

Into the Dark Heart of Ethnography: The Lived Ethics and Inequality of Intimate Field Relationships

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Abstract In response to critiques from feminist, existential, and postmodern qualitative researchers, the idea of maintaining objective and distant relationships with research subjects gave way to the belief that researchers could and, in some cases, should become intimately connected to research participants. These traditions opened the door for contemporary field workers to unapologetically forge close relationships to setting members. Several ethical evaluations have emerged from this intimate literature warning researchers of the harm that can come when we “go to far” in the quest for intimate familiarity. In this paper, I reflect on some of the debates regarding intimacy and exploitation by examining my experiences of dating, marrying, and eventually divorcing my key informant. I trace the way that, despite my attempts to follow the existing ethical guides, I reinforced several larger inequalities in my intimate stance. Using my failure to avoid or mitigate harm, I argue that our discussions of intimate methods and immersion in the field have failed to accurately note how we reinforce or resist structure in our research endeavors. Viewing ourselves as “doing structure” in the field would lead us to stop debating whether intimacy is better than objectivity, celibacy is better than sex, disclosure is better than silence, or conventional behavior is better than deviance in the field. Instead, we should locate how our behaviors, research roles, or discursive choices enact structures and the effect this enactment has on the people who we research.

Keywords Intimate methods · Emotions and fieldwork · Experiential immersion · Research ethics · Vulnerable populations

Introduction

By the end of the 1990s, the feminist, existential, and postmodern research traditions had lifted the veil of objectivity shrouding the scientific method. Not only did these paradigms

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provide a boost to qualitative sociology, but the ensuing celebration of the subjective nature of research inspired a collection of personal methodological tales focusing attention on researchers' sexual (Bolton, 1991, 1995, 1996; Cesara, 1982; Davis, 1986; Goode, 1999, 2002; Johnson, 1975; Kulick & Willson, 1995; Lee, 1978, 1979; Murray, 1996; Palson & Palson, 1972; Rabinow, 1977; Schneebaum, 1969; Stewart, 1972; Styles, 1979; Turnbull, 1986; Van Lieshout, 1995; Wade, 1993), loving (Blackwood, 1995; Gearing, 1995; Newton, 1993), or emotional (Ellis & Flaherty, 1992; Gubrium & Holstein, 1997) immersion in the field. Although late twentieth-century qualitative researchers were always encouraged to gain "intimate familiarity" with their subjects (Lofland & Lofland, 1995), what makes these contemporary treatments of field intimacy different is that intimate encounters are no longer merely interesting methodological side-notes. They are now central topics of investigation.

In the late 1990s, I began to pay special heed to the emerging intimate ethnographic tradition. The reasons for my interest in this type of ethnography were personal. Fascinated with the topic of deviance and, especially, deviant occupations, I had begun researching my boyfriend's tattoo shop, called the Blue Mosque, and realized immediately that being in love with my key informant—who eventually became my husband during the course of my study—would influence almost every aspect of my research and personal life. Initially, I felt encouraged by the intimate trend in ethnography and believed that my relationship would be seen as a sign of my emotional immersion in and deep commitment to the setting. I was, however, concerned about research ethics and began to scour the intimate methods literature in search of useful guides to help me to avoid harming field members.

Although I delighted in the increasing acceptance of intimate stances in the field and closely followed the ethical guidelines governing intimacy, in the end, I realized that my research role opened the door for painful power imbalances that held tragic implications for my husband, Lefty Blue,¹ and me—even more tragic than the consequences associated with remaining distant, unemotional, and objective. This led me to return to some of the models regarding intimacy in the field. While I initially agreed with most of the methodological claims, I found when my marriage and research ended that harm and injustice operated quite differently from what had been suggested.

In the following sections of the paper, I review the claims for and ethical warnings within the intimate research tradition. Examining this literature closely, I argue that the literature has focused most of its attention on the micro-contexts in which exploitation and harm occur, at the expense of locating our ethical discussions in a larger structural context. By structure, I mean the properties encouraging social practices (especially inequalities) to become patterned and systematic over time and space.² In the next section of the paper, I

¹ All the names of people and places (except for Hawai'i) in this paper are pseudonyms.

² I am drawing very specifically from Giddens' (1984) structuration theory in this definition rather than from classic ethnomethodological treatments of structure, despite the fact that ethnomethodologists are often credited with advancing the "doing structure" concept. I prefer Giddens' definition because it evokes the many discursive *and* non-discursive ways that systematic inequalities among and between actors (say between a man and woman in a particular context) and collectives (say between deviant subcultures and conventional society) are enacted or resisted. In addition, Giddens' definition of structure, which revolves around the *properties* making regularized practices possible, focuses attention on the rules and resources that underlie and are activated during interactions. Therefore, when a heavily tattooed person is spat at in public or denied service in a restaurant, this is evidence of an often unspoken, tacit rule that is being evoked and sustained. In addition, it also serves as a moment when power enters the scene by influencing actors' access to resources (status and/or material).

explore the ups and downs of my journey through my research and close relationships and describe the ways that I tried to use the ethical guides. As I noted, to avoid exploitation, I used several interpersonal strategies that did very little, in the end, to help me avoid hurting and damaging my husband. In fact, despite my best intentions, I found that my intimate field stance actually reinforced several inequalities and, when we tried to resist them, we were crushed by our efforts. Finally, I look back at the end of the research and my marriage and conclude that the ethical guides and debates surrounding subjectivity, intimacy, and immersion have focused on the micro-contexts of exploitation and harm. They have failed to trace the way that we enact structure in the field and how our “doing structure” negatively or positively affects setting members.

Intimacy and ethics

To date, intimacy in field methods has been supported by three paradigms: the interpretive, feminist, and postmodern. Eschewing objective, distant, and emotionally detached research stances for slightly different reasons, each of these traditions has opened the door for researchers to forge and disclose deeply personal, close, and emotional field work encounters. Emerging from this call to intimacy has been a set of ethical debates highlighting the problems associated with personal and emotional immersion in the field. Interestingly, the ethical writings have primarily focused scholarly attention on the individual behaviors and choices in the field. The result has been a growing literature on the micro-politics of research ethics that focuses our attention on a litany of minor research decisions at the expense of understanding, articulating, and pointing out the structural sources and processes of inequality and harm.

Within the contemporary interpretive paradigm, researchers are encouraged to get close, go in deep, and participate intimately in the life world. Wacquant (2004, p. viii) dubbed this type of work “carnal sociology” in which ethnographers “submit to the fire of action *in situ*” and experience the taste, ache, and action of their settings. That authors contributing to the July 2005, *Qualitative Sociology Symposium* have nearly universally applauded Wacquant’s (2004) call to moral and sensual immersion in the field provides further evidence of a distinct methodological moment in sociology. To throw one’s self into the field, *body and soul*, is now not only a valid research stance, but marks investigatory excellence. Complete bodily and emotional immersion, however, has not always been celebrated. First and Second Chicago School ethnographers often attempted to avoid “over-rapport” and bias by striking a balance between empathetic participation and complete engagement with field members. The case for intimate involvement came in the late 1960s and early 1970s when existentialist ethnographers argued that complete immersion is necessary to penetrate fronts (Goffman, 1959), to dive underneath the surface of accounts (Douglas & Johnson, 1977), and to truly understand the world as an insider experiences it. Over time, instead of being critiqued for their over-involvement, researchers who became deeply immersed in their settings were praised for having better data and much more complex and sophisticated renderings of their subjects.³ Probably the most forceful contemporary statement for complete immersion in the field is Ferrell and Hamm’s *Ethnography at the Edge* (1998a, 1998b) in which they argue

³ Duneier’s (1999) *Sidewalk*, for example, has been hailed as “intensely personal” (Manning, 2001, p. 15) and Fine (2004, p. 506) writes of Wacquant’s (2004) *Body and Soul* that “few other field essays so instantly immerse us into social (and economic) worlds as hauntingly and eloquently as this.”

that sharing in the “pleasures and dangers” of a setting can render a more accurate picture of the lived realities in which crime and deviance take place.⁴

Despite the celebration of participation in the pleasure and dangers of the field, a few scholars have argued that ethnographers can go too far in the quest for intimate familiarity. For example, Goode’s (1999, 2002) accounts of sex with informants have touched off a collection of ethical warnings. The arguments against Goode included that researchers can turn subjects into sexual (or emotional) targets or prey (Saguy, 2002), mislead subjects regarding research and emotional intentions (Saguy, 2002; Williams, 2002), and fail to recognize the power imbalances between researcher and subject⁵ (Bell, 2002; Williams, 2002). Furthermore, Bryant (1999) and Manning (2002) argue that Goode’s intimate disclosures are gratuitous and spurious and fail to say anything about the dimensions and features of the study population (Bryant, 1999; Manning, 2002).⁶

Another argument for intimacy in the field, predominantly espoused within feminist research, is that emotional connection is less exploitative of research participants than an objective stance. In her classic work, Oakley (1981) argued that remaining detached and distant while research subjects bared their souls underscored and perpetuated inequalities between researchers and subjects. According to this perspective, subjective, intimate, and emotionally close relationships in the field, at least on the surface, promised to correct the inequalities embedded in the masculine scientific tradition by emphasizing everything that had been suppressed and devalued in patriarchal divisions between objectivity/subjectivity, distance/intimacy, and rationality/emotionality. Following these critiques, it became common for feminist researchers to argue for emotional connection (Ribbens, 1989) and deep commitment to field members (see Acker, Barry, & Esseveld, 1996; Cotterill, 1992; Finch, 1984; Gorelick, 1991; Kirsch, 1999; Patai, 1991).

Despite the call for “no intimacy without reciprocity” (Oakley, 1981), many feminist researchers note that intimacy is not a panacea for exploitation. For example, Stacey (1988, p. 23) argued that “fieldwork represents an intrusion and intervention into a system of relationships” that, although sometimes satisfying, work to benefit the researcher much more than the participant. Stacey (1988); Finch (1984); Acker et al. (1983); Cotterill (1992); Gorelick (1991); Kirsch (1999); and Patai (1991) have all expressed feelings of “inauthenticity” in their research relationships and have noted that friendships and friendliness can be false and easily manipulated to hide the true goal of the relationship: to obtain rich data. Some have also noted that the researcher’s ability to leave the field marks an additional inequality in research relationships. On this subject, Stacey (1988, p. 26) argues that “beneficiaries of such attention may also come to depend upon it, and this suggests another ethical quandary in fieldwork, the potential for, indeed the likelihood of, desertion by the researcher.”

Supporting intimacy in the field from a different angle, postmodernists have argued that intimate disclosures can change and challenge colonizing research traditions. Viewing ethnographers as historically located and their texts as “constructed, artificial. . . cultural accounts”

⁴ For their intimate engagement, authors in Farrell and Hamm’s collection have been praised as being “intrepid brave souls” (Adler & Adler, 1998b).

⁵ Bell (2002) argues that because researchers have more power than research subjects and men have more power than women, that male researchers having sex with female informants is unethical.

⁶ Ferrell and Hamm (1998b) also highlight research ethics, which they call “lived politics,” of full immersion in settings. They (Ferrell & Hamm, 1998b, pp. 7–8) argue that despite personal and professional “thrill-seeking” satisfaction for the researcher, some research topics (especially deviant and criminal ones) put researchers at risk for unusual trouble with gatekeepers, stigmatization, physical danger, moral compromise, and self doubt. While this is true, in this paper I want to focus on the effect of experiential immersion—in my case, intimate relationships—on research participants.

(Clifford, 1986, p. 3), postmodern, post-structural, and postcolonial researchers have viewed the distant, objective ethnographic voice of traditional narratives as a fiction “based on systematic, and contestable, exclusions” (Clifford, 1986, p. 6). Because researchers’ emotions, desires, and intimate relationships in the field are among the many “exclusions” from the ethnographic canon, focusing on silenced or taboo subjects repositions the ethnographer and ethnography in the production of knowledge. For example, Kulick (1995, p. 4) uses this very argument to support an emerging methodology of desire and argues that writing about researchers’ intimate behaviors in the field alters the “unidirectional discourse about the sexuality of the people we study.”

The postmodern call for alternative discourses has launched an obsession with writing form. If traditional ethnographic texts have silenced, colonized, and misrepresented the people who we study, then the key has been to craft a different narrative style. For example, proponents of such experimental or “new” ethnography as novels, short stories, poetry, letters, speeches, films, plays, or blended accounts, argue that these non-traditional narratives liberate social science from homophobic traditions (Clough, 1994), move to center stage silenced or marginalized voices (Rose, 1990, 1991), decrease barriers between researchers and subjects (Finn, 1995; Gordon, 1995), reach—and potentially mobilize—larger audiences (Behar, 1995), or represent multiple voices and perspectives (Denzin, 1997; Kondo, 1990; Trinh, 1989).⁷ Ronai (1995, p. 423) argued that narrative form and content is no minor consideration when she equated traditional, detached, and objective writing—the kind of writing that reviewers asked her to produce—to an “abusive patriarch who demands the silence of his children.”

Taken in combination, the interpretive, feminist, and postmodern traditions have seemed to argue that intimate methods, and especially our accounts of intimacy in the field, can be more accurate, less exploitative, and less “colonizing” than objective and distant methods. This would imply that concerns about inequality, power, and social location bolster the intimate research tradition. On the topic of ethics, however, each of these three paradigms has become excessively bogged down in micro-concerns and has failed to link the everyday research experience to larger practices, relationships, and inequalities. For example, according to these ethical guides, if researchers want to mitigate exploitation and harm they should avoid indiscriminate sex with subjects (especially those who are less powerful than researchers), acknowledge that their relationships with informants might be manipulative, fully disclose research and emotional intentions to informants, and not write sexy, violent, voyeuristic, or self-obsessed narratives that do more to pique lurid attention than to meaningfully describe research settings. Researchers should also consider the writing process carefully and find ways to write against the traditional ethnographic canon.

In the end, the ethical problems and solutions within each of these three paradigms are pitched at the level of: (1) how to behave when researchers are in the field and (2) how to write about the field. Although these debates give a nod to structural power (they acknowledge that there are power distinctions between researchers and subjects and that researchers, informants, and discourses are situated within power relations), they fail to meaningfully analyze how researchers’ behaviors and writing reinforce, enact, or resist these power relations and distinctions. Moreover, they present a static image of ethics as a behavioral formula in which certain choices (i.e. having casual sex with informants,

⁷ Feminist researchers have also been concerned about writing form and have made similar claims that non-traditional social science writing is better able to represent multiple, often marginalized, perspectives, reach and mobilize large audiences, and be personally liberating (Anzaldúa & Keating, 2002; Collins, 1991; Moraga & Anzaldúa, 2002).

making research goals a priority over relationships, and producing traditional academic discourse) are viewed as always and everywhere problematic. By failing to view all of our research behaviors as engaging in structure, these ethical warnings fail to see inequality and exploitation as dynamic forces that can occur regardless of whether we follow the ethical formulas and codes. More than this, they mislead us regarding the way that inequality, harm, and exploitation function in the lived research experience and, in so doing, distract us with a litany of minor concerns.

On the methodological edge

Fieldwork, in many respects, has been considered exceedingly edgy. As outsiders angling for insider knowledge, professionals dependent on personal relationships for data, and members of research settings as well as the academy, field researchers ride the lines between and across multiple boundaries, and the journey, as many have attested, can be emotionally and existentially uncomfortable. Researchers give many names to this brand of discomfort. Some have discussed experiencing culture shock, while others have written about marginality, self-doubt, or just plain insecurity. Ferrell and Hamm (1998a) add another dimension to the concept of field research as “edge work” by arguing that criminal or deviant settings force researchers to grapple with the lines between legality and illegality, discredit and legitimacy, and morality and debauchery. The resulting emotions, according to Ferrell and Hamm (1998b, p. 2), are adrenaline-infused tangles of “. . . pleasure, excitement, and fear.”

My experiences in researching the world of professional tattooing resonated with these accounts. I experienced marginality, conflicting loyalty pulls, professional and personal angst, moments of intense pleasure and joy, as well as devastating bouts of self-doubt and failure. Although these were the daily obsessions of my research, they did not comprise the forces that ultimately mattered. The most salient aspect of my intimate research stance was the institutionalized divisions and inequalities between my husband’s deviant subculture and my world of professional academics and the way we reinforced and attempted to resist them. Although I imagined that loving and marrying a member of the field would create a bridge between these very different worlds, in the end I was wrong and we were both devastated by our attempts to gain a permanent place in each other’s lives and settings. This is where the lived politics of research, especially research in deviant or criminal settings, becomes unambiguous. Becoming intimately close to setting members can do more harm than good.

Falling into the field

My first introduction to the Blue Mosque was in the spring of 1996, during my fourth semester of graduate school. I took a break from studying to accompany my friends as they received their first tattoos. I was immediately infatuated with the clean, comfortable shop, the different types of tattoo clients, and especially with the five Blue Mosque tattooists: Lorna, Mark, John, Thomas, and Lefty. All the tattooists were white, from middle-class backgrounds, in their mid- to late twenties, and were interested in breaking conventional norms. The most blatant evidence of their taste for the outlandish was their appearance. Covered from neck to toe⁸ in bright, big tattoos and sporting multiple body piercings, including lip, nose, tongue, and eyebrow rings, they were, quite literally, a colorful cast of characters. The dramatic image

⁸ None of the artists had face tattoos.

that they projected—an image that conventional individuals might describe as alarming and freakish—expressed their lifelong commitment to living outside of mainstream America. While my friends found the foray into the tattooists' world interesting, the experience struck me more deeply. By the end of the tattoo sessions, I was smitten with Lefty and his shop. In the next few months, I returned to the Blue Mosque several times and became friends with the tattooists. Eventually, Lefty and I began dating.

My introduction to hardcore tattooing (i.e., the world of professional tattoo artists and tattoo collectors who were covering their bodies with tattoos) then, began as a personal pursuit, as I have presented it here and elsewhere. Although I did not contemplate studying this world during my first few months of dating Lefty, I was a sociology graduate student and could not help but “analyze” this community. Lefty listened to my reflections about his world and my descriptions of the deviance literature, and offered his own analysis of the structure and organizational features of the hardcore tattoo subculture. Having read the work of Sanders (1989), a sociologist and tattoo collector, Lefty was familiar with field work methods and the writing and analysis produced by these endeavors. It is not surprising, given this, that during one of our conversations about the tattoo industry and lifestyle, Lefty suggested that someone should conduct a study to note the many changes that had occurred in tattooing since Sanders' work. I agreed, but did not take up the task until I enrolled in a graduate field methods course and needed to find a setting to study for the year.

Initially, the project seemed exciting and my role as a girlfriend offered some advantages. Echoing the call to start where you are and gain intimate familiarity (Lofland & Lofland, 1995), my advisor encouraged me to use my intimate connection with this world as an advantage to access and immersion in the setting. I also quickly noted that, unlike other ethnographers (see Mitchell, 1991), I enjoyed this setting and liked, even loved, the people in it, which seemed to be a boon, given the discrediting of fieldworkers who loathe the populations they study. In our early conversations about the research, Lefty offered some provocative insights to convince me to take on this project. He pointed out that numerous journalists and researchers were investigating tattooing and its increasing popularity, and, in his view, were offering derogatory and inaccurate depictions (for exceptions, see DeMello, 1993; Vail, 1999a, 1999b). Here, we both imagined that the research might be beneficial. I assumed that I could use the research to demonstrate that the heavily-tattooed were not the dangerous and exotic other, and Lefty felt that his profession might gain more conventional credibility and legitimacy as a result of the work.

Once I had taken Lefty's suggestion seriously, I became very uneasy about the project, and the readings I completed for my methods course that semester only increased my anxieties. One concern that quickly came to the fore was maintaining access to data. Having met several tattooists' ex-girlfriends, I realized that I was hinging my research entirely on a potentially unstable union. This troubled me. What concerned me more than the stories of failed fieldwork projects was harming study members whom I believed to be a vulnerable group. I made this vulnerability assumption after witnessing occasions when Lefty or our friends were sneered at, refused service in restaurants, followed by suspicious security guards in stores, verbally insulted, or spat upon by individuals who viewed people who had covered their bodies with tattoos as extremely threatening and dangerous. The fact that I could roam freely and without comment in conventional settings suggested to me that I had more power than this heavily tattooed crew. Moreover, the fact that I was engaging in a sexual and romantic relationship with a field member—a topic that was gaining increasing attention at the time—meant that I needed to tread very carefully.

Hoping to avoid preying upon or misleading research subjects, I continually communicated to Lefty my dedication to our relationship and informed Lefty and our closest friends about my research progress. This meant more than going covert about researching tattooing, and led me to explain what topics I was exploring in my field and analytical notes, discuss what avenues I was thinking of pursuing for papers, and share my fledgling theoretical ideas with the group. Despite this, waves of guilt and anguish came over me each night as I went home to write field notes. I perpetually felt like a spy and worried that I was gaining much more from this group than they were going to receive from my research. The only thing that I could offer was my friendship, admiration, and the promise that the product of my work might help lend some legitimacy and understanding to the world of hardcore tattooing.⁹

Although I felt that I needed to acknowledge the power differences between the heavily tattooed and myself and to make sure that I was not taking advantage of setting members, I found that this power was a bit more elusive and fluid during my everyday interactions in the field. Unlike heavily tattooed members of the study, I never attracted instant suspicion or invasive amazement by strangers. In contrast, strangers routinely, and without invitation, would reach out to touch tattooees' exposed skin or lift their clothing to get a good look at a large tattoo. This did not mean, however, that I was always and everywhere more powerful than Lefty or other members of the study. Without many tattoos (I acquired two relatively small tattoos during the research), I was seen as having a low commitment to the subculture. My credibility, it was clear, came from my association with Lefty. Certainly many ethnographers find this type of dependence upon informants for data to be unsettling. This dependence was double-edged for me because my low status as a tattoo novice was coupled with larger gender inequalities within and surrounding the community (see Warren, 1988).

Although there were more female tattooists at the time of my study than in any previous era, tattooing was a male-dominated profession. Through male networks, men were able to learn such trade information as machine mechanics, needle construction, and tattoo application more easily than women (Mifflin, 1997). On some occasions, I witnessed tattooists and hardcore tattoo collectors discussing "tattoo babes," women who were both heavily tattooed and at the same time matched white, middle-class beauty standards (thin, clear skin, petite features). Tattoo magazines, which usually placed on their covers scantily clad and tattooed women in sexually provocative poses, reinforced the idea that women were tokens and sexual objects in the community. By initiating my research as "Lefty's girlfriend," rather than entering the field as an anonymous researcher, I would never be outside the gender hierarchies in the community. I would be judged like any other girlfriend in the setting. This was the uncomfortable bargain I would have to make for my intimate type of research.

From the outset of this research, I experienced multiple anxieties. I worried about taking advantage of my boyfriend and new friends by misrepresenting them and offering them very little in return for their intimate confidences. As time progressed, I also became concerned about the gender inequalities in the community. More than observing these dynamics as an outsider, I felt limited by them daily. Although, I initially believed that researchers have more power than research subjects, being immersed in the lived realities of the setting often made me feel very powerless and helpless. Numerous ethical concerns during the early stages of research preoccupied me with undertaking a litany of tasks to protect against harm.

⁹ This made me even more conscious of my writing. I wanted desperately to avoid offering another voyeuristic installment of what Liazos (1972) called the "nuts and sluts" brand of deviance research.

Ironically, while these ethical strategies were designed to overcome inequality, hierarchy, and exploitation, they ultimately dragged me down a fruitless path.

Falling out of the field

Aware that any small, personal infractions or misunderstandings can blow up into major breaches in trust, ethnographers use many techniques to maintain positive relations, including providing services for others (Adler, 1993), manipulating their appearance and demeanor (Van Maanen, 1991), and forging close ties with key setting members (Liebow, 1967). However, relationships cool, trust is broken, and sometimes researchers are exiled from the field. The nature of field relationships, especially the sometimes daunting call to remain non-exploitative (see Reinharz, 1993), coupled with the general volatility of romance, made the girlfriend role a precarious one and carried numerous pressures for both Lefty and me. These and other pressures eventually ended our relationship and the girlfriend role for a short time.

The girlfriend role not only made me preoccupied with being a “non-exploitative” researcher, my research also unduly burdened Lefty with a score of obligations to our relationship and my work. When we first met and began dating, Lefty had been separated from his wife for one year. At the time, Lefty explained that he and his wife were on their way to an amicable divorce and had worked through their grief. Despite these early assurances, he was overcome with feelings of emotional failure during the first year of our relationship. The prospect of committing to me and risking failure in yet another relationship proved debilitating and all-consuming. Making our relationship more stressful, he felt he could not break up with me because he had promised to assist my research. By November of 1996, Lefty and I had become a particularly tense couple. I spent most of my time trying to craft an ideal research project—that would not harm or misrepresent study members—he spent most of his time grieving for his failed marriage and trying desperately not to “let me down.” After several tear-filled conversations, Lefty and I separated. My worst fears were realized. I had lost my relationship, my friendships, and my research. I had failed.

Surprisingly, the breakup was a relief. Exiled from my research, I stopped worrying about exploiting others, spying on friends, fighting injustices, opening avenues for emancipation, or being relegated to the uncomfortable role of the traditional tattooist’s girlfriend. I realized that by keeping my feelings and behaviors under meticulous scrutiny and holding myself to many lofty research demands, I had become preoccupied with the research (especially the harms that can come from it) and had made it my first priority. I had stopped living my life. As an ex-girlfriend and an ex-researcher, I was free from the burden of research and all of its obligations and responsibilities.

To my surprise, slipping into the ex-girlfriend status did not segregate me from the other tattooists. Lorna included me in social events at the shop. During this time, I ignored any research and focused exclusively on my friendships, and, in return, I received considerable support and kindness from this group. A few months after Lefty’s and my breakup, I settled into a routine of occasionally going out to dinner, movies, and musical shows with Lorna, Lefty, and the other tattooists.

As soon as I ended my research and all of its dilemmas, solidified my friendships with the tattooists, and repaired relations with Lefty, I began to witness increasingly tense interactions among the tattooists. Growing frustrated with the town around the Blue Mosque and hungry for new challenges, Lorna, Thomas, and John talked about leaving the Blue Mosque to open their own shop in another part of the country. Worried that he would have to close the shop, Lefty expressed considerable anxiety at the prospect of losing these artists. As their

departure date approached, the conflicts within this group mounted and I often found myself torn between what had become two factions in the shop: those who wanted to leave the Blue Mosque and those who wanted to stay.

Although frustrated by these loyalty pulls, I was delighted to discover that these interpersonal antagonisms had nothing to do with me or my research. I felt no pressure to analyze the scenarios, apply theories, or to sociologically understand the unfolding events. Furthermore, I felt no obligation to correct any harms. It was a comfort to take the position of supportive friend and let events naturally evolve. To me, this felt “authentic.” Later, when writing about this phase of the research, I wondered if this was the type of “authenticity” discussed by feminist researchers (Acker et al., 1996; Cotterill, 1992; Finch, 1984; Gorelick, 1991; Kirsch, 1999; Patai, 1991; Stacey, 1988) who described feeling torn between their commitment to research participants and their research goals.

At this stage, I began to question the nature of harm and inequality and redrafted, although tenuously, my relationship to and responsibility for exploitation. The schism in the shop also made me abandon my obsession with situational ethics. I realized that by attempting to meticulously apply the many ethical codes in the literature, and to control, correct, and fight every injustice, I had crafted an unnatural, unrealistic research project and relationships. Because I believed that researchers usually have more power than research participants, I felt that I had more responsibility to overcome injustices. In essence, the ethical advice I gleaned from the literature did not assist my efforts and became a burdensome and long list of correct and incorrect behaviors. By being a friend rather than a researcher, I stopped focusing on the ethical codes and responsibilities that accompany research. Giving up the research opened the door for deeper and more intense commitments to the group.

Making up

Having weathered the ups and downs of field relationships, many researchers have reported feeling as close as kin to study members during the latter stages of their fieldwork. Some researchers were adopted by families in the field and a few, like me, married setting members (Gearing, 1995; Lois, 2003). Forging deep and sometimes even permanent ties to the field introduces a complex set of considerations to fieldwork, including negotiating spousal roles in the field (Adler, 1993; Adler & Adler, 1991, 1998a, 2004; Corbin & Corbin, 1984; Oboler, 1986; Scheper-Hughes, 1992; Schrijvers, 1993; Vera-Sanso, 1993; Wolf, 1992), dealing with pressures to “go native,” and negotiating the end of research (Gallmeier, 1991; Snow, 1980).

After Lorna, Thomas, and John moved, I continued my friendship with Lefty and Mark (who remained at the shop). Instead of closing the Blue Mosque, Lefty recruited two new tattooists, Francis and Rubin, whose arrival marked the beginning of new friendships and a new era for the shop. In fact, there were many new beginnings at that time. Without the pressures of my research and having recovered from his divorce, Lefty felt free to pursue our relationship again. Although I was initially reluctant, I gave our romance another chance. In fact, Lefty and I remained committed to each other throughout my graduate school years.

After two years of dating, Lefty and I married. The wedding itself was a special, intimate occasion attended by our parents, siblings, and Blue Mosque tattooists and regular clients. Further cementing my connection to the tattoo world, Mark moved in with us. This time in all of our lives was accompanied by a frenzy of tattoo activity. The three of us frequently traveled to tattoo conventions around the country and Europe and, when we were not on the road, several tattooists from around the country stayed with us while they worked “guest spots” at the Blue Mosque. Our apartment, located a few blocks from the Blue Mosque,

became the after-hours meeting location where tattooists and regular clients wound down from difficult tattoo sessions.

When Lefty and I reunited, I had completed my third year of graduate school and, according to my advisor who kept an eye on my progress through the department, I was falling behind in my dissertation work. To help me catch up, she encouraged me to meet with her during her office hours to discuss research progress. I felt conflicted. I did not want to turn my private life into a research topic again, nor did I want to become enmeshed in any other setting. In essence, I had become completely uninterested in the research and had focused primarily on being a good-faith member (see Adler & Adler, 1987) of the Blue Mosque. I conducted no interviews and was uninterested in examining or negotiating “my research role” and saw my meetings with my advisor as a chance to chat about interesting aspects of my life. This satisfied her for a short time.

When I entered my fourth year of graduate school, my advisor warned me that I needed to progress in my dissertation or risk failure in the program. I discussed my concerns with Lefty, Mark, and our closest friends. I explained my dilemma and worries regarding exploitation, misrepresentation, and the heavy burdens of research. My husband and friends, however, felt that I was being overly sensitive and cautious. They also offered to help me as much as they could if I took up the work again. As in my first formal research attempt, I drew from feminist research perspectives (Acker et al., 1996; DeVault, 1990; Mies, 1993; Smith, 1974, 1987; Stanley & Wise, 1983) by viewing Lefty and our friends as research collaborators. I talked openly about the status of my writing, checked my thoughts and ideas with Lefty and our friends, and invited everyone to read my writing. In fact, I often left my unfinished papers out around the apartment for anyone to read. There were also several moments when I became stuck while writing and ventured out to the living room with my portable computer in hand, asking Lefty, Mark, and other tattooists and collectors visiting our house to help me. I eagerly took down what they offered, changed what I was writing, and read back to them the final product. Using these methods, I completed my dissertation and published two papers on the topic of tattooing.

Formal research progressed much more smoothly after my marriage than before. After marriage, I no longer felt like a spy, worried about misrepresenting or misinterpreting the tattoo world, or about harming members of my study. I would like to say that this smoother course was because of all the steps I took to avoid harm, including collaborating with study members, opening my work to their critiques, and acknowledging power differences. The changes, however, came because of marriage, which altered how I related to many traditional expectations and relationships inside of and outside of the community.¹⁰ Certainly, marriage signaled to me and to the setting members that I was committed to the world of tattooing in ways that went beyond my research. I no longer felt or worried about coming off as an outsider reporting on the exotic and strange lives of hardcore tattooists and collectors. Also, Lefty and I conformed to many larger gender expectations that are traditionally reinforced through marriage. Lefty played the part of the careerist husband and, by following his profession around the globe and doting on his business associates and friends, I performed the part of dutiful wife, eager and happy to support my husband’s career. That I made his world the center of my writing further reinforced traditional gender relationships. Moreover, my graduate student status allowed me the flexibility to do this and shielded me from

¹⁰ Marriage solved some of my initial qualms about the girlfriend role. Because marriage was a well respected institution within the tattooing subculture, wives were not evaluated by the same criteria as girlfriends. Setting members rarely discussed wives’ physical appearance or their sexual attractiveness, which was a significant relief to me.

full professional academic expectations. Similarly, the research also reinforced traditional ethnographic relationships where Lefty served as the ideal research subject. He introduced me to key players in the community, helped me to set up interviews, and completely opened himself and his world to the researcher's gaze. He was my subject, morning, noon, and night, and because we were in love, he did not mind. In this way, my research goals were served very well. That I was uncomfortable with this arrangement at times was beside the point. My marriage allowed me unprecedented access and immersion into a world that few other conventional individuals could ever penetrate.

Endings

The ability to leave the field and return to the everyday academic world is often viewed as a significant power distinction between researchers and subjects (see Gottfried, 1996, p. 15; Kirsch, 1999; Stacey, 1988). According to this perspective, setting members become dependent on researchers and are hurt when abandoned, and researchers, in turn, find themselves able to escape the exigencies and problems plaguing everyday research settings. My research experiences suggest that the end of research does, indeed, mark significant structural distinctions between researchers and setting members. The real risk, however, comes not through abandonment, but through the reverse—when researchers and setting members attempt to carry on close relationships after the end of fieldwork. This is the moment when they fight against and resist the most pernicious and divisive forces. It is also when, as was true in my case, they can be devastated by their efforts.

The nature of my research and my status as a graduate student allowed and even required that I leave academia and cross over to another world for a short time. Researchers have long been allowed to “research down,” without permanent damage to their identities and reputations. Therefore, while engaging in research, my marriage to Lefty allowed me to demonstrate my commitment to the tattoo community and feel better about my work. Marriage also forced Lefty to enter my world in ways that were very problematic for him. The pain, anguish, and loneliness he experienced when trying to enter my world rivaled the culture shock accounts of any ethnographer. He was chided, looked down upon, and marginalized during many interactions within academia. My closest friends in graduate school, including my advisor and her family, certainly fell in love, as I did, with Lefty and our friends, but other professional colleagues kept a chilly distance.

One day I realized how awkward Lefty felt at my university. After walking down the hallway to meet me in my office, Lefty arrived there rather frazzled and angry and noted that as he passed by a group of my colleagues, they immediately stopped their conversation and stared as he approached them. The talking did not resume until he was out of sight. When Lefty explained his discomfort, I attempted to argue that he was being overly sensitive and that my colleagues meant no harm. When he commented that he had “felt their hatred,” I stopped arguing. Having traveled with Lefty and other heavily tattooed individuals extensively, I had witnessed several incidents of silent hostility directed toward us but I would be hard pressed to prove that this hostility existed. I just felt and knew it. The disdain was palpable behind individuals' hardened stares and whispers. In addition, I was told by one professor, after bringing Lefty to an American Sociological Association conference in Chicago, that I should not be seen with Lefty in professional circles. The gist of this comment, I assumed, was that being seen with someone covered with many visible tattoos would make me look unprofessional by association.

These initial encounters made Lefty very reluctant to spend any time around the university or academia. This was easy to manage when I was in graduate school. At that time, my

school work became the excuse for me to retreat into my husband's world. Once the research ended, Lefty and I took on academic institutions and gender expectations in a new way. The ultimate struggle came when I completed my dissertation and attempted to find a job. I applied for positions near our home for a few years and encountered no luck. Eventually, I broadened my employment search and finally received an offer from the University of Hawai'i, located thousands of miles from the Blue Mosque. Here, I had two difficult choices: give up my aspirations of becoming a professor or move away and risk losing my marriage and friendships.

After many discussions, compromises, and negotiations, Lefty and I attempted an equitable solution that we saw as "undoing" traditional gender inequalities between husband and wife. We were to move to Hawai'i and return to the Blue Mosque during every academic vacation. Therefore, the Blue Mosque would stay open, Lefty could tattoo for part of the year, and I would retain my professorship. Unfortunately, from the outset, the move proved to be exceptionally taxing for Lefty, as it uprooted him from his financial and social support networks. Where he had enjoyed being the center of a deviant and colorful social circle at the Blue Mosque, he found himself friendless and jobless in Hawai'i. Given the 15 to 4 ratio of male to female professors in my department, faculty husbands were few and far between.

Although I was also friendless, this social isolation did not matter significantly to me because, as a new assistant professor, I had more than enough work to keep me occupied. In fact, I rarely had time in my new schedule to tackle many domestic tasks. Lefty, with nothing but time, found himself adopting another role—househusband—and, although he initially expressed delight in challenging traditional gender roles by being the supportive man behind the career woman, eventually this dependent and isolating role grated on him. He became increasingly agitated and depressed and frequently commented that he "was no longer the man I married" and that he was "losing himself." One day, as I was cleaning our bedroom, I found the following poem Lefty had written on the plane to Hawai'i:

three thousand miles
 I tell myself stories
 higher and higher
 the piles on piles
 and three thousand miles
 to get to the bottom
 and come to the conclusion
 smiles all smiles
 and three thousand miles
 such a small distance
 to be with my lover
 and feel like a child
 again and again
 over and over.¹¹

Two years into my assistant professorship, Lefty announced that he could not live in Hawai'i. Our flirtation with riding the lines between our worlds had come to an end and we were forced to choose between our careers and our relationship. Like many academic women (see Landau, 1994), I confronted the difficult career/family dilemma.¹² My intimate research stance added additional complexities to this traditional dilemma and placed me amid and,

¹¹ Lefty has granted written permission for me to publish this poem as part of this paper and has read this paper several times.

¹² In fact, as Dugger (2001, p. 132) found, the percentage of female faculty with tenure decreased from 24 percent to 20 percent between 1977 to 1995.

ultimately, unable to fight multiple inequalities. If I left academia, I would be perpetuating the unequal ratio of male to female faculty members, abandoning many female graduate students who had few female mentors, and acting as a poor role model by demonstrating that you have to give up your career to have a family. If I stayed in Hawai'i, on the other hand, I would be turning my back not only on my husband but also on the very people whose lives, intimacy, and experiences had helped to advance my career.

More important than all of my concerns, feelings, and experiences was the effect of our choice on Lefty. Because he had first hand experience with the “househusband” role, Lefty did not want me to give up my career and face the same childlike dependence. Having been covered from neck to toe with tattoos for all of his adult life, Lefty was often second-guessed, questioned, and forced to prove himself as a very smart, talented, and responsible professional to many individuals who saw heavily tattooed people as degenerates and social misfits. Therefore, Lefty understood how important it was to me to advance professionally in a world that often questions and devalues women. As a result, he did not want to collude with dominant gender expectations by asking me to give up my career for a family. In fact, he refused to let me make this choice. In many ways, I feel that Lefty made the ultimate sacrifice for me. He chose to lose the marriage over asking me to leave my job. We filed for a divorce during the second year of my assistant professorship.¹³

Despite the fact that I walked away from my marriage and fieldwork with considerably more than I feel that I could ever give back to the community, Lefty remains far from bitter about my study, this paper, or the end of our marriage. In fact, he has continued to support me and our friendship unconditionally and, from thousands of miles away, I continue to support and cherish him and our friends. When discussing the end of our relationship, we locate the source of our problems in the difficult obstacles surrounding us, rather than in each other.

Discussion

In the process of researching my husband's social world, I anticipated that ethical problems would arise, and naively believed that I could mitigate them by following the advice from the literature. These pieces of advice shaped many of my early emotions, behaviors, and choices in the field. I worried about being an authentic friend, announcing my dedication to my relationships over my research, disclosing my research plans and progress, and writing up my findings in the most non-derogatory and inclusive way that I could. At the end of my marriage and research, I realize that I had become obsessed with numerous micro-considerations and had lost focus on the links between my data collection, relationships, research choices, writing, and institutionalized inequalities. I had lost this vision primarily because the literature had failed to discuss these distinctions meaningfully. This insight suggests that our ethical guides and discussions regarding intimacy, immersion, and subjectivity in the field need some refocusing.

What I should have focused on, and did not in the early days of research, was the way that my data gathering, writing, and relationships colluded with, enacted, or resisted historic

¹³ The divorce dealt a significant blow to both of us. I could not write and could barely teach and Lefty had difficulty tattooing for a year. This further highlights the lines dividing us. Tattooists are self-employed service workers, therefore, being unable to work meant that Lefty earned very little that year and had difficulty managing health insurance benefits, shop rent, and other out-of-pocket expenses associated with the occupation. My university salary allowed me to recover from the divorce with significantly greater stability and financial support.

exploitative practices and relationships surrounding me and the community that I studied. I certainly acknowledged the structural barriers between my study population and me, but this was not enough. In essence, I should have seen myself and others as enacting structures in the field. Furthermore, if the researchers' ethical code is to avoid harming study members, then I should have articulated very clearly how "doing structure" during research would affect informants' everyday lives.¹⁴ For example, by making Lefty's world and profession the center of my research, I was reinforcing traditional gender relationships between husbands and wives. Lefty and our friends submitted beautifully to traditional relationships between researchers and subjects by granting me unlimited access to their world, thoughts, feelings, perspectives, and lives. Here, however, intimacy, commitment, and subjectivity were not liberating. They greased the wheels of traditional gender and research relationships. It is not that I was being manipulative or inauthentic in my relationships, it was that intimacy was the vehicle through which we all reinforced larger structural relationships.

When the research ended and my professional status changed, however, is when we resisted, rather than reinforced, larger inequalities. Through our committed relationship, Lefty and I attempted to create a permanent space between the conventional academic world and the deviant tattoo subculture. Numerous barriers prohibited this. Historically, academics are allowed brief forays into other cultures (see Stoller, 2005), but they are not permitted permanent membership without career-threatening consequences. Researchers are not only barred from permanently living in the other's world, but they are also prohibited from bringing the other back with them from the field¹⁵ (see Blackwood, 1995; Gearing, 1995). Lefty and I were regularly reminded that he did not belong in my world through disapproving comments and stares from my colleagues. Couple this treatment with the isolating experience of being removed from a supportive deviant subculture, and the effect was grueling for Lefty. In addition, larger gender systems surrounding academia, as well as many other professions, made the role of househusband an awkward stance for any man—heavily tattooed, or not. These historic expectations, practices, and relationships closed the doors between our worlds in ways that went beyond what I did and how I acted in the field. Whether I loved or hated him, was intimate or distant, had sex in the field or remained celibate, committed my life to the tattoo community or used it only for career-building data, Lefty, or any outsider, will always have difficulty crossing over into legitimate domains. When marginalized men attempt to ride into middleclass worlds on women's coattails, the passage becomes even more attenuated. The structures ensure it.

¹⁴ Such a focus would have significantly altered my project. When Lefty and I discussed the advantages and disadvantages of the research, we should have focused our attention on the structural barriers between us and we could have discussed, with more clarity, how these would play out differently for us. Lefty, who initially imagined that his profession might gain greater legitimacy through the research, could have more clearly imagined how being married to a researcher might change his everyday life (i.e., having to circulate in my academic world and negotiating the job market with me). I might have predicted that I would have to choose between professional advancement or personal and professional commitment to study participants. Although, in the end, we might have made the same decisions, our choices would have been more informed.

¹⁵ My thanks to Carol Joffe for noting that the central problem was not marriage, but the class differences between Lefty and me. Although Lefty was born and raised in a similar middle-class background to mine, his status as a heavily tattooed man meant that he had difficulty circulating freely in middle-class society. I have known several female researchers who married members of the field and who encountered few of the problems facing Lefty and me. The difference is that they were researching up or across, instead of down. Gearing (1995), a white woman who married a Vincentian man, also divorced, although she did not elaborate on the reasons why. Blackwood (1995), on the other hand, explicitly noted the reason why she could not bring her lover back to live with her. Because the U.S. does not legally recognize same-sex partnerships, Blackwood's lover could not gain a visa to enter the U.S. from Indonesia.

The idea that we “do structure” in the field sheds light on the ongoing ethical debates regarding subjectivity, immersion, and writing in qualitative research. One of the primary discoveries from my experiences is that subjectivity is not more or less exploitative than objectivity. For example, several researchers, primarily working in the feminist paradigm, have warned that there is a distinction between “true” friendship and the friendliness that researchers offer (Acker et al., 1996; Cotterill, 1992; Finch, 1984; Gorelick, 1991; Kirsch, 1999; Patai, 1991; Stacey, 1988). Furthermore, researchers’ friendliness is interpreted as manipulative because it masks objective data-gathering goals, rather than establishing genuine bonds. On this subject, Reinharz (1993, p. 72) noted that special bonds between researchers and research participants are an excessive demand to place on feminist researchers.

My experiences push Reinharz’s critique a bit further by noting that genuine and long-lasting bonds are not only an excessive demand, but are also not the source of the exploitation. If researchers and research participants enact inequalities when they are intimate, intimacy can be even more damaging and problematic than objectivity. If there is any distinction between real and false intimacy, my relationship with my key informant and other study members was as true, real, and genuine as any in my life. Being genuine, committed, and forging special bonds were not the problem. The problem was the structures between my study population and me. In the end, the bonds we formed were not strong enough to overcome multiple inequalities. At times, also, these emotional bonds reinforced traditional gender relationships. Had I been a distant, objective observer abandoning the field as soon as it stopped yielding new data, Lefty might have been spared the devastation of culture shock, divorce, and the emotional obligation of assisting my work. It is important to insert a key warning about subjectivity here. It seems that the pleasures and dangers of the field can leave informants, as well as researchers, bruised and aching from their close encounters.

My research also sheds light on the politics of immersion in the field. For example, proponents of carnal (Wacquant, 2004) and experiential (Ferrell & Hamm, 1998b) sociology have suggested that partaking both physically and emotionally in the action of settings renders more vivid, visceral, and “better” images of the lived realities of social life. This stance leads to several ethical questions. If we cannot truly understand or faithfully describe the lived realities of a setting unless we become experientially involved, where does this put researchers who study the prostitution of underage girls (Inciardi, 1993), men who practice and boast of gang rape (Bourgois, 1995), or informants who use, produce, or traffic illegal drugs (Adler, 1993; Tunnell, 1998)? Can we not truly understand these worlds unless we engage in every behavior with setting members? Certainly, by having sex with informants, Goode (1999, 2002) has led some to begin drawing lines between valid experiential immersion and gratuitous thrill-seeking.

My research experiences suggest that we should avoid arguing that any behavior in the field is inherently unethical or ethical. To pick apart which behaviors are defensible expressions of immersion and which are unethical, would be to extend our obsession with the micro-politics of research. Researchers would do well to avoid debating whether it is wrong to have casual sex with informants, to witness an underage girl exchanging sex for crack, to listen silently as informants share gang-rape stories, or to engage in any other activities in the field. The most important tool to avoid harm and exploitation is to locate the structural context surrounding our research. This means we need to do more than acknowledge differing structural locations that we and study members occupy, and note how we enact inequalities when we have sex with informants (Goode, 1999, 2002), watch sex-for-crack exchanges (Inciardi, 1993), or listen to gang-rape stories (Bourgois, 1995). We should also ask, how did having casual sex in the field change the lives of researchers’ sex partners and other community members? What became of the girl who exchanged sex for crack in the crack

house? What happened, not only to gang-rape perpetrators in El Barrio, but to their victims? How were their lives affected during the course of and because of the research? Being able to answer these questions comprises the difference between gratuitous investigation and immersion.¹⁶

This insight also speaks to questions about discourse and representation. According to the postmodern perspective, some writing forms and topics (especially disclosing taboo subjects) can challenge dominant knowledge production systems. Characterizing this stance, Behar (1995, p. 4) has noted that some forms of writing can “decolonize the power relations inherent in the representation of the Other.” I want to argue that non-traditional writing styles and topics are not always and everywhere liberating or exploitative. We also “do structure” when we write. Just as subjective field relationships can reinforce gender and other inequalities, experimental texts and taboo topics can also support and perpetuate larger practices that perpetuate historic inequalities. We should ask how we enact, resist, or cope with structures when we write. Also, we should note how our writing changes setting members’ lives.¹⁷

In summary, I have argued that we have gone terribly off track in our ethical evaluation of intimate field relationships. The focus of our attention should not be on such micro-concerns as how to ethically engage in sex, love, or friendship with informants, what constitutes ethical immersion and what constitutes thrill seeking, how much or how little of our emotions and relationships we should disclose, or how and in what style we should write about the other. In the perspective advanced here, there is no inherent ethical advantage of subjectivity over objectivity, friendship over friendliness, intimacy over distance, celibacy over sex, crime over legality, disclosure over silence, or experimental writing over traditional discourse. We should locate how our relationships (intimate or distant), behaviors (norm-breaking or conventional), emotions (love or hate), writing (traditional or non-traditional), and other research choices are constrained by, work against, or reinforce social structures. In essence, we should look at all of our research activities as “doing structure.” This also means focusing most of our attention on the effects of this resistance or enactment on our research population—not on ourselves. This should be, and sadly has not been, the compass guiding our choices and behaviors.

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¹⁶ In fact, I would argue that any researcher who cannot tell a story about what became of research participants and the consequences that the research had for them, was not immersed in the setting.

¹⁷ For example, I assumed that asking study members to coauthor with me would be a way to avoid misrepresenting them. What I found, however, is that setting members did not have the time. Writing with me would mean losing valuable income and advancement in their other projects. While the tattoo community and the dominant academic discourse might have benefited by having multiple perspectives and voices included in what is being written about tattooing, in the end it was important for setting members to make the choice about the personal risks of authorship (or any other research practice and stance). In many cases, the individual deficits of writing against the traditional research canon might be more than setting members want to take on. This is their choice. Also, there is no guarantee that our writing, experimental or traditional, will emancipate and liberate more than it will perpetuate larger power relationships.

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