attachment stay-at-home parent... (Wendy)

> I think I'm more controlling. So, I'm OK with it, but at the same time I need more help. (Sharon)

> ...so, I'll give him [my husband] six hours on Sunday to work by himself. Well, I've got...work coming out of my ears and no time to do it in... I would love two hours...if I don't ask for it I don't get it and because I haven't asked for it, I don't get it... (Victoria)

It seems, then, that mothers' choices to limit their partners' involvement with childcare are akin to, "shooting themselves in the foot." The only way in which to reduce their work overload is to open the gate for their partners. At the same time, doing this lessens their sense of control and power in this domain. For maternal gatekeepers, this is too high a price to pay for a little rest.

While fathers clearly gain from maternal gatekeeping in terms of personal time for leisure and/or career advancement, fathers also expressed a sense of loss from maternal gatekeeping. Many men reported that they wished that they had the same type of bond with their child as their partners had and they often felt confused as to how to go about attaining this without upsetting their partner. Moreover, many fathers expressed that they actively sought ways to establish a sense of belonging in their changing families. Fathers often felt lost as to what exactly their role with their children should be (a concern that was never expressed by mothers). This sometimes led to their feeling left out and in a few cases even led to feelings of unease with family life or resentment.

Ambivalence

While it seems clear that maternal gatekeepers are hurting themselves and their partners when they limit fathers' activities, the roots of their behaviors are complex. Several fathers who reported that their partners' engaged in gatekeeping activity felt ambiguous concerning this practice. Sometimes, for instance, men would ask their partners to leave instructions or lists for them concerning childcare or they would call their partners on the phone for parenting advice. Women sometimes reported that their partners expressed a lack of patience for particular parts of parenting (such as dealing with crying, illness and tantrums) and were relieved at the opportunity to hand authority over to mothers. In some cases men relied on mothers to do infant childcare, setting patterns that were not easily broken. In one extreme case, Tod, who made many comments concerning how he felt pushed out of his children's lives, mentions that he and his partner have had difficulties in the past, but neglects to explain that they were actually separated for a year after the birth of their first child. During this time, his partner Sharon explains that parental role patterns became set. And so, when asked how it came to be that she did most of the parenting work Sharon answers:

> Because we didn't plan to have a kid, it was just something that happened and he was more, "I wanna do what I wanna do and not revolve my life around a child." So, when I was, "Oh yeah, here's a kid and I'll revolve my life around this child"...Because I accepted it I guess.

While this example is extreme, men's ambiguity towards their partners' gatekeeping is significant. While Tod's feelings towards his children certainly changed when he returned to the family and he reported, "butting heads" with Sharon over gatekeeping issues, he still seemed relatively unbothered by the fact that Sharon carried out the overwhelming majority of parenting work and decision making. He made comments such as, "well, when it comes to the kids, I let her do what she wants." This is typical of many of the men in my sample who seemed to simply accept their wives as default parents and were not particularly bothered by gatekeeping activity. Molly's husband, Tom, for example, tells me:

> T: She's the boss of the house. I mean, she's the one that takes care of everything, so...and that's OK with me.
> Interviewer: How did she come to be the boss of the house?
> T: I think she just took it one day. I don't know (laughs). I don't really want it, so I'll let her have it, you know.
> Interviewer: Was it the same even when you were single [and a stay-at-home father]?
> T: Well, until she got home from work and then she was the boss...when mommy's around, mommy's the boss.

Hector makes similar comments:

> Don't argue with your mom, your mom is right. So, I do the same things that she does when she's there. But, when she's not there, I do it the way I want to [do] it.

This attitude from fathers is not surprising considering that men have been socialized to feel that women know best when it comes to parenting. Hector, for instance, makes it clear that he believes that Amber's parenting style is better informed than his own:

> Even, like, no matter how much I disagree, if it's going to make him better, I'll hold my tongue, you know?

> ...when she wants something done a certain way, there's no arguing. There's no arguing, we just do it. You know, she's like, "Put him to bed – put him to bed the right way." I'm like, "OK, I will..." And I – I put him to bed the right way.

Comments, such as Hector's, concerning women's superior knowledge when it comes to childcare were peppered throughout the majority of my interviews. However, men also seemed not to be extremely bothered by maternal gatekeeping because they did not base their personal identities on parenting behavior. While women made comment after comment about their conceptions of the role of mothers and how they perceive themselves in this role[1], fathers mostly spoke of their parenting roles as one component of their overall identity and, often, as linked to their work outside the home. It is, thus, completely reasonable for them to not feel threatened by their partner's gatekeeping activity.

In general, fathers who cared to be involved with their children's upbringing, but faced maternal gatekeeping activity viewed it as a trade-off for either lessened responsibility when it came to the rougher parts of parenting or, sometimes, for being allowed into the formerly female domain of childcare. This is best put by Hector who understands that his partner feels the loss of power associated with control over the home.

1 There is a large body of literature that deals with maternal identity (before pregnancy, during, and after) and the link between maternal identity and gatekeeping is well documented. For more see Rubin (1967); Mercer (1995); Gaunt (2008); Loftus and Namaste (2011). My own data concerning maternal and paternal identities is rich, yet beyond the scope of this paper.

> When I had a full-time job I wanted it like caveman days. I'd come home and there better be dinner, everything better be clean. And then now, since we had him, I kind of did a 180. "I'll stay home. I'll clean. I'll cook for you. As long as I can just stay home with him...I'll do whatever you want..." So, ever since he was born I want to switch roles, but she still wants control. And that's fine...

Hector's comment exemplifies the feelings that many of my father respondents had concerning maternal gatekeeping. They seemed to view this activity as a normal part of parenting relations. They felt pushed outside and, at the same time, were thankful for the fresh air. Maternal gatekeeping seems to be viewed as both a blessing and a curse, allowing for more personal freedom and less work while restricting the benefits that this work awards.

CONCLUSION

This research challenges general discourse concerning fairness of gendered work in the home. Typically, dialogue on equality and fairness is limited to discussion concerning who is subject to the greater work load and who is burdened by childcare responsibility. This analysis suggests that it would be important in our quest for equality to also consider the benefits and rewards that accompany a greater workload and responsibility. Equality involves both give and take and while maternal gatekeepers are giving in terms of workload, they are not giving in terms of work benefits. Maternal gatekeepers, metaphorically, keep their male partners unemployed in the home.

The data illustrates that women partake in maternal gatekeeping by asserting control over parenting decisions and over information concerning their children. Gatekeeping also takes the form of controlling parenting activity when one is away from the home. In many cases, gatekeeping activity restricts the involvement of fathers with their own children. Understanding how this activity takes shape is essential as society moves toward greater gender equality.

Secondly, this research draws attention to the complexity of the issue of maternal gatekeeping, pointing out that while men have much to lose from the practice, they sometimes fashion their partner's behavior themselves by displaying reluctance to participate in some of the less rewarding and more demanding tasks of parenting and by conceding authority to their partners. Indeed, both parents involved with gatekeeping behavior feel a sense of ambivalence towards it and accept and reject it simultaneously. Thus, the direction of the behavior is circular and reinforces itself.

While it is beyond the scope of this paper, it is important to mention that another source of maternal gatekeeping comes in the form of rituals surrounding breastfeeding, including co-sleeping with children in a manner that requires fathers to sleep in separate beds (and for some of my respondents even separate rooms), joining organizations and clubs, which systematically exclude fathers such as La Leche League International and the Holistic Moms Network (both of which do not officially exclude men, but are clearly not designed to include them, a detail which was not lost on my male respondents whose partners were members), and by using breastfeeding as a primary source of bonding well into toddlerhood. The link between breastfeeding and possible maternal gatekeeping is a matter of interest and calls for further research and analysis.

Other directions that future research may take include an examination of maternal gatekeeping among separated couples, same sex couples and teenaged couples. Several respondents hinted that behavior outside the home is rather different than that inside the home, which calls for an examination of maternal gatekeeping in both public and private spheres. Most importantly, the link between maternal gatekeeping and both maternal and paternal identity is well documented in the literature and deserves the benefit of a qualitative analysis. There is surely a great deal more to contribute to this growing field of investigation.

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APPENDIX A: RESPONDENTS

#	Pseudonym	Age	Occupation	# kids	age kids	Years Together	Ethnicity/Religion	int length	int place	int State	# of Partner	Married?
1	Daniel	35	Campus Crusade for Christ	2	2,4	11	none	30	university	WI	#2	y
2	Heather	34	Campus Crusade for Christ	2	2,4	11	Christian	35	friend's home	WI	#1	y
3	Jim	32	phlebotomist	2	2,5	10	Pagan	50	res home	WI	#4	y
4	Wendy	33	homemaker	2	2,5	10	Pagan	65	res home	WI	#3	y
5	Steve	27	Campus Crusade for Christ	2	1,2	4	Christian	50	university	WI	#6	y
6	Mary	30	Campus Crusade for Christ	2	1,2	4	Christian	60	res home	WI	#5	y
7	Mathew	29	unemployed (disability)	1	2	6	Christian	90	public library	WI	#8	y
8	Maria	27	student	1	2	6	Christian	90	Starbucks	WI	#7	y
9	Hector	24	odd jobs (stay at home)	1	1	4	Mexican	60	Starbucks	WI	#10	n
10	Amber	24	grad student	1	1	5	Catholic	55	Starbucks	WI	#9	n
11	Hilda	25	teacher	1	under 1	almost 2	Christian/part Swedish	70	Starbucks	WI	#12	y
12	David	27	project manager/MBA student	1	under 1	almost 2	Catholic	60	Starbucks	WI	#11	y
13	Robert	37	stage-hand (often home - night work)	1	2	12	Irish	75	Café	WI	#14	n
14	Ashley	32	bookstore	1	2	12	none	60	Café	WI	#13	n
15	Ruth	32	nurse/paramedic	2	1,3	6	Lutheran	60	Café	WI	#16	y
16	Richard	33	fireman	2	1,3	6	Lutheran	30	Café	WI	#15	y
17	Henry	40	military	2	under1, 10	17	Lutheran	45	res home	WI	#18	y
18	Klara	36	homemaker/bank	2	under1, 10	17	Lutheran/German	60	res home	WI	#17	y
19	Reuben	33	engineer	5	1,1,1,5,7	10	Christian	60	res home	IN	#20	y
20	Katie	33	long-time grad student/homemaker	5	1,1,1,5,7	10	Christian	65	res home	IN	#19	y
21	Molly	27	student	2	1,3	3	Catholic but not anymore	65	Barnes+Nob	IN	#22	y
22	Tom	30	unemployed/odd jobs	2	1,3	3	none	80	Barnes+Nob	IN	#21	y
23	Agbenyaga	40	homemaker	1	2	4	West African/Christian	45	park	IN	not in sample	y
24	Victoria	30	professor	2	1,4	6	Italian	45	res work	WI	not in sample	y
25	Ruth	31	homemaker	1	2	4	Pentecostal	50	church	IN	#26	y
26	Adam	31	Pastor	1	2	4	Pentecostal	50	church	IN	#25	y
27	Sharon	35	human resources	2	2,13	15 - 1 year break	Catholic	90	university	WI	#28	n
28	Tod	42	sales (self employed)	3	2,13,20	15 - 1 year break	none	120	res work	WI	#27	n
29	Lisa	30	student	1	newborn	4	Jewish	65	res home	OH	#30	y
30	Jordan	34	professor	1	newborn	4	Jewish	65	res home	OH	#29	y
31	Tanya	28	pizza delivery driver	1	2	6	pagan	90	Café	WI	#32	y
32	John	32	sales coordinator	1	2	6	pagan/ Native Americam	90	Café	WI	#31	y
33	Samantha	37	homemaker/ doula	1	2	4	Italian	70	res home	IN	#34	y
34	Don	47	self-employed	1	2	4	New Zealand	50	res home	IN	#33	y
35	Jamie	30	teacher	1	1	6	none	90	res home	WI	#36	y
36	Jerry	unknown	teacher	1	1	6	Lutheran	55	res home	WI	#35	y
37	Chantal	24	homemaker/midwife	1	3	6	Christian	65	res home	WI	#38	y
38	Jacob	unknown	unemployed/disability	1	3	6	Christian	65	res home	WI	#37	y
39	Tina	32	surgeon's tech	3	under 1,2,9	4	Lutheran	25	res home	WI	#40	y
40	Gabe	34	carpenter	2	under 1, 2	3	Lutheran	20	res home	WI	#39	y

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A REFLEXIVE LENS: EXPLORING DILEMMAS OF QUALITATIVE METHODOLOGY THROUGH THE CONCEPT OF REFLEXIVITY

Abstract Reflexivity has emerged as a central and critical concept in the methodology of qualitative social research. However, the concept of reflexivity is defined and taken up in a wide variety of ways, and the assumptions underlying each incarnation have particular implications for research outcomes. This paper argues that these various conceptualizations of reflexivity offer the qualitative researcher a critical lens through which to analyze key methodological dilemmas. Exploring the concept of reflexivity in its various forms provides an entry-point for understanding key dilemmas in the epistemology of qualitative methodology, qualitative research relationships, and the evaluation of qualitative research. These dilemmas present important challenges in terms of the complex issues of power, knowledge production and subjectivity. This paper thus demonstrates that a reflexive exploration of methodological dilemmas can provide a starting point for assessing the consequences and transformative potential of our qualitative research.

Keywords Reflexivity; Qualitative Methodology; Epistemology; Research Relationships; Power; Positionality; Quality; Research Dilemmas

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Too often in the social sciences there is a tendency to raise methodological issues and problems only when we "are talking about concrete techniques of evidence gathering" (Harding 1988:2). Working through methodological dilemmas need not be relegated to discussions of specific techniques, debating the merits or shortcomings of what are particular variations in the doing of qualitative methodology. Rather, I would like to focus on what all these various techniques have in common: the various dilemmas of doing qualitative work, regardless of the myriad strategies for data-gathering and analysis that one may employ in doing so. This paper uses the concept of reflexivity to render these dilemmas salient to a discussion of qualitative methodology.

The doing of "reflexive research" can be found to mean many things within literature. Authors vary in terms of their descriptions of what reflexivity actually is, although numerous attempts have been made to categorize and confine its various interpretations (Wasserfall 1997; Lynch 2000; Macbeth 2001; Pillow 2003). Despite the difficulty of pinning down a single definition, it is important to consider how the various incarnations of reflexivity have been central to the project of developing qualitative methodology and its historical transformations (Smith 1992; Zavella 1996 Finlay; 2002). As Wanda Pillow (2003) notes, the ways in which the qualitative researcher claims to practice reflexivity matter for our particular research outcomes. Thus, while there is no right or wrong way to do reflexivity, how we conceptualize reflexivity and incorporate it into our research practices has implications for the qualitative research we produce. I argue that these implications, in all of their complexity, can be made clear by exploring some key interpretations of the concept of reflexivity.

As Denzin and Lincoln have observed, qualitative researchers today continue to struggle with an ongoing crisis in qualitative methodology: a "triple crisis of representation, legitimation, and praxis" (2000:17). This paper will demonstrate that the concept of reflexivity allows us to break this crisis down into three questions that are important to explore in any qualitative inquiry. First, in our representations of the social world, what are our underlying assumptions about the production of knowledge – how do we know, and who can claim to know? What is considered legitimate knowledge, and what role does power, identity and positionality play in this process? Finally, how does one put into practice the reflexive techniques and address methodological issues in a way that results in valid, good-quality social research? These are the three main methodological dilemmas, which this paper will explore. The intention here is not to offer a resolution to any of these issues, but rather to demonstrate that it is in reflexively thinking-through these dilemmas that the researcher may benefit the most. Thus, this paper argues that the concept of reflexivity offers an important opportunity to explore crucial questions in the "thinking," the "doing" and the "evaluation" of qualitative methodology.

Though this separation is somewhat arbitrary, these three categories of dilemmas all relate to questions about the production of knowledge in qualitative methodology. Regarding the "thinking" of qualitative methodology, Douglas Macbeth (2001) notes that one reading of reflexivity involves raising questions about the *epistemology* of qualitative methodology: what is the foundation of our knowledge in this aspect of the social sciences, and who can make claims to "know" and represent others using qualita-

tive approaches? Similarly, Frosh and Emerson (2005) interpret reflexivity as a process of testing one's interpretations, and being accountable for the means by which we arrived at a particular "reading" of the data; in other words, making explicit the process by which we came to know. These are the two approaches to reflexivity that I will be using in investigating the epistemological dilemmas of qualitative methodology.

Dilemmas within the "doing" of qualitative methodology are drawn from reflexive accounts of the research relationship. The primary themes here are the issues raised by an understanding of power in the research relationship, the role of the researcher in applying qualitative methodology, and the dual problematizing of identity and positionality. Problematizing the presence and effects of power in the research relationship has been the primary concern of feminist reflexivity (Zavella 1996), raising the question of how we as researchers are implicated and located within these power relations in making use of qualitative methodology. For Jacqueline Watts (2006), reflexivity involves asking questions about role performance in conducting qualitative research, a dilemma complicated by Amy Best's (2003) own reflexive questions about the co-construction of identity. These readings of reflexivity dovetail into Reinharz's (1997) argument that reflexivity is a process of understanding how the multiply-positioned self is meaningful (and made meaningful) within qualitative projects.

Finally, reflecting on these epistemological and relationship-related dilemmas leads us to consider dilemmas inherent to the evaluation of qualitative methodology: namely, the issues of validity and quality. Howard Becker (1996) has argued that there is no concrete recipe for the production of quality qualitative work. In the absence of a check-list for evaluating research, we must reflexively consider other ways for evaluating the validity of knowledge produced through qualitative methods.

Overall, the dilemmas raised in this paper present some important challenges to qualitative researchers when analyzed through the lens of reflexivity. Methodology expresses particular forms of consciousness, a consciousness that is bound by a limited ability to conceptualize and enact social change (Sandoval 2000). Exploring methodological dilemmas through the concept of reflexivity provides an opportunity to reveal and understand these limits, which is a necessary first step to understanding the consequences and transformative potential of our research.

THE "THINKING" OF QUALITATIVE METHODOLOGY: REFLEXIVITY AND EPISTEMOLOGY

To think about methodology is to ask questions about epistemology (Hawksworth 2006), and in many ways, the project of developing a qualitative methodology has been founded upon epistemological dilemmas. In his classic study *Street Corner Society*, William Foote Whyte (1943) took the epistemological position that social researchers need to be in proximity to those they study, that we can learn about social life by being close to those who live it. The idea that we should take the point of view of those we study is a project with the epistemological foundation that accurate knowledge about social life can be achieved by going to the source of those who actually experience it, with the belief that "the nearer we get to the conditions in which [people] actually do attribute meanings to objects and events the more accurate our descriptions of those meanings are likely to be" (Becker 1996:58). Underlying this epistemology is an assumption that there is distance to be bridged between the researcher and the researched "other:" the source of information.

Feminist epistemology was founded upon an assumption that this distance between the knower and the known could be ameliorated, while at the same time drawing attention to the ways in which hierarchical research relationships have the potential to objectify our research participants (Wolf 1996; Shope 2006). Furthermore, an epistemological reorientation was offered by feminist theorists working towards an inclusion of experiential accounts in the production of knowledge about social life. Women (and other marginalized groups) have struggled to have their experiences included in the study of social life, for "experiential accounts are too readily equated with anecdotal evidence" (Code 1995:18), and thus devalued in positivist epistemology. Experiential accounts have been an important source of knowledge for critiquing long-standing sociological "facts" and theories about social life that did not correspond to the lived realities of marginalized persons (Collins 1990). Feminist critique of qualitative methodology also problematized the notion of value-free research, arguing that ideals of objective knowledge were not only impossible to attain, but also undesirable (Smith 1987; Harding and Norberg 2005; Hawksworth 2006). As such, reflexivity came to mean a deeper consideration of the subjectivity and role of the researcher in the process of knowledge production.

However, we should take care to understand the ways in which knowledge production that begins and ends with researcher subjectivities may reproduce epistemological problems for the project of qualitative methodology. Douglas Macbeth terms this "positional reflexivity," which involves "a disciplined view and articulation of one's analytically situated self," or how the subjectivity of the researcher enters into the process of knowledge production (2001:38). Underlying positional reflexivity is a problematic epistemological assumption that the reflexive social actor is the sole source of knowledge production. As such, positional reflexivity retains traces of enlightenment discourses of order and reason, and "organizes a professional gaze that locates the foundations for knowledge production and methodological rigor in the skeptical-analytic ego" (Macbeth 2001:41). Locating knowledge production within experience conveys the authority and ability to know as being within the reflexive researcher. Thus, in sourcing knowledge from experience, we run the risk of reproducing positivist divisions between the knowing researcher/unknowing participant; the very subject/object division that methodological

critiques had originally sought to challenge (Lal 1996).

Experience must therefore be reflexively positioned within the broader social contexts in which they occur, so as to avoid the dilemma of experiential knowledge standing in for a claim to authority. That is, we must be careful not to replace "the old tyranny of authoritarian expertise," one that discounts people's lived experiences, with "a new tyranny of «experientialism» that claims for first-person experiential utterances an immunity from challenge, interpretation, or debate" (Code 1995:36). Experiential accounts must be understood as particular interpretations; to accept experiential accounts as exempt from critical analysis runs the risk of romanticizing "knowledge on the margins" (Haraway 1988:584) and reproduces the dilemma of granting unquestioning authority in answering the epistemological question, "who can know?" A further epistemological dilemma arises when we consider the possibility of competing knowledge claims on the basis of experience: we have no means to decide between contradictory claims to knowledge on the basis of experience (Ramazanoglu 2002:78).

This leads us to problematize how to include the subjectivity of the researcher as an element in the process of knowledge production. Despite the above-identified problems that experiential knowledge presents for qualitative epistemology, it does raise the important reflexive question: what effect does the insertion of the self into the research process have upon the production of social knowledge (Lal 1996:200)? Answering this question has been a central and early theme in the development of reflexive methodology. For instance, the researcher's dedication to preconceived conceptual categories, political agendas, and an alignment with particular theoretical positions has long been identified as sources of methodological problems for sociological analysis. Glaser and Strauss (1967) argued that, at the time of their writing, the generation of new sociological explanations had stagnated due to a focus on knowledge verification between evidence and explanation, which oftentimes involved forcing a "fit" between empirical findings and pre-established grand theories of major sociological forerunners. To address this gap between empirical findings and theoretical explanation, Glaser and Strauss emphasized empirical data as the source for generating sociological knowledge – that is, our explanations for what we observe should be grounded in the empirical findings themselves. Grounded theory thus involves paying attention to, and making explicit, the process by which one generates explanations on the basis of one's data, with conceptual categories emerging from the data itself (Glaser and Strauss 1967).

What we "see" in our qualitative investigations must thus be reflexively thought of as "what we think we see," questioning the basis upon which we have made this interpretation. This involves understanding how one's own conceptual categories are brought into our observations and analyses, for as Trinh T. Minh-ha has observed: "questions are always loaded with the questioner's prejudices" (1989:69). The extent to which a researcher's own conceptual categories affect their production of knowledge is a particularly important question when we understand these conceptual categories as a product of our situation within a disciplinary field and academic tradition (Wacquant 1992; Bourdieu 2004). Beyond our prior commitments to theoretical perspectives and political orientations, our own culturally-contingent conceptual categories can also impact upon our understandings of what we see and hear in the research process (Becker 1998), and whether or not we will believe what our participants tell us versus what we think is "really" going on (Gordon et al. 2005; Shope 2006).

It is not an easy task to let go of one's conceptual categories. This approach has been attempted by Mykhalovskiy and McCoy, who rejected conceptual categories from the ruling academic discourses of health care policy in favor of an "empirically empty term, one that waited to be filled as [participants] told us about their practice and their experiences" (2002:24). In spite of this intention, they found that even such an empty term as "health work" reproduced a normative standard for their participants, who read work as an assumption that one is actively doing something about one's health in a way that would be conceptually categorized within health policy as good/acceptable (Mykhalovskiy and McCoy 2002:28). This experience demonstrates that preconceived conceptual categories will always be present within our methodology. What matters is whether and how well we acknowledge some of the ways in which these conceptual categories play a role in the process of knowledge production, and what the potential consequences might be for our analyses. We should also take care that the interpretations we make based on what we know (as academics immersed in a field of knowledge) do not erase our participants' knowing, and thereby their experiences (Frosh and Emerson 2005). As researchers, our dedication to particular theoretical (and methodological) approaches can hinder our interpretations by imposing conceptual categories that do not necessarily fit with people's experiences.

If conducting reflexive qualitative research entails asking questions of knowledge production, then these questions "have to be addressed locally, in piece-by-piece analyses of specific instances of knowledge-making, in which innovative techniques are adduced and tested, and the best of older methods and assumptions are re-evaluated for their residual viability" (Code 1995:43). In this sense, we are urged to consider using a methodology based upon a reflexive epistemology – one that continually questions and problematizes the social process of knowledge production.

THE "DOING" OF QUALITATIVE METHODOLOGY: REFLEXIVITY AND THE RESEARCH RELATIONSHIP

This section may appear to take up dilemmas in qualitative methodology that primarily apply to interactive research techniques (fieldwork, interviewing, observation, etc.) rather than text-based analyses. However, certainly not all techniques under qualitative methodology involve

interviewing, observation or other first-hand means for gathering data, and the dilemmas in this section are just as applicable to text-based and secondary analyses. For instance, the issue of power cannot be avoided in text-based analyses just because the research "relationship" is between the researcher and pieces of text rather than the researcher and human participants. There remains the issue of the power and authority of the researcher in presenting a certain version of reality by authoring a particular interpretation of that text (Macbeth 2001). As such, the dilemmas of qualitative methodology transcend whatever particular techniques we may employ in doing qualitative research, particularly when it comes to the issue of power.

REFLEXIVITY AND POWER DILEMMAS

Wanda Pillow (2003) argues that reflexivity arose as a methodological tool in the social sciences when power in the research relationship started to be discussed as a central concern, particularly in feminist research. Asking destabilizing questions about the objectivity of research led to questions about researcher/participant subjectivities, and from this, the question of power imbalances between them. However, if power is to be discussed as a dilemma in qualitative methodology, it should be noted that the ways in which we imagine power matters. Our definitions of power will impact how it is conceptualized as a problem, the kinds of issues we identify (or fail to identify) as methodological dilemmas, and potential solutions. This, in turn, influences our understandings of social differences and subjectivities (Sandoval 2000; Gordon et al. 2005). I argue that an understanding of power that goes beyond the immediate interactions of research is most useful for understanding the implications that power differentials have for qualitative methodology. To limit ourselves to an understanding of power solely within the interactional context renders us unable to consider the full extent to which power may pose dilemmas in all relevant aspects of qualitative research.

For instance, Elizabeth Hoffman (2007) argues that further attention should be paid to emotion in the interviewing relationship, and that emotional labor in the interview should be included as important research data. She interprets emotional labor as shifts in the power relationship between interviewer and interviewee. In doing so, Hoffman characterizes power as something that shifts back and forth: at one point the interviewer has power, then the interviewee does, and so on. She also imagines power as multidimensional: while the researcher has the power to craft particular stories about the research subjects' experiences, our participants also have the power to challenge this, by refusing to answer, redirecting questions, confronting the interviewer – as well as answering questions however they choose. Power is imagined as a possession that can be attained and used by social actors. For Hoffman (2007), these power shifts represent important data that tends to be neglected in the production of social research. The methodological challenge posed by this analysis is to examine these power shifts and the emotional labor that accompanies them as spaces in which interviewer/interviewee relationships are contested and social knowledge is produced.

However, conceptualizing power as a possession holds particular assumptions about the research relationship: first, that power shifts between the interviewer and interviewee, like a ball being passed back and forth. This limits problematizing power differentials to the question of who holds more or less power at any given point in time through an interaction-based analysis of the research relationship. Power becomes something that can be given to or taken away from a social actor; indeed, Hoffman notes that qualitative researchers necessarily "abandon some of their power" by choosing to interview in the first place (2007:321-322). This implies that her methodology is inherently imbued with the potential to empower those we research. What is not considered in Hoffman's account are broader power relations that exist beyond the research interaction; from this perspective, we run the risk of homogenizing entire categories of participants without considering the kinds of power differentials that exist between them. An alternative reflexive analysis of power would thus seek to contextualize power imbalances of the research relationship within broader relations of power outside of the immediate research setting.

To broaden our understanding of how power might be imagined in the research relationship, we can turn to the writings of Michel Foucault. Foucault argued that relations of power are circulatory (Foucault 1977:199). For Foucault, power is not something that is intrinsically held by persons; it is the effect of discursive struggles over the realm of meaning and production of knowledge. Nor is power simply imposed from above or held by a singular source; it is distributed throughout social relationships (Foucault 1978:101). Susan Bordo clarifies this conceptualization of power, noting that, within a Foucauldian approach, "the fact that power is not held by any*one* does not entail that it is equally held by *all*. It is «held» by no one; but people and groups *are* positioned differently within it. No one may control the rules of the game. But not all players on the field are equal" (Bordo 1993:191). This understanding of power allows us to consider the ways in which our research participants are variously located within relations of power outside of the immediate interviewing context, as well as the ways in which we as researchers are variously positioned. The utility of this relational approach to power is illustrated in the work of Shanaz Khan (2005) when she takes care to note the heterogeneity of her research participants in terms of their positions in relations of power. In studying "third world women," Khan reflects upon the risk of constructing a unitary identity for her participants as racialized, oppressed and powerless, as this would have the effect of erasing the various positions of her interview subjects in relation to the social hierarchies that informed their everyday experiences. We might also consider the ways in which researchers themselves are variously located within relations of power, lending a multidimensional understanding to the dilemma of power in qualitative method-

ology (Hsiung 1996). A multidimensional approach to power can assist us in understanding power relations beyond the immediate research interaction as a potential dilemma, with macro-social forces impacting upon both relationships in the field and the way the research is received by international audiences (Michalowski 1997; Khan 2005).

Conceptualizing power as a possession to be passed back and forth in the research relationship may hinder our ability to understand the research relationship as a site of problematic power differences. A possessive understanding of power leads Hoffman (2007) to speculate on how power differences may be ameliorated – and yet, she is unable to articulate how sharing personal information with her informants gives them power. How can one's informants use personal information about the interviewer in a powerful way when they are so differently positioned within broader power relations outside of the interviewing context? Conversely, in sharing information with us, our interviewees take greater risks. In interviewing women engineers, Watts' (2006:387) research operated on a "model of mutuality:" disclosure was traded back and forth between the researcher and the participants, leaving Watts to wonder about the extent to which researcher's political values should be revealed when rapport may depend on this mutual sharing. One question that is *not* asked by this assumed "model of mutuality," however, is: how can we be assured that disclosure is indeed ever fully mutual? By neglecting the existence of power relationships beyond the interaction between researcher and researched, we may be limiting our understanding of the ways in which our participants are similarly constrained in their ability to disclose.

Power is ever-present in the research relationship and trying to equalize the relationship does not erase the researcher/researched power differentials that reflexivity reveals (Wolf 1996). While we can come to understand these power differentials, there are limits to our ability to address the issue of power in qualitative research through a reflexive methodology. In examining the relationship between feminist methodology and political commitments, Wasserfall (1997) argues that care should be taken not to overextend our reading of reflexivity as a solution to the dilemma of power differences in the research relationship. While we can understand the impact of power, we deepen the dilemmas posed by it if we assume they can be erased. She analyzes this as the difference between a "weak" and "strong" reading of reflexivity. The "weak" reading characterizes reflexivity as an ongoing self-awareness project, a continual mindfulness of the social processes between the researcher and researched, including an understanding of power differentials and the ways in which we represent the subjects of our research. Yet, Wanda Pillow (2003) cautions against this form of reflexivity for its tendency to deal with power merely as a confessional tale. As Pillow notes, while transparency may enhance our understanding of power differentials, it does nothing to actually alleviate those problems. Transparency can "yield a catharsis of self-awareness for the researcher" by pointing out that the dilemma of power exists and affected the research in some way, but cannot offer any particular solutions (Pillow 2003:181). Thus, it is important that the promises of reflexivity not be overextended, for "reflexivity is not in itself a process for overcoming distortion or exploitation" (Wasserfall 1997:152).

Conversely, a "strong" reading of reflexivity begins from the assumption that reflexive research can promote a *break-down* of power differences between the researcher and the researched. This is problematic in multiple ways (Wasserfall 1997). The pitfalls of a "strong" reading of reflexivity as a methodological tool was apparent to Wasserfall in conducting her own research: attempting to help her participants connect their immediate experiences of oppression to broader social processes and thereby empower them (i.e., equalizing the power differences between researcher/researched). A "strong" reading of reflexivity hindered her relationships in the field when her participants resisted her own interpretations of their experiences. Wasserfall's experience speaks to Lynch's (2000:36) argument that it is problematic to assume reflexivity is ever "inherently radical" or necessarily transformative. Harding and Norberg (2005) point out that taking up reflexivity as a way to break-down power imbalances might be contradictory to our research aims: on the one hand, we may be preoccupied with trying to minimize power imbalances in the research relationship, while on the other we hope to evoke powerful transformations and social change. For instance: the power of academic research is what gave rise to the political usefulness of Mykhalovskiy and colleagues' (2008) work in collaboration with frontline health care workers. Taking up health care workers' experiences in the form of a scholarly report imbued the workers' narratives with the authority of academic work, providing them with a better means to enter into health care reform debates. Thus, the power imbalance that a "strong" reading of reflexivity assumes to break down is at times the very source of our authority by which we, as research "experts," are able to gain legitimacy and political impact.

While this tension cannot be resolved, a reflexive approach that involves making power imbalances explicit can be useful for understanding the diverse ways in which power operates. Using this approach, we can problematize the assumed binary between powerless participant and powerful researcher, and challenge the unitary identities that these positions are assumed to entail. First, reflexivity as an understanding of power differentials reveals problems with the assumption that the author is *always* in a position of power – especially when the author takes up reflexive methodology as a practice of writing the self into the text. The reflexive inclusion of the self in our authorial accounts is actually something that can work against the author's authoritative claims to knowledge. As Minh-ha (1989) has noted, good writing – that is, writing that can make an authoritative claim to knowledge, thus imbued with the power of legitimacy – has conventionally involved removal of the author from the text. In conventional writing, to write the self into the text is seen as problematic and always presents a dilemma to the author who does not fit the authoritative model of a white male self.

Minh-ha's work reminds us that not all authors are powerfully positioned, and the conventions of writing authoritative accounts are such that the heterogeneity of authors is erased.

A reflexive analysis of power thus critiques the tendency to understand our research subjects as a unitary group. Due to a tradition of "studying down," there is a tendency in qualitative methodology to erase hierarchies among those we study, which prevents us from understanding the heterogeneity of our participants and blinds us to the existence of power differentials between and among them (Khan 2005). In doing so, the qualitative researcher may miss important opportunities not only to analyze the diversity of one's participants, but also to produce research that challenges constructed homogenous identities of disadvantaged, disempowered others (such as "3rd world women"). These critiques illustrate the ways in which power differentials are a complex dilemma in qualitative methodology, for how we go about articulating power and using reflexive strategies to mediate power dilemmas will have important consequences for our research and analysis.

REFLEXIVITY AND SUBJECTIVITY: ROLE PERFORMANCE, IDENTITY AND POSITIONALITY

If problematizing power is an important part of reflexive analysis, it follows that the subject positions within these power relationships should be similarly investigated.

Understanding the self as a research tool is an important part of the reflexive research endeavor (Watts 2006). To assure the maximum utility of the self within the field, the researcher presents the self in particular ways (Goffman 1959): for instance, as having credibility, legitimacy and authority (Watts 2006), or conversely as being inexperienced, student-like, and in need of the participants' clarification (Hoffman 2007). One also performs emotional labor, playing various and sometimes conflicting roles in managing the emotions of our participants as well as our own (Hoffman 2007; Dickson-Swift et al. 2009). A reflexive approach to role performance can extend beyond the roles played by the researcher in order to similarly consider the role performance of our research participants. They too can be analyzed as performing in particular ways to fit the script required by their own audience (i.e., us as researchers) (Murray 2003).

Furthermore, a reflexive understanding of role performance reveals that research roles are neither stable nor static. Naples (2003:63) attempted to implement the approach of "passionate detachment" in her study of a small American community, but found that this performance was difficult to maintain as her relationships with community members changed over the course of her research. She became friends with some, and was perceived as an advocate by others as the research process went on. This experience underlines how our role performances necessarily change because our research relationships themselves change: in the field, our relationships – and the roles they entail – are as dynamic as the social processes we study.

If we can choose from and shift between multiple, dynamic roles in the field, the question remains: which role takes primacy, and when? Susan B. Murray's (2003) reflexive analysis of research roles suggest that these decisions may appear during times of crisis in the research relationship and the experience of conflict between one's multiple roles. In working at a women's crisis centre, Murray (2003) performed an official role as a representative from a feminist organization, expressing sympathy and an unwavering belief in a woman's telling of her abusive experiences. However, in a private conversation, her co-workers expressed some disbelief in the woman's story, contradicting the feminist principles of the crisis centre. This created a crisis of roles for the author, who at once experiences conflict between multiple self-presentations: the self as sociologist, as feminist, and as agency worker. Amidst this crisis, Murray takes up reflexivity as an analysis of the shifts between these roles: a means of understanding and making explicit how one's practices and decisions in both the front and back stages are informed and constrained by "the larger structural (and political) contexts that frames [one's] research" (Murray 2003:379), such as ethical codes, the need to continue with one's research, the rules of the research setting, and so on. While the qualitative researcher may experience ongoing role conflicts, we may work through the conditions under which these conflicts occur as a part of the "job" of reflexive analysis. In this way, reflexivity makes explicit how the self is a meaningful research tool that shifts back and forth between multiple, and sometimes conflicting, role performances, and the implications of this for our research relationships and decisions.

A second way to understand the self as an important research tool has been to take up identity as a methodological dilemma. There is a tendency to focus on researcher roles at the expense of researcher selves, leaving the question of identity unexamined (Reinharz 1997). This hesitancy may be due to the concern that, in discussing identity, we run the risk of reinforcing a discourse of the authentic self. Disputes over identity often involve a tension between one's "being" (identity) and one's "doing" (role performance) – with one's being assumed to be made up of static and essential attributes (Brekhus 2008). That is, there is a tendency to create a false division between what one *is* (authentic, essential) versus what one *does* (inauthentic, performative). By distinguishing between researcher roles and researcher *selves*, there is some concern this contributes to such a division. However, a challenge to this division may lie in a reflexive problematization of identity, which involves thinking about how identity is an ongoing process, co-constructed in the research experience.

Typically when identity is discussed as a dilemma in conducting qualitative methodology, it has been restricted to the problematizing of the baggage we bring to the field, with the underlying assumption that "the researcher's biography with regard to race, class, and gender is already

formed prior to the research experience rather than being an emergent feature of the research process itself" (Best 2003:908). A reflexive understanding of identity as a dilemma differs from this approach in trying to make explicit the ways in which identity is formed through the interactions of the research relationship. This is the difference between asking "what impact did the researcher's race/gender/class have on the research relationship?" and "how are race/gender/class *made meaningful* in this relationship?" As researchers, we may bring our own baggage to the research relationship in terms of how we conceptualize what identity actually *is* (Zavella 1996). A reflexive methodology can allow the meaning of identity to emerge from the perspective of those we study, rather than importing conceptual understandings of identity from pre-established academic categories.

However, care should be taken to address this methodological dilemma in ways that destabilize, rather than solidify, identity. The inclusion of identity in the research analysis runs the risk of creating monolithic, stable stories of identity construction if one's identity is analyzed as though it were a finished product. This stabilization of identity often takes place in attempts to address power differentials, assuming that one can "deconstruct the author's authority" through the inclusion of multiple voices in the writing of the narrative account – not just the voice of the author (Pillow 2003:179). The assumption is problematic because, as Pillow (2003) notes, the inclusion of multiple voices promotes a tendency towards solidifying the identity of those voices, drawing boundaries between each voice and reinforcing a distinction between the researcher/researched, self/other, us/them. Instead, reflexive research may be taken up as a way to destabilize identity as an ongoing process that is never finished nor fixed, as well as acknowledge the discomfort that arises from doing so. Furthermore, identity as a co-construction is not necessarily a process that runs smoothly, and identity can be misread or challenged (Best 2003). Our research participants are not passive recipients of our identity, in spite of how we may think of ourselves or how we may assume to be presenting a particular identity.

But an understanding of identity as complex and in-flux does present some anxiety for the qualitative researcher. This dilemma is similar to that presented by the multiple roles within the research relationship: Minh-ha (1989:6) struggles with the question of where to place her loyalties among her multiple and sometimes conflicting identities as a writer, a woman, and a racialized subject, and asks herself which should be prioritized in her writing. From a reflexive analysis, this dilemma is further complicated when we consider that to give priority to any one aspect of our identity runs the risk of being read as a unitary subject by the audiences of our research (Khan 2005). To combat this problem, a reflexive analysis can insist on the multidimensionality of identity. Rather than being concerned with which aspect of one's identity should take priority, reflexivity can instead aim for intersectionality, such that the relevance of any one dimension of identity is "fluid and context-dependent, with saliencies

that change and shift over settings and time" (Brekhus 2008:1071). A reflexive approach that prioritizes intersectionality can be useful not only in resisting the assignment of a "master status" (Brekhus 2008), but also in thinking through how the multiple dimensions of identity may be relevant to the research relationship. While we can only understand the research if we know what our attributes mean to those we research, we cannot know in advance what aspects of our identities will be important to those we study (Reinharz 1997). Of course, this begs the question of how we can know what aspects of identity are most salient in the research process, and whether or not these match up with the conceptual categories of race, class gender, et cetera as understood by our participants, our discipline, and the wider audience of our research.

A third and final way to understand the self as a research tool is in a reflexive understanding of positionality as a methodological dilemma. As researchers we are embedded within particular theoretical traditions and perspectives as well as methodological practices (Watts 2006). In conducting our qualitative research, we may be differently positioned by research participants who tend to define and situate us in relation to the context of their social world (Best 2003). Our pre-assigned or enacted positions as researchers can affect the kinds of research relationships we experience (Acker 2000). Furthermore, in entering the spaces (or fields) in which we conduct qualitative research, there is not necessarily a ready-made position for us to fit into, requiring us to re-negotiate our positions as researchers (Gordon et al. 2005). Positionality is thus as much a co-construction as is the meaning of identity in the research relationship.

Investigating positionality involves a reflection upon social location and the self as situated within broader social structures. While we occupy particular positions as researchers entering into the research relationship, there are numerous other ways in which we are positioned outside of the immediate context of the research relationship. National and international laws and social order also structure our positions, as Khan (2005) notes. By virtue of both her passport from a western nation, as well as her freedom of legal movement, Khan was "positioned differently" (2005:2025) by not sharing the risks of the imprisoned women she interviewed, and in terms of the potential consequences her project entailed. Watts (2006) further notes that positionality can refer to one's positioning within particular theory traditions and approaches, thus expanding the definition of positionality beyond social location in terms of such variables as race, class, gender, et cetera. In this sense, a reflexive understanding of positionality can involve "bringing to consciousness the social foundation of intellectual affinities" (Bourdieu 2004:113).

The value of understanding positionality has been emphasized as essential to a reflexive qualitative methodology. Harding (1988) insists that knowing the researcher's place makes the research understandable. Making one's positionality explicit is to give context to the researcher's voice, rather than reproducing the

anonymous, de-contextualized voice of authority. In other words, knowing the position from which the author speaks is crucial for our ability to understand what is being said. Furthermore, positionality can impact upon our perception of research problems (Bolak 1997:95). Depending upon one's positionality, one may have different answers to the very understanding of what counts as a research dilemma.

Perhaps the most important way of understanding positionality as a methodological dilemma arises from an analysis of insider/outsider positioning: that is, troubling the assumption of one's positioning as a researcher outside of the community/location under study, versus the insider positioning of those we study. Sandra Acker notes an interesting contradiction that arises from this binary subject position: in qualitative methodology, insider researchers are often encouraged to create distance between themselves and the social phenomena they study in order to think about what is "really" going on (2000:2). For insiders, holding the data at arm's length is characterized as being required in order to have a clearer picture of "the truth." Conversely, the outsider has been required to immerse themselves until the strange becomes familiar, for therein lies understanding: the outsider must work towards getting as close to the insider as possible, for the insider is the assumed source of knowledge (Acker 2000). This suggests that the subject positions of insider/outsider carry with them particular assumptions about the production of knowledge. In other words, the production of knowledge must be contextualized in an understanding of how it is "located in relation to the subject positions of its producers" (Watts 2006:401).

In using a Goffmanian analysis (1959), one's positioning in terms of "insiderness/outsiderness" is not taken as an essential subject position, but rather is understood to be a strategic performance. For instance, Berik (1996) performs the role of an insider by conforming to the expected gender roles of the women she studied in rural Turkey. Being a Turkish woman herself was not enough: she had to "play the part," so to speak, in order to gain access to the women she was interested in interviewing. Similarly, outsiderness can also be strategically performed in taking up the role of the novice seeking to learn from qualified insiders – a point noted above to be foundational to the early development of qualitative methodology (Best 2003). In analyzing insiderness/outsiderness as performative positionality is assumed to be something that we can manipulate for our own research needs.

However, the extent to which we can manipulate our positioning through performance is limited when we reflexively consider how we are positioned by others within and beyond the research relationship. In doing so, insiderness/outsiderness is not a clear positional dichotomy. Shope (2006) experiences the dilemma of insider/outsider as one of simultaneous positioning, noting that she is at once an insider in the community of women she studied by virtue of her gender, yet an outsider by virtue of her race/nationality. One can experience a simultaneous positioning as both an insider and an outsider,

occupying the subject position of the "outsider within" (Collins 1991). We might also consider insiderness/outsiderness not as fixed positions, but rather as dynamic positions within the research process: our participants may become insiders or informants to our own projects as we develop relationships with them (Murray 2003).

Troubling the positionality of insiderness/outsiderness reveals some important shortcomings that understanding these positions as a binary entail. In sharing a similar historical positioning with the Pakistani women she studies, Khan (2005) is concerned that her work will be received in western academia as being the product of a "native informer," able to convey an insider's knowledge. She notes this is problematic in two ways: first, as discussed above, the native informer is characterized as a homogenous, "unitary subject" (Khan 2005:2023) in a spokesperson position to tell things "as they really are." Herein lies the second problem in the assumption that native informing is the source of "authentic" knowledge. However, to the Pakistani women she interviewed, Khan was sometimes seen as "not authentic enough" in her position as a western-based researcher. Read as an insider by a western audience, and yet not accepted as an insider by the women she studied, Khan's experience leads us to question not only the binary positions of insider/outsider, but also our assumptions about "authentic" insider knowledge. Thus, problematizing the insider/outsider binary destabilizes the possibility for insider knowledge. Naples contributes to this destabilization in noting that, in her own work, she "[has] yet to meet a community resident who feels completely like the mythical community insider, although several people presented themselves as more «legitimate» than others" (2003:57). If the community insider position is one that does not fit with people's experiences of community life, then the possibility for insider information about that community is called into question. Bolak (1997) further problematizes insider knowledge in the process of studying one's own. Simultaneously, as a researcher and a member of the community she studied, Bolak found herself challenged by having to rethink some of her own assumptions about what she thought she knew as an insider. Identifying with the position of insiderness does not free the researcher from the afore-noted dilemma posed by importing conceptual categories into the research relationship.

Note that this problematizing of the insider/outsider binary – and subsequently insider/outsider knowledge – is a different approach from that of Becker, who urges the researcher to be skeptical of "the instability of «native» meaning" (1996:6) in seeing the insider informant as not necessarily a reliable, consistent source of information. Rather than taking up the dichotomy between insider/outsider as a dilemma, Becker takes issue with how our informants may be indecisive about the meanings and explanations they provide us with. While this perspective allows the researcher to remain open to inconsistencies in the meanings attributed to the social phenomena under study, framing these inconsistencies as an issue of trustworthiness or reliability of our informants introduces anxiety around the

truthfulness of informant knowledge-claims, while leaving the division between insider/outsider positions intact. While Becker's perspective does not provide a way of breaking apart the insider/outsider dichotomy, his approach to reflexivity does lead us to problematize the issue of quality in qualitative methodology, and the role that reflexivity can play in raising such evaluation dilemmas.

REFLEXIVE EVALUATIONS: VALIDITY AND QUALITY IN QUALITATIVE METHODOLOGY

The quality of qualitative research has historically been a story of pressure to appeal to scientific models of evaluation in establishing credibility (Denzin and Lincoln 2000). However, reflexivity can complicate the taken-for-granted use of such measures of quality in qualitative methodology – in particular, our ability to use validity as a research goal. Cho and Trent (2006) argue that the validity of qualitative research must be understood as contingent upon the context of our research problems and the goals we hope to achieve. As such, validity must be seen as a process that is "ever present and recursive" rather than just one step in the research process. Further, validity is contingent upon the context and goals of our research problems (Cho and Trent 2006:327). What this suggests is that procedural evaluations are problematic in qualitative methodology, and applying a set of universal evaluative criteria to all qualitative projects is not possible. When criteria check-lists are applied to evaluate qualitative research, they tend to follow a positivist model in defining quality as to whether the researcher made the right choice of method and executed it in the right way. Thus, researchers are viewed as potential sources of error and contamination (Eakin and Mykhalovskiy 2003:190).

It is not only the activities of the subjective researcher that have been problematized as a potential source of error, but also the processes of social scientific method within which the researcher is positioned. As discussed above, Bourdieu (2004) has argued that employing the conceptual categories of a disciplinary tradition without critique or question can cause problems for our understanding and interpretation. To combat this problem, Bourdieu makes use of a "reformist" reflexivity, which he describes as "an effective means of increasing the chances of attaining the truth by increasing the cross controls and providing the principles of a technical critique" (2004:89), involving a constant reflection on the modes of thought embedded in the academic system in which we are positioned. Distinguishing this from "narcissistic" (self-centred, experience-based) reflexivity, Bourdieu insists that a reformist reflexive analysis must go beyond explicating individual experience and the steps taken in the research process in order to understand how one's position within a disciplinary field and academic universe "is liable to obstruct knowledge of the object" (2004:92). Operating on the ontological assumption that there is an objective reality to be known, reformist reflexivity is conceptualized as a way of obtaining "the truth" – of coming closer to an objective reality by eliminating errors inherent to disciplinary modes of thought, and thus producing better quality research.

What is not clear in Bourdieu's concept of reformist reflexivity is how the researcher is able to fully step outside of the intellectual modes of thought inherent to the discipline/academy in an objectivist pursuit of "the truth" as a research goal. Furthermore, the evaluation of a research account as more or less "truthful" can be a complicated achievement in that the extent to which one has achieved accuracy is not a self-evident, objective evaluation. Judgments about the accuracy of our work and the ability to lay claim to credible knowledge are just as much a part of the social process as all other aspects of knowledge production that Bourdieu critically questions. Thus, it is not clear how one could test the extent to which such reflexive processes actually improved the quality of knowledge production, even over the long term. As noted in the earlier critique of positivist epistemology, women and other marginalized groups have struggled against positivist approaches to social science that left them with "no way of making their experiences count as informed or knowledgeable" (Code 1995:20). This has been the struggle of marginalized groups to claim a position of trustworthiness or credibility in making their experiences known. Howard Becker (1967) has also long-noted the anxiety in qualitative work around establishing trustworthiness as a methodological dilemma. In the production of knowledge, there exists a "hierarchy of credibility" regarding whose knowledge is considered more trustworthy, and whose knowledge is considered suspect or biased. Becker argues that accusations of bias are typically made "when the research gives credence, in any serious way, to the perspective of some subordinate group in some hierarchical relationship" (1967:240), because we have given attention to that which is seen to be untrustworthy and not credible; the unofficial account of those who are not in positions of power. For Becker, it is the official accounts that we should be most suspect of. Official accounts are a bad source for knowledge because "things are seldom as they ought to be" (Becker 1967:242). By thinking about the ability to claim credibility as an issue of power, we introduce two important complications: first, that power is involved in claims to legitimate knowledge; and second, that marginalized populations are diverse in their ability to claim credibility, being differentiated as more or less credible on the basis of class, race, gender, ability, occupation, and so on. This is not to deny that powerful government actors are also often treated skeptically, as their claims are often interpreted as ideology or propaganda. Perhaps having too much or too little power is what signals alarm bells for potential receivers of information, since it is in these cases that the stakes and interests in the claims are highest.

The ability to claim credibility and quality further depends upon one's adherence to the norms and expectations of knowledge production. For instance, emotions are not typically included as data, since they are often seen as an element of contamination, obstructing objective knowledge. This is the case even though emo-

tions and reactions to emotional expressions play an important role in the sharing of information during interviews (Hoffman 2007). Furthermore, the reflexive method is often limited solely to an analysis of the steps taken in the research process, rather than applied to the development of theory and conceptual explanations – perhaps because it does not seem as rigorous to talk about theory development, including all the mundane interactions that led to adopting particular concepts and understandings (Puddephatt, Shaffir and Kleinknecht 2009). We may understand this oversight as symptomatic of the lingering pressures placed upon qualitative method to conform to particular accepted forms of knowledge production. The extent to which we avoid a reflexive analysis of theory development in order "to preserve, if not salvage, the scientific credibility of our work" (Puddephatt et al. 2009:12) may be a product of the norms of knowledge production embedded (and unexamined) within the discipline. Thus to use reflexivity to engage with even the most mundane aspects of research, as Wacquant (2009) argues for, is to challenge the rules of sociological (scientific) knowledge in taking up an analytic frame that does not fit with accepted modes of rigorous knowledge production.

Knowledge produced in collaboration with groups who are differently positioned in their ability to claim credibility also runs the risk of being read as biased or "contaminated" (Mykhalovskiy et al. 2008). This is complicated by the problem that marginalized groups can benefit from the advocacy of "experts" in having their experiences processed into legitimate, credible forms of knowledge, such as academic reports (Code 1995) – but only if said academic reports and the knowledge presented therein are sanctioned as legitimate and acceptable by conforming to the expected norms of knowledge production. We may experience a knowledge clash in presenting knowledge that does not fit with accepted academic terms, concepts and politics (Zavella 1996). Thus, the researcher who seeks to disrupt inequitable claims to credibility by presenting work in the language of the marginalized is faced with the dual problem of risking misrepresentation on one hand, and limited opportunities for political critique on the other.

In problematizing the ability to make a claim for credible knowledge, it is useful to turn to an understanding of how power is implicated in the process of legitimating knowledge. Michel Foucault's (1972) method for analyzing the relationship between knowledge and power is to focus on discourse. Discourses are the boundaries of meaning on a particular topic: what one is able to say about something. This meaning, however, does not overlie some truthful, authentic existence that can be discovered outside of discourse; in this way, discourse is not just "surface content" or "a mere intersection of things and words" (Foucault 1972:48-49), nor is it an expression of a previously established synthesis (Foucault 1972:55). That is, there is no ready-made meaning that can be "read" from the world (Foucault 1981:67). To analyze discourse is not to question "whether things exist" but rather to approach our research with questions "about where meaning comes from" (Hall 2001:73). Foucault did not dispute that there are such things as material experiences or existence. Rather, he argues that there is no meaning to these things outside of discourse. In this sense, discourses are "practices that systematically form the objects of which they speak" (Foucault 1972:49). Foucault's discursive method analyzes the productive process by which *we come to know*.

From a Foucauldian approach, an analysis of discourse entails a particular form of critical engagement with the statements and social practices associated with one's topic of social inquiry. If discourse is the meaningful way by which we make sense of the world, and this meaning is the *limit* of our ability to apprehend the world, we are faced with important implications for our ability to evaluate the quality of sociological research. From a Foucauldian perspective, evaluations that critique the quality of any expression of knowledge as being inaccurate misrepresentations are problematic. Rather, by focusing on discourse, the point of critique becomes how statements *produce* a meaningful reality, and how social practices are limited to this realm of meaning. However, this is not an unproblematic process of making meaning. For Foucault, not all statements are considered simultaneously and equally within the realm of possible expression. Foucault argues that this is because *relations of power* circulate within discourse, and in this way particular knowledge and meanings come to be normalized and considered legitimate over excluded others (Foucault 1977:199).

Foucault's methodology provides a way for reflexive analysis to move away from evaluations of quality in sociological research that rely on the re-inscription of a positivist concern with accuracy. It is easy for qualitative researchers to fall into the same patterns of reassuring accuracy while simultaneously trying to problematize the possibility for accurate representations. Shope (2006) exemplifies the lure of the accurate research evaluation in an interesting instance of contradiction. In presenting her work on interviewing women in rural South Africa, she assures the reader that she had "a South African professor of African languages" go over the transcripts of her research interviews in order "to ensure that the responses of the women were accurately transcribed into English" (2006:167); and yet, how can this accuracy be ensured while simultaneously acknowledging that "conveying rural women's words in English mutes their voices" (Shope 2006:167)? Claiming to have produced valid knowledge (through accurate translation) while at the same time problematizing the very possibility for accurate knowledge (due to the impossibility of full translation) might be read as a contradictory position. The claim that the translator ensured a greater accuracy of conveying voice than without the translator would seem more tenable. However, perhaps Shope's concern with accuracy can be understood as a broader methodological anxiety about *representation*. The researcher wishes to be a "worthy ally" (Shope 2006:167) in her project, and to avoid the harm of telling a problematic story that misrepresents her participants. In simplest terms: as researchers, of course we want to

"get it right" in producing our research accounts – and to convey to the reader that some steps were taken towards this purpose. Yet, in so doing, we should be aware of the ways in which we may be continuing to associate quality with the goals of perfect accuracy, absolute truth and complete knowledge, since this echoes the traces of a positivist reality (Minh-ha 1989:64).

In striving to uncover "the truth" of an account, the researcher may misidentify problems of validity. In doing so, our attempts to re-construct a story to conform to an idealized consistent, single narrative may erase important points for further reflexive analysis. Watson (2006) takes up this critique in her reflexive analysis of the meanings of lapses and inconsistencies in her analysis of interview transcripts. She concludes that "slips" in the narrative consistency should not be viewed as problems of validity in need of correction, but rather as points at which we may reflect upon the (re)negotiation of identity. Contradictions in the interview "can be interpreted as pointing to shifts in the prevailing discourse" (Watson 2006:381), rather than validity problems caused by the seemingly contradictory stories of unreliable narrators. As researchers reflecting on our interview transcripts, resisting the temptation to smooth out contradictions in our anxiety over validity can open up the possibility to see these retellings as points for reflexive analysis: in this case, a reflexive analysis that involves linking narrative reconstructions of interviewees' lives, experiences and explanations to the broader socio-cultural context.

CONCLUSION: LIMITS AND PROMISES OF REFLEXIVITY

While reflexivity is not a magic cure for methodological dilemmas, what matters for the consequences of our research in the end is, as Michael Lynch has observed, "who does it and how they go about doing it" (2000:36). One thing that is clear from this discussion of reflexivity is that qualitative methodology in its complex entirety, and the kinds of qualitative accounts we produce, may simultaneously benefit from and be constrained by one's particular approach to reflexivity. Working through the various conceptualizations of reflexivity discussed above thus offers a useful analytical lens for better understanding central dilemmas in the doing, thinking and evaluation of qualitative research.

If the qualitative researcher only relegates reflexive analysis to particular points in the research process, one may miss the opportunity to challenge long-held myths about the complexity of thinking through and practicing qualitative methodology. For instance, Pamela Nilan (2002) divides the "implied subject positions" of the researcher into the binary of formal/informal positions, with the informal role sanctioned as a reflexive role in contrast with the formal objective role. However, the literature on reflexivity suggests that this division appears to be unnecessary and practically impossible to maintain in actually doing qualitative methodology. First of all, while research reports may seem to construct the division of formal and informal roles for the researcher, such reports are themselves constructed stories that may not include all of the "messiness" of doing the actual research. The write-up is not a mirror reflection of the experience in the field. As Donna Haraway (1988:576) has cautioned, a researcher's professed *ideology* of knowledge-production and the *practices* of actually going about obtaining information are not necessarily a match. So often in the discipline of sociology, the "messiness" that defines the conduct of the qualitative researcher is hidden from view – particularly in the teaching of methodology (Murray 2003). As such, to divide the roles of the researcher into formal/informal, objective/reflexive is to perpetuate the cordoning off of reflexive methods to particular stages of the research process. This prevents seeing the research process in its entirety as a potentially reflexive exercise – with both benefits reaped and dilemmas raised throughout.

One way to think about reflexivity as an ongoing process rather than as a single stage in the research process is to consider the ways in which reflexivity is a normal, everyday strategy for surviving everyday life (Glaser and Strauss 1967; Lynch 2000). By questioning our interpretations, we struggle along with the familiar and the unfamiliar and make choices about our actions accordingly, within the constraints of our settings. Becoming a reflexive qualitative researcher thus involves making "this *normal strategy* of reflective persons into a successful *research strategy*" (Glaser and Strauss 1967:227), with particular implications, as this paper has explored. We might also pay more attention to the reflexivity of our participants. We cannot imbue ourselves as researchers with the special ability to be reflexive, for this reproduces the divide between knowing researcher and known participant. From this perspective, Best fails to see her interviewees' reflexive attempts to deal with perceived race- and age-related language gaps. Rather, she interprets their techniques as "intuitively" translating (2003:907). In doing so, she misses an opportunity to understand her participants' actions as a reflexive social strategy – and she never admits that she may indeed have heard her participants wrong had it not been for their translations.

Attending to the reflexive strategies of our research participants expands the opportunity to consider the roles of multiple social actors in shaping the research experience. This notion can be expanded further to consider the responses to our research from colleagues and the broader academic community as a part of the reflexive process as well, in providing a dialogue around our findings and an assessment of our methodological approaches. It is perhaps only within this collaborative, dialogical process that we might assess the reflexivity of our research accounts: whether an explanation of context is relevant, whether contingencies have been sufficiently explicated, and whether our form of reflexive analysis has provided an insightful and useful contribution. All of which are concerns that have been raised as critiques against reflexivity as a "potentially endless" process (Lynch 2000:45). It may be important, therefore, for future analyses to consider reflexivity not as a solitary, individualistic process (nor the sole domain of the "expert" research-

er), but rather a process that is collaborative, interactive and inherently *social*: a collective effort (Bourdieu 2004).

Why does reflexive methodology matter for qualitative research? Why take up these dilemmas, when none of them are easily solved? Despite the impossibility for reflexivity to provide a universal cure-all for the dilemmas of conducting research, the importance of discussing reflexivity lies within its ability to bring methodological dilemmas to the forefront in the first place. The internal practices of qualitative methodology shape the subsequent political effects of the qualitative research we produce (Mykhalovskiy et al. 2008). A reflexive analysis, however it is that we may approach it in our methodology, can provide a starting point for thinking about the social process and consequences of our research practices.

ACKNOWLEDGEMENTS

I would like to thank the QSR editors and two anonymous reviewers who provided many helpful comments on earlier versions of this paper. I am especially indebted to Antony Puddephatt for his excellent editorial revisions, advice and cheerful encouragement.

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REFLEXIVE ACCOUNTS: AN INTERSECTIONAL APPROACH TO EXPLORING THE FLUIDITY OF INSIDER/OUTSIDER STATUS AND THE RESEARCHER'S IMPACT ON CULTURALLY SENSITIVE POST-POSITIVIST QUALITATIVE RESEARCH

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Abstract Reflexivity and acknowledging the role of the researcher in the research is a well-established practice in post-positivist research. In this paper we use reflexivity to examine our personal experiences in conducting qualitative research. We use reflexivity to understand how our intersecting identities and resulting insider/outsider status may have influenced the data collection phase of a study regarding the culturally and religiously sensitive issue of male-female intimate relationships. Using an intersectional approach, we explore the fluidity of our insider/outsider statuses resulting from our multiple and intersecting identities such as ethnicity, religion, age, and sex. The multiple identities a researcher possesses can cause him/her to be perceived as an insider and outsider simultaneously, which can play a significant role in shaping the interactions between the interviewer and interviewee. We present reflexive accounts on how our identities may have affected the data collection process and participants' comfort level when discussing sensitive issues, in this case sexuality. Overall, we seek to provide insight into the role of intersecting multiple identities and the resulting insider/outsider status in qualitative data collection when examining culturally and religiously sensitive issues from the perspective of the researchers.

Keywords Intersectional Approach; Insider/Outsider; Sensitive Research; Reflexivity; Qualitative Research; South Asian Youth

This paper advances the area of post-positivist qualitative research. First, our analysis challenges the dichotomous static division of insider/outsider status. Second, our method of inquiry takes on a unique approach by going beyond extant methodological scholarship and introducing an intersectional approach[1] to understanding the role of a researcher's multiple identities in shaping and negotiating insider/outsider status and, consequently, qualitative data collection. Taking an intersectional approach means recognizing that individuals possess multiple, intersecting, and inseparable identities that shape their lived experiences (Brah and Phoenix 2004), which can include identities such as gender, race, ethnicity, class, religion, and so on (Sokoloff and Dupont 2005). Within the research process, these identities are recognized as playing an integral role (Lumsden 2009). As Devine and Heath argue, researchers "cannot be divorced from their autobiographies and will bring their own values to the research" (1999 as cited in Lumsden 2009:503). How participants interact with the researcher is contingent on how the participants perceive the researcher

1 Intersectional approach is synonymous with intersectionality, a term frequently used in the literature by authors such as Crenshaw (1991), Davis (2008), Verloo (2006), Yuval-Davis (2006), and Zine (2008).

or the role they assign to the researcher (Walker 1998 as cited in Lumsden 2009). Thus, who the researcher is, as a person, and the identities that person has, are relevant in the research process (Moran-Ellis 1995 as cited in Lumsden 2009), and the data collection process in particular. This is especially so when using interviews as a data collection method because they are social encounters or "socially situated" activities (Fontana and Frey 2008:145).

To achieve our goal, we explore the literature on reflexivity in research. We then explain and problematize the dichotomy of insider/outsider status. To support our argument that insider/outsider status is not merely a dichotomy, we present an intersectional approach to address the complex nature of identities. We extend this methodological discussion by examining the fluidity of intersecting identities and the resulting researcher identities, namely insider/outsider status, within a qualitative study of a culturally and religiously sensitive topic. More specifically, we use an intersectional approach to examine our personal reflections on being insiders and outsiders simultaneously. We do so by drawing on and describing our experiences conducting interviews on the sensitive issue of sexual intimacy with second generation South Asian youth. Overall, this paper sheds light on the role of having multiple intersecting and inseparable identities in being an insider/outsider and how being an insider/outsider is neither a binary nor static status, but rather is constantly changing and negotiated depending on who is being interviewed.

LITERATURE REVIEW

Being mindful of the complex ways in which our identities impact data collection and analysis has significant implications for research practice. As Campbell has said, "researching the researcher…is a much needed new area of investigation" (2002:9). This is because "the biographical journeys of researchers greatly influence their values, their research questions, and the knowledge they construct" (Banks 1998:4). A post-positivist research framework allows for the penetration of researcher and research participant's subjectivities in the research process (Russell and Kelly 2002) to better understand the relationship between the knower, the known, and the process of knowing (Sprague 2005). Hawkesworth maintains that a researcher's epistemological stance to claim "superior knowledge" should be to accept "a minimalist standard of rationality that requires that belief is apportioned to evidence and that no assertion be immune for critical assessment" (1989:557 as cited in Sprague 2005:40). One way of doing this is to share reflexive accounts of research experiences to acknowledge and understand the role researchers play in the research process.

Reflexivity

Discussions of reflexive methodology began in the 1970s as a reaction to criticisms of classical, colonial ethnographic methods (Pillow 2003). As social sciences began to embrace qualitative methodology, there was a general consensus in qualitative research that reflexivity in methodological inquiry and understanding how "knowledge is acquired, organized, and interpreted" (Altheide and Johnson 1994:486 as cited in Mauthner and Doucet 2003:416; see Macbeth 2001; Kusow 2003) is critical and goes beyond "navel-gazing" (Sultana 2007:376). Many scholars over the years have made great efforts to define reflexivity, as well as explain how it is used in qualitative research, especially in immigrant communities (Kusow 2003; Hamdan 2009). While there is no universal definition of reflexivity, the literature offers various descriptions. For example, some argue that reflexivity focuses on the on-going dialogue of "how" and "what" I know (Hertz 1997 as cited in Pillow 2003:178). Here, the reflexive construction of knowledge is seen as a social process, which may not be so transparent (Dowling 2006; Riach 2009). Others define it as "a process of self-examination that is informed primarily by the thoughts and action of the researchers" (Porter 1993 as cited in Russell and Kelly 2002:2; see Barry et al. 1999). It has also been seen "as a challenge to commonsense worlds" (Gray 2008:936), critical awareness (McNay 2000), to perceive beyond one's self (Skeggs 2005), and to establish "how one is inserted in grids of power relations" (Sultana 2007:376). Being reflexive means recognizing a researcher's involvement and awareness throughout the research process, as well as giving careful consideration to their own assumptions such that meaningful analysis can be constructed (Barry et al. 1999; Watt 2007). It forces one to see beyond the unthinkable (Bourdieu and Wacquant 1992 as cited in Gray 2008) and "contributes to producing knowledge that aids in understanding and gaining insight into the working of our social world, but also provides insight on how this knowledge is produced" (Pillow 2003:178). Furthermore, reflexivity calls upon the researcher to realize that their interactions with participants alter the direction of the research from start to finish (Barry et al. 1999; Watt 2007). It also points to the importance of considering how the researcher is part of the data (Richards 2005; Watt 2007; Gray 2008). More specifically, we, as researchers, affect "the collection, selection, and interpretation of data" (Finlay 2002:531) and data analysis (Mauthner and Doucet 2005; Watt 2007; Gray 2008). According to Gray "the nature of the relationship between the researcher and the object of study has important affective dimensions with implications for research practice" (2008:936). The end product of reflexivity, according to Barry and her colleagues (1999) as well as Guillemin and Gillam (2004) is to enhance the quality and validity of data by expanding awareness and understanding of the social phenomenon under study, as well as knowing the limitations of knowledge production.

Insider and Outsider

In being reflexive, researchers consider their own role in the research process and one way this is done is by considering their insider/outsider status. There are advantages and disadvantages to data collection that go along with being an insider or an outsider. An insider is best defined as someone who shares similar characteristics, roles, and/or experiences with those being studied (Dwyer and Buckle 2009). Due to this, there is general agreement that if

you are an insider, you are able to easily establish rapport with your participants (O'Connor 2004) and have an enhanced understanding of your participants lived social realities (Dwyer and Buckle 2009). It is argued that an insider "can provide insights, inner meanings, and subjective dimensions that are likely to be overlooked by outsiders" (Hamnett et al. 1985:374 as cited in Hamdan 2009:381). Moreover, the participants may be more open with the researcher thus generating more data (Dwyer and Buckle 2009). Dwyer and Buckle (2009) argued that this is because the participants feel the researcher is one of them and therefore able to understand them. Furthermore, considering race specifically, Rhodes (1994) argues that there are aspects of racial experiences that a researcher of a different race may not have the language or cultural knowledge to understand. Thus, similar identities (i.e., being an insider) are thought to lead to more successful communication between a researcher and participant (Rhodes 1994), which may translate into more "authentic" accounts (Allen 2004).

Too much familiarity, however, may breed a lack of "interpretability" and "presumptions" that may not exist if the researchers are outsiders (O'Connor 2004:169). In other words, the researcher may presume to understand meanings that an outsider would further investigate (O'Connor 2004). Or, as Dwyer and Buckle (2009) suggest, some participants may not completely explain their experiences to an insider if the participants assume that researcher already understands. Furthermore, there is increased probability of role confusion on behalf of the researcher, which is when the researcher "responds to the participants or analyses the data from a perspective other than that of a researcher" (Asselin 2003 as cited in Dwyer and Buckle 2009:58). Another shortcoming of being an insider, especially within immigrant communities, is becoming a "suspicious insider" (Kusow 2003:595). This occurs when participants begin to question the researcher's true intentions (Kusow 2003). This complicates the research process further because an automatic divide between "community expectations" and "intellectual impulses" exists (Kusow 2003:594). Moreover, being an insider can blur the boundaries between the researcher and the research insofar as community members begin to advise the researcher about what to write and how (Kusow 2003). Community members can sometimes think that you, as a researcher, should only write about the *good* qualities of your people or present them in a positive light. Another issue is recruitment. While being an insider to the community provides access to places of interest and can help in developing a sample, issues of trust and rapport can, despite popular belief, remain problematic (Kusow 2003). Furthermore, when recruitment is successful and there is an interview, if the participant is still uncomfortable, responses may be limited to short one-word answers (Kusow 2003), thus lacking necessary descriptive accounts for an in-depth analysis.

Outsiders, on the other hand, are the opposite of insiders; they are researchers who are not seen as similar to their participants. It has been argued that this causes a lack of empathetic

understanding or, as Max Weber puts it, *verstehen* (Kusow 2003). Some suggest, however, that there are benefits to being an outsider. For example, an outsider "may achieve greater clarity in their work" (O'Connor 2004:169) because they will ask for further clarifications or details during the interview phase to ensure they are understanding and/or correctly interpreting what is being said to them. An insider, on the contrary, may assume to understand what the participants are saying because of the "shared" knowledge amongst them (O'Connor 2004:169). Furthermore, some have argued that outsiders are more objective as they do not have loyalties to the culture being studied (Banks 1998). This loyalty is believed by some scholars to be a "corrupting influence…upon the human understanding" (Merton 1972:19 as cited in Kusow 2003:592). For instance, Mullings pointed out "outsiders also argue that they are likely to have a greater degree of objectivity and ability to observe behaviors without distorting their meanings" (1999:340). In addition, Rhodes (1994) has suggested that outsiders have "stranger value," which can result in the researcher being given information that would have been presumed as understood by an insider. Sometimes an outsider status is preferential "as it is free from potential bias that arises from too close affiliation with research subjects or «going native»" (Allen 2004:15; see Kusow 2003).

Clear-cut insider/outsider dichotomies, however, tend to oversimplify the complexity of the researcher's identity. As Naples has stated, "the insider/outsider distinction masks the power differentials and experiential differences between the researcher and the researched…and creates a false separation that neglects the interactive processes" (1996:84). The post-positivist view of research, especially noted by feminist and critical scholars, is reassessing the epistemological and ontological implications of such binary divisions. There has been much effort to shifting modes of inquiry by deconstructing the traditionally rigid methodological beliefs and encouraging relationships that shape, define, and challenge the research experience. Scholars such as Hartstock, Haraway, Smith, and Hill Collins have embraced the reality of holding "multiple" or "plural" viewpoints by exploring the relational, as well as subjective nature of the research process via reflexivity (McDowell 1992; England 1994; Gilbert 1994; Archibald and Crnkovich 1995; Lawson 1995; Pratt and Hanson 1995 all as cited in Mullings 1999; Russell and Kelly 2002; Sprague 2005). For example, Naples suggested that "outsiderness and insiderness are not fixed or static positions; rather they are ever-shifting and permeable social locations that are differentially experienced and expressed by community members" (1996:84). More scholars are now recognizing and have clearly stated that it is next to impossible to have a fixed dichotomy of insider/outsider membership roles since these roles are fluid entities continuously being negotiated and re-negotiated during the interactive research process. Essentialist and reductionist claims of insider/outsider status are now being challenged (Rainbow 1977; Messerschmidt 1981; Clifford and Marcus 1986; Rosaldo 1989; Karim 1993; Naples 1996 all as cited in Sherif

2001; Dowling 2000 as cited in O'Connor 2004). For example, Mullings (1999) wrote:

> [t]he insider/outsider binary in reality is a boundary that is not only highly unstable but also one that ignores the dynamism of positionalities in time and through space. No individual can consistently remain an insider and few ever remain complete outsiders. Endeavors to be either one or the other reflect elements of the dualistic thinking that structures much of Western thought. (p. 340)

Extant research reveals cases of "partial" insiders (Sherif 2001). Moreover, these statuses, according to Kusow (2003), are more complex and dependent on the socio-political context and situation than is typically recognized and thus insider/outsider status cannot be completely isolated or reduced to a mere dichotomy. Being reflexive, as well as critical during the research process is imperative in negotiating and going beyond insider/outsider statuses (Hamdan 2009). Therefore, during the reflexive process, it is essential for researchers to think beyond a unilateral understanding of their insider/outsider status.

Intersectional Approach

Recognizing that the researcher is not confined to being an insider or an outsider is further justified when we acknowledge intersecting identities. Merton argued that "individuals have not one but multiple social statuses and group affiliations that interact to influence behaviour and perspectives" (1972 as cited in Banks 1998:7). Kusow's (2003) experiences studying his Somali community echo the need to recognize the researcher's multiple identities. During his research, he began to call attention to the impact of not only his ethnicity, but also his gender and how those two identities affected his data collection in different ways depending on the context. This supports the application of an intersectional approach in particular. Acknowledging an intersectional approach as part of the reflexive process encourages the researcher to consider individuals' multiple, intersecting, and inseparable identities that shape their lived experiences (Brah and Phoenix 2004). Moreover, the categories, or identities, we fall into are not static and distinct (Stanley and Slattery 2003). The various identities one can possess are countless and can include, but are not limited to, race, ethnicity, class, and religion (Sokoloff and Dupont 2005). These identities are synchronous and intersect with each other in unique ways (Joseph 2006), which, again, will affect individuals' experiences. While identities are inseparable, the prominence of each can change depending on the context or the situation the individual is in (Anderson and Hill Collins 2006). Even during an interview, the importance of various identities or status may change (Rhodes 1994). Here, issues of race, class, gender, occupation, age, power and other factors play a critical role in how one identifies and positions oneself within the dynamics of research (Banks 1998; Merriam et al. 2001; O'Connor 2004). Banks (1998) maintained that social status affiliations, like race and gender, interact in not only one's knowledge production, but also influences perceptions of reality.

Moreover, the participant plays an active role in defining the interview process (Padfield and Procter 1996). As such, the ways in which they perceive the researcher's identities will shape the interview experience.

Therefore, when turning to the researcher's roles, it is unreasonable to dichotomize insider/outsider status and think of them as exclusive since they are based on our numerous intersecting identities, which are inherently complex. Moreover, to be defined and positioned as a complete insider or outsider is unrealistic because "in the real world of data collection, there is a good bit of slippage and fluidity between these two states" (Merriam et al. 2001:405). This is further evident in Merton's argument that "we cannot permanently locate individuals according to a single social status. Rather they occupy a set of social statuses such that one individual can occupy an insider status at one moment and an outsider in another" (as cited in Kusow 2003:592). Accordingly, it becomes necessary to reconsider, reconstruct, and negotiate or even reject insider/outsider status as a dichotomy. Thus, as Dowling so eloquently puts it, the researcher is "never simply an insider or an outsider" (2000 as cited in O'Connor 2004:33).

CONTEXT: CULTURALLY AND RELIGIOUSLY SENSITIVE RESEARCH

Our reflexive accounts presented in this paper are drawn from our experiences as a South Asian female (Arshia Zaidi) and a Caucasian female (Amanda Couture) conducting 57 semi-structured qualitative interviews with South Asian students from the Greater Toronto Area (GTA) and Durham Region for a project funded by the Social Science and Humanities Research Council. The study explored cross-gender relationships (i.e., male-female interactions, such as dating and sexual encounters) amongst second generation South Asian youth. More specifically, the goal of the research was to not only uncover how second generation South Asians view and develop cross-gender relationships, but also to understand the sexual scripting that takes place within these relations. This involved asking very sensitive and personal questions about the participants' sexual experiences including what specific activities they engage in, where they engage in these activities, if they use protection, and so on.

According to Lee, a subject area is sensitive if it "poses a threat to those involved in it" (1993 as cited in Platzer and James 1997:627). This threat can result when there are "issues of social control over activities, which are stigmatizing or incriminating" (Lee 1993 as cited in Platzer and James 1997:627). This is applicable to issues of sexuality among South Asians. It is arguable that this research is sensitive as cultural and religious norms strongly discourage sexual intimacy prior to marriage and it is often considered a threat to their family honor and is shameful (Varghese and Jenkins 2009). Since it is considered a threat to the entire family's honor, the community and family, in particular, attempt to control and restrict it (Haddad, Smith and Moore 2006). Often daughters, especially Muslim daughters, are forbidden from dating

(Haddad, Smith and Moore 2006). As such, research regarding South Asian youths' sexuality is sensitive.

There are numerous challenges to conducting sensitive research, such as gaining access and establishing rapport (Platzer and James 1997; Dickson-Swift et al. 2007). Platzer and James (1997) suggest that their insider status, however, alleviated some of these challenges. This implies that the researcher's status plays a pivotal role in influencing the research process. In our study, due to the sensitive nature of sexuality for South Asian youth, establishing rapport and comfort were two critical challenges in particular that we had to manage. We recognized that it was very likely that our identities would influence the participants' perceptions of us, which would in turn impact their comfort as well as the data we would be able to collect. Taking an intersectional perspective, and recognizing that a researcher is never solely an insider or outsider, we determined it to be necessary for us to consider how our multiple identities, including our insider/outsider status may have shaped our data collection.

Prior to the data collection phase of this project, the research team had concerns, given the nature of the topic being studied, about how participants would respond to various interviewers. Thus, we decided to "test" multiple interviewers. Once the data collection began, it became seemingly obvious that our membership roles helped shape our research experience and define our status of being insider, outsider, or both simultaneously. We argue that this is because of our intersecting identities. Arshia is not just a South Asian person and Amanda is not just a Caucasian person, nor are we perceived just that way by our participants. This will become evident in the reflexive accounts that follow.

INSIDER/OUTSIDER ACCOUNTS OF DATA COLLECTION

To uncover how our intersecting identities led us to be both insiders and outsiders simultaneously, which then shaped the data collection experience, we will first begin with a brief overview of our intersecting identities (primarily the ones that are observable to others) and how those identities translated into our insider/outsider status. Then we will explore how we perceive those identities and the resulting insider/outsider status may have affected the interviewee/interviewer dynamics, including the researchers' interview style. While we are not able to concretely compare the interviews conducted by Arshia or Amanda since they are qualitative and we are unable to measure differences or discomfort, we use reflexive accounts to discuss how we perceived the overall encounter and our perceptions of the interviewees' discomfort, or our sense of their discomfort.

Arshia

Intersecting Identities and Insider/Outsider Status

I am a second generation Canadian South Asian female professor. Although being South Asian in general is a source of insider status, my family name can indicate to some that I am Pakistani. Since we interviewed a diverse group, participants or their families were from varying countries (e.g., Sri Lanka, Nepal, India, Pakistan, and Bangladesh). Being of Pakistani origin and having ties to Pakistan gave me insider status with other Pakistanis and outsider status with participants with other origins. My identity as a female was a source of both insider and outsider status depending on the sex of the participant being interviewed. My identity as a professor was solely a source of outsider status. Being labelled or seen as a "Dr." or "Professor" was in direct contrast to the undergraduate student status of most of the participants. This specific outsider status, at times, created a power differential from their perspective. This was especially true when they were in my classes as this heightened the power differential and added yet another layer to my outsider status. I was older than the students whom I was interviewing as well, making my age a source of outsider status. My identity as a Muslim was not necessarily known at the outset of an interview; however, as an interview would progress, the participant would become aware of it either by my religion coming up in discussion or based on the questions I asked or did not ask. For example, when I would ask Hindu participants about the particulars of their religion, they would realize that I am not Hindu as I would not need to ask such questions if I were. My religious identity would lead to me being an insider when interviewing other Muslims and an outsider when interviewing Hindus and Christians.

Interviewee/Interviewer Dynamics

Perceived Participants' Comfort Levels

In the beginning, being a visible minority South Asian caused the members of our research team to primarily consider me as an insider to our participants. Given this, we assumed that data collection would come easy for me. Early on in the interview phase, however, it became quite evident that it was not that simple. Being an insider in terms of my ethnicity and gender did have some advantages. However, it also led to some unexpected challenges or disadvantages due to the culturally and religiously sensitive data that was being collected.

Some of my experiences as an insider paralleled past research of others on insider status. For example, my insider identities made it easier for me to recruit participants (those who would feel more comfortable with someone from their ethnic community) as well as connect easily with some and establish rapport. With some participants there was a natural sense of empathy, belonging, and knowing that eased the conversation. In an interview with a female participant, I was able to build rapport by talking about things we both experienced growing up. For instance, I explained, "I was raised and born in Canada and assimilation was always a problem…I hated high school… My whole high school experience, I hated it because to fit in you had to do certain things that weren't

Islamically allowed…" While discussing this, the female participant kept responding "exactly" indicating an immediate understanding of shared experiences, which was used to build comfort. When I asked a female participant about specific sexual activities and prompted her by listing kissing, holding hands, and hugging, she responded, "OK, well I guess if you consider that…it's like the Brown version [of sexual activities]." Together we laughed as this was a mutual understanding; I knew exactly what she meant. Later, when describing her sexual experiences she said, "it wasn't like Caucasian, like White serious." This was not likely a description she would have used with a Caucasian interviewer, but she felt that I would understand what she meant.

Participants also expressed their comfort more directly. For example, one female participant said, "I feel so comfortable talking with you; I feel you are able to understand where I am coming from." Another female participant said, "it is so cool you are South Asian and fit well with our generation." Yet, another said, "you are South Asian and all and just perfect to be interviewed by...I got no reservations." One female participant in particular mentioned feeling at ease during the interview and stated, "it is very cool that you are doing this research and it makes it so so much easier to discuss the matter with you."

There were, however, notable disadvantages to being an insider in terms of ethnicity. At times I felt this acted as a deterrent. It seemed as though participants would answer in a socially desirable manner, especially regarding issues of sexuality. There appeared to be some fear or shame in telling me, a fellow South Asian, their stories. There were also times when they seemed suspicious of me. Consistent with Kusow's observations, there were some moments of awkwardness or silent moments that made me feel like a "suspicious insider" (2003:594). At times it felt as if there was a gap left in the interview and I was not, despite my persistence and efforts, able to capture the "real" story. In some interviews I yearned for more information, but was met with shallow answers. For instance, when I would ask participants about their sexual experiences, some would respond indirectly or vaguely by calling it "being physical." When I asked one female participant in particular about the details of her sexual activities (e.g., what specific activities and where), she responded, "do I have to answer that?" She also kept mentioning, without prompting, that her relationship was not all about sex, that it was a balanced relationship and it was "mostly like kissing." This gave the impression that it was important for her to let me know that sex was not a priority in her relationship, perhaps because she feared my judgment as a fellow South Asian and possibly assuming I share a similar belief system regarding sexuality as other South Asians. Another female participant started off being open about her sexual activities, but once we started discussing her pregnancy scare, she commented, "this is awkward." Moreover, none of the participants went into detail by naming the sexual activities, such as oral sex or touching. As a researcher, this was a major

setback for the study and it became understood that sameness of ethnicity or shared community ties does not always work in one's favor as there may be traces of participants answering in a socially desirable manner. Other times, potential Muslim participants would ask me "how this study makes any positive contribution" to specifically Islam, or they had concerns of me writing about Islam in a negative manner. There were also times when very conservative potential male participants would shun my research and say it is "haram," or completely forbidden by Allah (God), and the result would be no interview. Thus, the challenges of being an insider were greater than expected.

While the majority of participants I interviewed did express some discomfort in discussing their sexual experiences and required additional prompting to get more details, there was one male participant in particular who seemed very comfortable. When I asked this participant what kinds of sexual activities he engaged in, he responded, "like positions? (laughter) I'm just joking." Then, without prompting, he explained how in class that very day he had a conversation with a girl that turned him on "so much."

My insider status as a woman also created a few challenges. There were some female participants who would not provide detailed accounts and/or explanations, as they would assume that certain things are just understood because of my gender and South Asian identities. Many times female participants would respond with "you know how it is" and not provide the thick description that a qualitative researcher thrives for. For example, one female participant was using a "Brown" soap opera to describe her father's behavior and she did not offer a detailed explanation of this because she knew that I would know what type of television show she was referring to. Also, when interviewing male participants there were, at times, a sense of awkwardness and silent moments, especially when issues of sexuality would come up. This lack of comfort was most evident with religiously conservative participants.

Although it was assumed that I was primarily an insider, as the interviews progressed we realized that some of my identities also simultaneously made me an outsider, which also had its difficulties. As a professor and a person older than the participants, I was an outsider and I was able to sense this during my interviews. My academic identity was one that I felt especially inhibited the interview process and this was specifically mentioned by some participants. I sensed that my academic status led to a power differential during some of the interviews and, at times, there would be continuous negotiation of power. Some people clearly stated that they were not comfortable being interviewed by me given my professorial identity and others said they would like to be interviewed by someone else. For instance, a male participant stated his discomfort and said, "I am not really comfortable in discussing my dating experiences with you being a professor... you know...can we move to the next questions please." A female participant also expressed her uneasiness with a professor conducting the

interview on such a personal issue and stated, "it's just hard 'cause you're a professor."

Interviewer Comfort Levels

In addition to the participants' comfort levels, we also had to manage our own comfort issues. Within South Asian cultures, including mine, issues regarding intimacy and male-female relationships are never discussed directly. Intimacy is viewed as a private matter that is not meant to be openly discussed. For instance, when parents and children watch television shows, if an intimate scene appears, the channel is changed immediately and there is no discussion of what took place. Although many of us are rarely specifically told that sex is bad, it is just "known." This coupled with South Asian cultures' shame-orientation along with my own gendered and racial socialization has shaped my own comfort with the subject matter. As a South Asian woman, the participants will have their own expectations of me and I also need to be respectful of the culture, since I am considered a member of their cultural community. To maintain my comfort and to be respectful, I did not directly ask about especially sensitive sexual activities, such as oral sex. I would limit myself to asking about kissing, hugging, and holding hands. I also used colloquialisms, such as asking them details about "the bed thing" rather than saying "sex" specifically to improve the participant's comfort as well as my own. When it came to issues of familial conflict, however, I was comfortable asking them directly if their parents hit them. For me, this was not a sensitive issue because there are different disciplinary styles of parenting, and generally within South Asian cultures, the family is more authoritative and patriarchal, with hitting often being an accepted form of discipline. As a fellow South Asian, there was not a fear of stigmatization.

Amanda

Intersecting Identities and Insider/Outsider Status

I am a relatively young Canadian-born Caucasian female graduate student. Being Canadian-born is an identity that I shared with some of the participants, which could have been a source of insider status. However, my Caucasian identity was something that made me an outsider. Similar to Arshia, being female made me both an insider and an outsider depending on the sex of the participant I was interviewing. My age and student status were two identities that I shared with the participants and, therefore, were sources of insider status. While my religious identity as a Christian was not something that could be determined solely by looking at me, the participants likely assumed that I was not Muslim or Hindu given my ethnic identity. As such, my religious identity made me an outsider with the Hindu and Muslim participants, but an insider with the Christian participants.

Interviewee/Interviewer Dynamics

Perceived Participants' Comfort Levels

My ethnicity, or my "whiteness," was something we originally assumed made me solely an outsider with our participants. Being an outsider in terms of my ethnicity had both advantages and disadvantages. I am not nearly as knowledgeable of the South Asian culture as Arshia is because I have never lived the experience of it. Since it was assumed that I was not well versed in the culture or religions, participants often provided great detail when explaining norms, customs, and experiences. For instance, a male participant went into an elaborate description of the arranged marriage process, which he likely would not have done had Arshia conducted the interview. He stated:

> ...there's a common belief that, you know, just blindly jump into it…but contrary to that, uh, it's actually both sides get equal say believe it or not, like a lot of people have this, uh, idea that the woman doesn't get to say anything; they just have to do it…it's not like that at all actually. The female actually does…have some say…first, what they'll do is they'll meet up, the families will meet up, they'll talk and then they'll give the male and female some time.

He then goes into even more detail about the role of Hinduism, including palm readings and astrology. It seemed that he felt the need to clarify the arranged marriage process to me because he thought I might hold that common belief. If Arshia interviewed him, he may have left it unsaid since she is South Asian and would likely be aware of the realities of arranged marriages.

There were times, however, especially with a few female participants, when the interviewees did not seem to be completely comfortable talking with me. It felt as if there was something separating us. While this was not something always verbally expressed, it was the impression I was given during the interview. Some were very brief in their responses and seemed like they were not at ease. A female participant who did specifically mention her discomfort said, "yeah, sorry. This is really embarrassing." It is difficult, however, to establish if this discomfort was because of my ethnicity or for other reasons such as the general personal nature of the topic.

On the other hand, there were also participants who did not show this discomfort. For example, when only asked generally what types of sexual activities she engages in, a female participant responded, "um, I would say oral." Unlike some other participants, she volunteered this information without specifically being asked if she engaged in oral sex. Another female participant, who is a lesbian, went into great detail about not only the specific activities, but also how she felt during those experiences without being specifically prompted. She said:

> ...oral sex is a big thing. Oral sex is probably the biggest… It's like kissing a female it's a lot more passionate…just having sex with a female it can get to that real deep level…I've been sexually active with a girl who…I hardly knew…we just really had this huge attraction…and just kind of let it happen and it was really, really passionate strong sex… It's all about the intimacy, it's really romantic being with a girl…like with a guy it's, a kind of like, in and out like with his penis.

Some participants specifically mentioned that me being an outsider in terms of my ethnicity was a comforting factor. One participant said that me being "white" could make it easier for South Asians to talk to about sexuality because, he joked, "the white girl has probably seen it all." This implies that some perceived that nothing about their sexual experiences would surprise me or, more importantly, cause me to judge them. A participant specifically mentioned this after an interview as well. He said:

> South Asians might be more comfortable talking to you because they won't feel like you're judging them because you don't really understand the significance of this. A South Asian, however, could be thinking "Well, I grew up here and didn't do those things."

When I tried to make another male participant feel more at ease, I explained that I had probably heard it before and he responded, "oh, I know."

Similar to Arshia, my identity as a female was another source of insider/outsider status. Although my identity as a female contributed to my outsider status when I interviewed the male participants, I rarely felt that it was an inhibiting factor. Although, technically an outsider in this regard, it did not appear to have a negative impact during the interviews. For instance, one male participant was so open with me that when I asked what sexual activities he engages in, he responded, "there's not much we don't engage in…everything, but anal sex I guess." Another male participant was somewhat explicit when he explained to me what he meant by "almost having sex." He was the only male participant who stated something so directly and said that it is "just not actually putting it in I guess." While there were some male participants who did express some embarrassment and awkwardness when starting to discuss their sexual experiences, they would still continue and often become more comfortable. Others, however, specifically stated they did not feel uncomfortable with me. For instance, a male participant hesitated slightly when asked if he was a virgin and said, "no one's ever asked me that before... oh no, no, no, I'm not uncomfortable. I've just never had anyone ask me that and then I just admitted it right away (laughter)." When asked if he was uncomfortable with answering a question about his sexual experiences, another male participant stated, "it's OK. I'm fine with anything." There were two male participants who actually enjoyed the interview and went so far as to tell me that it felt like a kind of therapy. For example, one said, "God it's like a therapy session…it's amazing." Interestingly, although the research team considered that my gender identity could lead to men exaggerating their sexual experiences, it was not something that I ever perceived during the interviews.

My age and student status were both identities that contributed to my insider status with the participants. However, since we possess multiple inseparable identities, it is difficult, if not impossible, to distinguish whether someone felt more comfortable with me because I was an outsider in terms of ethnicity, making them less fearful of me judging them or breaching confidentiality to their community, or if they were more comfortable because I was an insider based on my age and student status, which may have made me more relatable.

Interviewer Comfort Levels

During my early interviews, I was a little hesitant when asking about sexual experiences. I knew that within South Asian cultures sexual intimacy is a very private matter. As a result, I was unsure of how the participants would receive me and I wanted to be sure that I was approaching the topic in a sensitive way. I was concerned with potentially disrespecting someone or making them feel uncomfortable. Moreover, I was also a little uneasy about how the participants would perceive me in general. I worried that they would question my motives and be thinking "Why is this «white» girl interested in my sexual experiences?" I soon discovered, however, that this was more in my head rather than how the participants actually felt. Nobody questioned my motives and sometimes when I would discuss the project as well as my own Master of Art's thesis on dating abuse from the perspective of South Asian Muslims, some would explicitly state that they were glad that I took an interest and was conducting research in the area. As I continued with the interviews, my uneasiness subsided. Furthermore, in mainstream Western society sex is not generally a taboo. For example, on primetime television it is easy to come across individuals engaging in sexual relationships; on magazine covers in the grocery store we see "101 ways to please your partner;" our public education system offers sex education; and so on. Within my own family, I recall my mother always telling me, "if you ever think about having sex, please come talk to me first." While she did not encourage it, she did not forbid it outright either or suggest that I would make the family look bad. She was more concerned with my own emotional well-being as well as my physical safety. As a result, I do not generally feel uncomfortable discussing sexual intimacy and thus, was not overly uncomfortable asking participants about their sexual activities in a blunt manner. Being respectful, I would directly ask them if they engaged in oral sex or touching, or if they were virgins. However, I was not comfortable asking specifically about anal sex. This could be because this sexual activity is still fairly taboo in Western culture.

While I generally did not feel uncomfortable asking questions about sexual experiences, I was more hesitant to ask about issues of familial conflict, and more specifically physical discipline or abuse. Within mainstream Western society, physical forms of punishment have become stigmatized. I personally perceive experiences with parental abuse to be a sensitive topic. During my interviews with participants, I was not at ease asking participants about such issues directly. While Arshia was able to ask questions about this directly and bluntly, I did not typically feel comfortable addressing the issue if the participant did not discuss it voluntarily, especially because it was not the main research purpose. I felt by asking such questions I would be intruding into a sensitive area without a reason. Due to stigmatization surrounding corporal punishment within main-

stream Western culture, I was unsure of how the participants would react to me asking such questions. I was also concerned that they might think that I am assuming that South Asian parents are all abusive. In both the areas of sexual experiences and familial conflict/abuse, my own comfort levels also shaped the interview experience. While Arshia and I, as well as many other researchers, try to remain as objective as possible and simply ask the interview questions and elicit in-depth responses, we are still human and our comfort levels and the expectations we believe others have of us impact how we communicate with participants, especially how or if we ask particular questions.

CONCLUSION

Our reflexive accounts as insiders and outsiders forces one to think beyond the insider/outsider dichotomy. Our insider and outsider statuses were fluid as they shifted depending on how we were perceived by the participants. Moreover, it is evident that our identities played a role in the participants' comfort with us when discussing the sensitive topic of sexual intimacy. Our identities also influenced our own comfort during the interviews. This level of comfort directly speaks to how we, the researchers, have been socialized. Ethnic, racial, and gender socialization affect the outcomes of these discussions (Brown, Linver and Evans 2010). Thus, the intersections of our ethnicity, race, gender, and so on and our socialization experiences all influence the nature of our discussions with participants, our analyses of the data, and even our own reflexive accounts.

This work demonstrates that one is never just an insider or an outsider. It speaks to our inseparable intersecting identities and insider/outsider statuses as it was impossible to determine which identities or statuses changed the dynamics of the interview. Thus, it is the unique combinations of our identities and how those identities are perceived and brought to the forefront by the participants, which influence not only the collection of sensitive data, but also the quality of data received from each participant. Overall, taking an intersectional approach recognizes that we never just have one identity and thus allows us to appreciate how our identities make it possible for us to be an insider and outsider simultaneously.

While it is impossible to fully tease out the ways in which our multiple intersecting identities impact data collection, the value of taking a reflexive, intersectional approach lies in helping to better approximate the ways in which our various identities combine to influence the process. To involve the participants more directly in this reflexive process, by having them comment on the impact of our identities and perceived insiderness/outsiderness, would further help to shed light on the ways in which the characteristics attributed to the researcher impact data collection. In this regard, a limitation of the current study is that we did not specifically elicit the participants' thoughts on the interview process or how they felt about us conducting the interviews. It would be useful for future work regarding sensitive issues to have a follow-up component to explore the participants' perception of the experience including how they feel about who interviewed them.

This article establishes that it is valuable to consider how certain statuses shape interactions, experiences, and conversations with participants, especially regarding culturally and religiously sensitive topics linked to dating and sexuality. We have attempted to show the implications of intersecting identities on insider/outsider status and in turn the implications of insider/outsider status on the data collection process. This supports the "critical importance of a researcher's cultural and sociological position" (Gadamer 1975 as cited in Hamdan 2009:380). Moreover, we argue that it is necessary to think beyond the single identities we assume to be most important in determining insider/outsider status and the assumption that insider/outsider status is a distinct binary. This is evident in our assumption that Amanda was more of an outsider because more weight was assigned to her ethnicity and that this status may cause problems in the interviews. The data would have been limited had this dissuaded the research team from allowing her to do the interviews. Therefore, it is important to recognize the merits and demerits of our varying intersecting identities and with related insider/outsider status to maximize its utility to benefit the quality of the qualitative research process.

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BOOK REVIEW:

LONDON, JEFFREY MATTHEW. 2009. *HOW THE USE OF MARIJUANA WAS CRIMINALIZED AND MEDICALIZED.* LEWINSTON, NY: EDWIN MELLEN PRESS

The debate on the use of marijuana between those who focus their activity on contributing to the philosophy of liberalization and the representatives of "hard line" fighting for prohibition is still popular not only in the United States, but also across European countries. Both sides used to employ arguments that are more or less rational, but catchy; trying to win a broader support for their particular goals. In many countries, we can observe social movements, usually gathering young activists who popularize the idea of open access to marijuana for personal use. In Poland, one of the newly born liberal parties – Palikot's Movement[1] – even introduced the proposal of the liberalization of the use of marijuana, one of their official points in their political program.[2] It has opened the public discussion and divided society into declared supporters and opponents who used to perceive the role of government and internal politics in a different way. The dilemma is, to some extent, similar to the one described by Erich Fromm in his book *Escape from freedom* (1941) (also known as *Fear from freedom*). Adjusting Fromm's dilemma, there is the question: what should be the proportion between security dimension understood as prohibition of using marijuana and freedom of choice with all its consequences? There is a question of citizens' freedom and its limitations rooted in democratic standards.

Looking at the latest history of Western countries, we can assume that using marijuana has become an inseparable part of culture. This is the reason why so much academic attention is paid to this social phenomenon. In the late 60's and 70's marijuana was the symbol of alternative culture gathering young Americans fighting for peace against the war in Vietnam and other significant places. Smoking marijuana became a way to symbolize the peaceful philosophy linked with hippies who denied the official establishment and "from the bottom" were trying to build an alternative society based on *making love not war*. The song *Imagine* by John Lennon had become, in the 70's, one of the most popular songs directing the way of thinking represented by youth not only in the United States, but also in Iron Curtain countries. Across decades, the public disputes and opinions have changed many times, but the way of perceiving marijuana in terms of its criminalization and medicalization is still up-to-date. Jeffrey Matthew London was trying to describe the tendencies mentioned above by showing the changes in the main players' approaches to using marijuana through the decades. It is difficult not to agree with the words of Robert Regoli that "what makes this book exceptional is that [the author] provides a thorough qualitative historical analysis of marijuana's past and present social constructs" (Foreword:i). London successfully depicted "the way a plant is transformed into a crime, and how a crime is transformed into a medicine" (Foreword:i). According to his thesis, the criminalization process of using marijuana started in the early 1900's when the first government officials' efforts were focused "to define marijuana use as a criminal problem" leading to its eradication from public disputes (p. 1). London points out that in recent times we experience the opposite process of marijuana medicalization initiated by non-profit grassroots organizations. These attempts to redefine marijuana are focused on changing the public perception and, as a result, decriminalize using it within a specific frame. The two mentioned powers also propose a different way of describing marijuana users naming them *criminals* or *patients*. These two approaches also lead the way in the treatment of smokers by punishing them or curing them.

Surely, the discussed tendencies are divided into interest groups competing to win social support for their purposes by imposing their interpretations and the language used to discuss this dilemma. Both sides, in the process of mutual negotiations within countless discussions, have changed the model of debates and language. They have been paying more and more attention to make their arguments sound scientific and, as a result, rational for a broader public opinion. More and more, the science representatives were engaged to provide some proof used by lobbying groups to convince the public opinion and win with the opponents' argumentation. The mentioned processes, tendencies and changing social backgrounds can be found in this book.

Social scientists are interested in many aspects (political, cultural, social, legal, etc.) connected with smoking marijuana. Using marijuana also stays in the direct field of interest of sociology, especially the sociology of deviance and labeling theories that are based on medicalization theories as pointed out by London. Probably the most popular sociological text touching this problem is *Becoming a marihuana user* by Howard Becker (1953). Becker, adopting an interpre-

1 Original name: Ruch Palikota.

2 In the project of the change of the bill of drug addiction prevention we can read that those who possess small doses of marijuana will not be investigated by the police and prosecuted (see http://www.ruchpalikota.org.pl/sites/default/files/projekt_ustawy_-_o_przeciwdzialaniu_narkomanii.pdf, retrieved March 22, 2012).

tative perspective, investigates some social contexts and mutual interactions lying under the process of becoming a marijuana smoker and being labeled by the social environment as some kind of outsider. Jeffrey London also describes the problem of using marijuana in terms of deviance, but adopting a historical perspective. It enables him to follow changes taking place throughout decades. "The book examines how the label of deviant applies to marijuana users over the last 100 years" (p. 2). The reader is introduced to looking at this problem as a process that is based on the struggle between different, sometimes opposite, forces (medicalization and criminalization) that used to play important roles in creating a legal establishment. The *status quo* is changing in relation to the influence of supporters.

The reviewed book consists of six chapters encompassing presentation of theories used for analyses and some different aspects and processes based upon the medicalization and criminalization of marijuana usage. The division of chapters used in this work seems to be clear and logical, making the book easy to read. The reader has the possibility to look at the discussed problems in chronological order.

In the first chapter, *Introduction*, the author introduces the used methods of data collection and interpretation. Investigating the circumstances influencing the legal limitations of using marijuana, London bases his research on federal and state legislative acts that caused criminalization and decriminalization of marijuana to be identified both with crime and medical use. Moreover, there are also analyses of Court cases. London also presents and characterizes many documents influencing public debate and legal establishment in this area (Supreme Court cases, legislative records and transcripts, major newspaper articles, congressional debates, speeches, etc.). The material seems to be rich enough to make some conclusions helpful in understanding these two opposite processes influencing the discussed issues. Doing research and interpreting data, London decided to use three analytic perspectives: Peter Conrad and Joseph Schneider's theory to study the medicalization of deviance, Michael Foucault's five principles of the "Science of Discipline" and Elliott Currie's macro-level tenets to explain the institutionalization of deviant designations. The mentioned concepts "are connected to form a framework for studying deviance designation change" on a micro-, mid- and macro-level (p. 5).

The next chapter – *The three stages of Deviant Designation* – encompasses Conrad and Schneider's, Foucault's and Currie's approaches to show the process of deviance labeling from three levels (a micro-, mid- and macro-perspective). As a result, the built *three-stage theory* is used to examine the entire processes of deviant designation. In my opinion, the application of these theoretical perspectives rooted in an interpretative standpoint is useful for describing the elements of medicalization and criminalization. It is worth saying that the author not only operates with the mentioned theories, skillfully trying to combine them into one analytical frame, but also proposes his own interpretations in presenting the processes of deviance designation.

In chapter three, *Kendall and Wickham's Method*, we can find the way of interpreting historical materials adopted by Jeffrey London. "Kendall and Wickham's method instructs to view history as an action and not simply an existing record of unquestionable interpretations of the past...[This method] guides researchers not in what we look for but how to look" (p. 27). According to London, this method of qualitative analysis is especially applicable when there is a need to manage a huge amount of documentation, both historical and contemporary. The author argues that Kendall and Wickham's method, linked with Foucault's philosophy of describing reality, enables or even emphasizes the researcher's freedom in terms of skeptical interpretation of gathered documents (*Problematise history, spot contingencies, be skeptical of all political arguments* and *Suspending second-order judgments*). All the methodological hints characterized by London and enriched by examples direct a researcher both toward criticism and sensitivity with new evidences appearing and the interaction between them. In the next parts of the book the author applies them to analyze the presented problems.

In chapter four, *Archeology of Marijuana Criminalization*, the author uses Conrad and Schneider's theory to investigate the discursive mechanisms lying under the process of marijuana criminalization. London focused on "the way by which power and knowledge work together in an alliance through language and material actions to accomplish specific goals" that result in building the system of citizen control and even more (p. 51). The used methodological and theoretical perspective allows to follow changes across decades. It enables us to investigate connections including the mutual interactions and influences of the main players that lobbied for marijuana criminalization. Based on eight major legislative events, there are also other mechanisms of influence presented that cause the transformation of social perception relating to using marijuana. As London wrote, "this chapter focuses on the rapid shift in systems of thought about marijuana" (p. 53).

Chapter five – *A Genealogy of Marijuana Medicalization* – encompasses the opposite mechanisms that balance and neutralize the influence of the previously described tendencies by changing the social perception of marijuana and, as a result, the legal environment. Here, there are discussed state laws introduced in nine states in the US (Alaska, California, Colorado, Hawaii, Maine, Nevada, Oregon, Vermont and Washington). The author adopts Foucaultian genealogy as an analytical tool to present and interpret collected data. In this part he also distinguishes the main players and lobbyists who introduce the medical marijuana social movement to public discourse. As a result, those actions changed the public thinking about marijuana being transformed from crime to medicine. There is also visible the author's attempt to give information useful for predicting the direction of discourse in the future. London argues that "studying these tactics is important because these tactics involve changing the way that people talk, think, and act toward marijuana today, as well as how people may talk, think, or act toward one another tomorrow" (p. 93-94).

The last chapter contains *Conclusions* based on different kinds of data analyzed especially in chapter four and five. The main conclusion is that the public perception of marijuana and the processes of deviance designation have been changing dramatically over the period of one century. Over the years, there appeared a lot of conditions that played more or less important roles in shaping the public perception and consciousness relating to marijuana. As a result, the limits of prohibition and the reasons lying under it also have changed.

In my opinion, the main advantage of reading this book is the knowledge that could be applicable in understanding the public debates, voices of scientific authorities and "moral entrepreneurs" influencing legal limitations concerning using marijuana in other countries. This book puts more light on the interest group activities oriented on reaching the specific goals through discourses applied to convince the public opinion. The reality described by Jeffry London is flowing under the direction of lobbying main players.

Even if we take into account that London's analyses and interpretations are limited to America's reality that bases on different past and contemporary cultural conditions, philosophy of democracy and public opinion, other than the European understanding of free will, we can draw some interesting conclusions. These conclusions refer to the mechanisms playing important roles in shaping people's perception of disputable issues present in public life.

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Volume VIII ~ Issue 1
April 30, 2012

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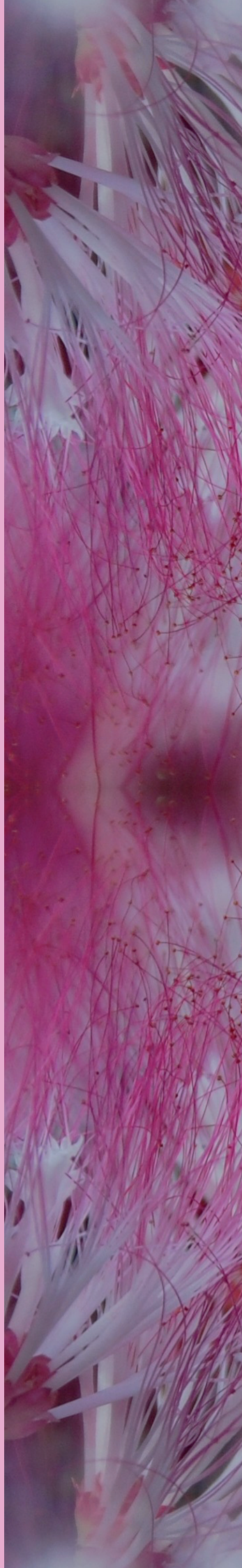
ISSN: 1733-8077