Patricia A. Adler
University of Colorado, U.S.A.

Peter Adler
University of Denver, U.S.A.

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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 cyberethnography 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.

email address: adler@colorado.edu

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.

email address: socyprof@hotmail.com

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 surprise, 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-history 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-determined 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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