



# Relaxing the Taboo on Telling Our Own Stories: Upholding Professional Distance and Personal Involvement

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**Relaxing the Taboo on Telling our Own Stories:  
Upholding Professional Distance *and* Personal Involvement**

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**Relaxing the Taboo on Telling our Own Stories:  
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Abstract

Scholars studying organizations are typically discouraged from telling, *in print*, their own stories. The expression “telling our own stories” is used as a proxy for field-research projects that, in their written form, explicitly rely on a scholar’s personal involvement in a field. (By personal involvement in a field, I mean a scholar’s engagement in a set of mental activities that connect her to a field.) The assumption is that personal involvement is antithetical to maintaining professional distance. In this paper, I argue that the taboo on telling our own stories stems in part from an epistemological misunderstanding. Learning from the field entails upholding both distance *and* involvement; the two dimensions should not be conceptualized as opposite ends of a continuum. Moreover, I suggest that the taboo has become too extreme and stifles our collective capacity to generate new insights. To make this argument, I start by discussing the general taboo on telling one’s own stories. Second, I focus on the rationale set forth to justify the taboo, but also its limitations. Third, I examine what distance entails and how involvement, far from lessening distance, creates opportunities for generating potentially strong theoretical insight. Fourth, I showcase several areas of theoretical development that might benefit from revisiting the taboo. I conclude by reviewing key practical implications of such a shift for our profession and by arguing that organizational scholarship could gain a great deal from relaxing the taboo.

A few years ago, a sentence in a book by the anthropologist Michael Taussig made me pause. Appraising his profession, Taussig wrote, “Anthropology is blind to how much its practice relies on the art of telling other people’s stories—badly” (Taussig, 2006, p. 62). Though Taussig’s assertion did not cause me to reject all past research on “other people” (including my own) as an utter failure, it resonated strongly and prompted me to ask myself whether telling our own stories might offer an alternate scholarly path. Throughout this paper, the expression “telling our own stories” is used as a proxy for field-research projects that, in their written form, explicitly rely on a scholar’s personal involvement in a field. By personal involvement in a field, I mean a scholar’s engagement in a set of mental activities that connect her to a field (Elias, 1956, p. 227). The resonance of Taussig’s statement was probably heightened by the coincidence of having presented a research proposal a few days earlier to a roomful of clinical anatomists. After my presentation, one member of the audience rose and asked, in astonishment, “You mean you want to study us?” I recall being irritated by what I perceived as some arrogance in his tone. He himself routinely studied other people in his anatomy classes; why should he be off-limits to a similar inquiry? Taken aback by his question, I said that my intention was indeed to study them, though my focus would be on their practices rather than their inner selves. Yet he had a point: I was again trying to tell other people’s story. I never got access to that field site, but the conjunction of these insights led me to wonder: Why not try telling our *own* stories?

Most organizational scholars already build stories by constructing narratives through which they convey their findings to audiences (Golden-Biddle and Locke, 2007, p. 5). In that sense, all scholars compose their own stories, but few tell (in writing) their own. Most scholars showcase, instead, other people’s stories—even when relying on their personal involvement in a field to construct their narratives. This suggests that many scholars consider relying explicitly on their personal involvement to be a bad idea when publishing their findings. When a researcher tells her own story—whether about the community she grew up in or the profession she belongs to—personal involvement is made transparent and, for many scholars, such involvement is seen as antithetical to professional distance. By professional distance, I mean a scholar’s engagement in a set of mental activities that detach her from a field (Elias, 1956, p. 227).

In this paper, I argue that the perceived problem of relying in writing on personal involvement and the ensuing taboo on telling our own stories stem in part from an epistemological misunderstanding of the nature of field research. The misunderstanding entails conceptualizing distance and involvement on opposite sides of a continuum. Yet as Michael Agar remarks, learning from the field involves “the paradox of professional distance and personal involvement” (Agar, 1996, p. 7). A paradox refers to a *seemingly* self-contradictory proposition. The term *seemingly* is important since it implies that despite appearing contradictory, the combination of professional distance and personal involvement might in fact

prove compatible. In that sense, distance and involvement should not be conceptualized on a continuum, but treated as two distinct, yet related issues. There is no reason to assume that personal involvement cannot coexist with professional distance. Indeed, upholding distance and involvement is crucial when learning from the field. I will go a step further and argue that telling our own stories is sometimes even desirable. The taboo on telling our own stories not only perpetuates false notions about what field work entails, but has also become, I believe, too extreme in our profession and currently restricts our collective ability to generate new theoretical insights.

To make this argument, I start by discussing the general taboo on telling one's own stories. I review in particular common strategies used by scholars to handle in writing their personal involvement—namely, omission of involvement, downplayed disclosure of involvement, and desynchronized disclosures of involvement—to highlight the prevalence of the taboo. Second, I focus on the rationale set forth to justify the taboo, but also its limitations by discussing shifts in the broader social sciences toward more tolerance for telling our own stories. In a third step, and to further clarify the relation between distance and involvement, I examine what distance entails and how involvement, far from lessening distance, creates opportunities for generating potentially strong theoretical insight. Fourth, I showcase several areas of theoretical development that might benefit from relaxing the taboo to encourage scholars to try telling their own stories. I conclude the paper by reviewing key practical implications of such a shift for our profession and argue that organizational scholarship could gain a great deal from relaxing the taboo.

### **You Shall Not Tell Your Own Story**

Regardless of a scholar's position in and relation to a field, she gains access to the everyday life of individuals and typically develops some form of field involvement. The organizational studies literature and the social science literature more broadly offer many variations on the researcher's position in and relation to a given field. The position can range from a temporary and atypical role (the "total researcher," who only observes events and other participants) to prolonged and regular engagement (the "total participant," who is a participant first and a scholar second) (Gans, 1967, p. 440; Junker, 1960, pp. 35-40). When participating in and observing a field, personal involvement gradually builds. Involvement can sometimes end up being so intense that, for example, other field participants confide their utmost secrets to the scholar—e.g., when magicians teach a fieldworker how to perform their long-guarded tricks (Jones, 2011, pp. 34-76)—or that other field participants become part of a scholar's dreams—e.g., as when a fieldworker dreams she is chased by villagers she is studying because she stole their armor (Scheper-Hughes, 2001, p. 70).

Oftentimes, the more intrusive the data-collection process, the more questions are raised about the relationship between a scholar and her field. The question of depth of personal involvement tends to be raised more frequently at the participant-observation end of the intrusiveness spectrum than at the archival data-collection end (cf. Katz (2004) for such a discussion). Field researchers, particularly ethnographers, are expected to exercise considerable caution when interacting with other field participants, since too much perceived involvement with a field is typically viewed as a potential distraction from scientific inquiry. While most scholars would agree that personal involvement is needed to study any given field, they are typically discouraged from telling their own stories. As illustrations, Stephen Barley (1986) writing on radiologists or Patricia Ewick and Susan Silbey (1998) analyzing individuals' experiences of U.S. courtrooms make few scholars pause. By contrast, had Barley been a radiologist explicitly writing about his experience in radiology or Ewick/Silbey a defendant explicitly writing about her legal experience, scholars might react differently.

The taboo on telling one's own story permeates scholarly publications. Whatever a researcher's personal involvement with a given field, the prevailing advice about writing up one's findings seems to be to convey distance and to suggest a fairly blank researcher entering a distant field. Consider, for instance, the main ethnographic writing styles described by Clifford Geertz (1988), most of which convey detachment rather than involvement with a field. Geertz discusses, in particular, E. E. Evans-Pritchard's "slide-show" style (a confident voice from on high, describing a society almost clinically), Ruth Benedict's "us/not-us" style (contrasting other people's observed habits and behaviors with one's own), and Claude Lévi-Strauss's abstract self-contained style (pinning observations of others through the lens of formal theory). All of these approaches suggest that the writers are quite detached from and hardly involved with the field. Even Geertz's fourth style, which he labels the "I-witnessing" style (associated with Bronislaw Malinowski), embodies more the excitement of an explorer in an unfamiliar land than personal involvement with other participants (e.g., "Let me tell you about all the exotic practices I witnessed."). All four writing approaches conform to the expectation that academic writing be largely "unadorned" and "disembodied" in style (Golden-Biddle and Locke, 2007, p. 10). Any cues suggesting involvements that could represent potential distractions from the main scholarly goal tend to be carefully avoided (Adler and Adler, 2008, p. 12; Zelizer, 2009, p. 28). Thus field settings mostly scrubbed of personal involvement seem best suited to an academic audience.

Many organizational scholars also tend to depict their relationships to a field as remote. The initial exoticism of the setting is often emphasized in the resulting publication: e.g., freelancers are described as inhabiting an unexplored universe (Barley and Kunda, 2006), engineers as obeying counterintuitive norms (Bailyn, 1980; Kunda, 1992), and software engineers or programmers as living in a virtually parallel world (O'Mahony, 2007; Perlow, 1999). Even bank employees are described as

navigating apparently unknown territory (Weeks, 2004). Yet whether by serendipity or design, some field researchers end up being quite involved in their field. As Kai Erikson once remarked, scholars “live careers in which they occasionally become patients, occasionally take jobs as steel workers or taxi drivers, and frequently find themselves in social settings where their trained eye begins to look for data even though their presence in the situation was not engineered for that purpose” (Erikson, 1967, p. 368). Though being present in a setting does not necessarily mean being involved in it, the condition does create many opportunities for involvement. When this happens, scholars seem to adopt three main writing strategies for handling the perceived dilemma of involvement: omission of involvement, downplayed disclosure of involvement, and desynchronized disclosure of involvement. I detail these strategies below.

***Omissions of involvement.*** Many involved researchers simply choose to omit, at least for publication, the full nature of their relationship to the field. As Robert Sutton points out, there are many circumstances in which scholars are apt to decide against revealing certain steps in the pursuit of their research (Sutton, 1997, p. 104). Leaving purposely unspecified one’s relation to the field might be one of these omitted steps. In the same way that fieldworkers routinely engage in small “lies” by, for instance, typically characterizing themselves as friendly, unobtrusive, and chaste (Fine, 1993), they might also scrub a setting of any traces of their personal involvement to simply comply with prevailing expectations about published academic work.

Most of us can probably name multiple examples of omission of involvement—including in our own publications. But both the genesis of a project and the nature of relations between scholars and other participants often fade from readers’ memory. How many of us recall, for instance, that one co-author of the Hawthorne studies of workers’ motivation (Roethlisberger and Dickson, 1939) had previously worked as chief of employee relations at the plant? In that instance, he listed his job title in the published study to make evident his entanglement in the field. In many other instances, I suspect, the involved relation between scholars and their fields is routinely left unspecified and rapidly forgotten by most readers. If not for personal communications with authors, it would not be evident that the timing of a study of funeral-home directors coincided with a proximity to death in the author’s family (Barley, 1983), that a study of hospital employees’ job crafting was partly shaped by one of the authors’ upbringing in a nurse’s household (Wrzesniewski et al., 2003), or that a study of airline pilots stemmed in part from the author’s having been raised in family of pilots (Ashcraft, 2007). Such omissions help eliminate any perceived taint associated with personal involvement.

***Downplayed disclosure of involvement.*** Another way to handle involvement is to disclose it in the initial publication but downplay its role in the study’s data collection and analysis. Instead, the other field participants’ experiences and lives are the focus of the analysis. In a study of Amway distributors, for example, the author explains that a family member who was a distributor had sponsored his access to

the field (Pratt, 2003). While acknowledging a personal relationship to the field in the study's published findings (Pratt, 2000, p. 456), the author does not openly rely on his relationship to reach conclusions. Similarly, relatives often help authors gain access to research sites (Anteby, 2008b, p. 183; Morrill, 1995, p. 233), but the analytical benefits derived from such relations remain unclear. Disclosing the relation seems sufficient: any analytical benefits gained via personal involvement remain backstage. Moreover, the disclosure of involvement is limited here to the access stage, suggesting that the data collection and analysis are not tainted by involvement.

Occasionally the analytical benefits of personal involvement are alluded to more clearly. When studying her religious order's restructuring, Jean Bartunek (1984, p. 357) writes that her interpretation is based in part on her "own experience as a member of the order since 1966." Also, in a study of how professors on funding panels assess submissions, the author (who sat on several different panels) asserts that her experience made her "the consummate insider" (Lamont, 2009, p. 16). She adds that "insiderhood has influenced my analysis in myriad ways—facilitating access to the rather secretive milieus of funding organizations, for instance, and helping me understand these milieus, even as I deliberately made the familiar strange." In addition, in a study of banker socialization, the author acknowledges not only that her former employment on Wall Street informed her relation to the field, but also that prior experience increased her "empathy with informants" (Michel, 2007, p. 515). Though all authors steer clear of using their own stories in their published analyses (relying instead on analysis of "other people's" stories), they strongly hint that their experiences had proven analytically beneficial. In these examples, disclosure of involvement reads a bit like a brief methodological disclaimer and still sustains the taboo.

***Desynchronized disclosure of involvement.*** A third way to handle the perceived dilemma of involvement is to disclose one's relation to the field only after the work has gained scholarly legitimacy. Consider one of the earliest studies of homeless men in the United States (Anderson 1923): the published study enjoyed wide readership, but not until nearly forty years did a revised introduction acknowledge that its author had been "an intimate participant observer of the life of the hobo on the road" (Anderson, 1961, p. xiii). The author had lived as a hobo in his youth, wandering the United States like his book's protagonists. Another example of desynchronized disclosure is William H. Whyte's research on street-corner life (Whyte, 1943). Fifty years after its initial publication, Whyte detailed in a revised preface and appendix that he had lived with an Italian-American family that operated a restaurant before moving with his wife to a flat in the neighborhood he studied (Whyte, 1993). In extensive notes, Whyte also details his interactions with other field participants and how they shaped his thinking; high involvement appears to have helped him develop his analytical argument, but was not made explicit upon initial publication. These examples suggest that telling one's own story earlier might have put the studies' legitimacy at risk.



Only once studies have gained sufficient legitimacy do some authors decide to detail the involved nature of their scholarship.

The three main writing strategies, described above, to handle personal involvement in a field underline the prevalence of the taboo on telling our own stories. Like longitudinal participant observation (Barley, 1990, p. 238), personal involvement can intensify the complications of maintaining professional distance and the taboo might be justified. To better understand these complications, the next section examines the potential justifications for the taboo as well as its limitations.

### **Rationale for and Limitations of the Taboo**

The rationale for avoiding telling our own stories is rarely articulated clearly, but tends to dwell on two main issues: neutrality and access restrictions. First, involved field researchers are sometimes seen as unable to assess “coolly” what they are doing and seeing. Such involvement seems to be at odds with what Max Weber called *axiological neutrality*—that is, the expectation that a social scientist exclude personal bias when analyzing data (Weber, 2004, p. 22). Critics might contend, for example, that a scholar who is himself a rower examining a crew team (de Rond, 2008) might be inclined to depict the pursuit in more heroic terms than would a non-rower. A focus on heroism might in turn distract him from other field dynamics, such as those linked to a crew’s ethnic composition (Deslandes, 2005, p. 223). Similarly, a study of Xerox field-service technicians yields novel insights into the shifting nature and form of work (Orr, 1996). But readers might also suspect the study’s author of having toned down the field participants’ less appealing behavior, because he had worked as a technician at Xerox prior to conducting the study. The study is fairly quiet, for instance, about how technicians might contribute to overcharging clients, a practice described in a less “involved” study of other technicians and salespeople (Darr, 2006, p. 91). The implication is that involved “insiders” might be partial and might lack scientific neutrality. They may overlook crucial aspects of themselves that an outsider would be likely to notice; they run the risk of “going native” (Adler et al., 1986, p. 364). This is not to say that fieldworkers deliberately misjudge or misreport what they see. Instead, they might simply “know the rules of the game so deeply” that they “never even notice that there are rules” (Luker, 2008, p. 157), and in particular those rules that are best left unmentioned. Many academic readers view this phenomenon with concern since these dynamics can call into question the scholar’s judgment and, ultimately, the study’s results.

Alongside neutrality, restricted field access is often invoked as a second rationale for avoiding personal involvement. Even if involved fieldworkers can maintain neutrality, they might face unique forms of rejection from the field. The prevalent notion that “at its core, fieldwork must be regarded as something of a traitorous activity” always creates problems for fieldworkers (Van Maanen and Kolb, 1985, p. 24). Everett Hughes’s use of terms like “spy,” “double agent,” and “betrayal” to characterize

fieldworkers and fieldwork captures both the potential stigma attached to the endeavor and the possibility that other participants might feel violated (Hughes, 1974). Pushback from other field participants occurs in most, if not all field projects, but the fear of betrayal by an involved insider differs from the fear of betrayal by a stranger. Other field participants' reactions may be heightened if the violation seems more intrusive (i.e., coming from someone involved); sensitive to possible intrusion, they might think twice before allowing full access to fieldworkers wanting to tell their own stories. Carol Warren and Paul Rasmussen's (1977) study of massage parlors provides an example of the limits of personal involvement. Rasmussen's role as "boyfriend" of a masseuse allowed access to some data and restricted access to others. Boyfriends, for instance, were not told about a masseuse's potential sexual encounters with other clients (1977, p. 354). Field participants might conceal more from involved researchers than they would in the case of less involved outsiders. In other words, not only one's "tainted" views on a field but also possibly one's "tainted" relationships with other field participants can render telling one's own story problematic (Emerson, 2001, pp. 126-127). The nature of involved scholars' field access and interactions will be qualitatively different than those of a less involved outsider.

These issues of neutrality and restricted access largely explain why some academics are reluctant to consider findings from "involved" scholars and why authors engage in strategies to avoid discussing their involvement. Anthropologists have long recognized the special problems linked to telling their own stories and have traditionally been advised to steer clear of such situations (Richards, 1972, p. 299). Aspiring anthropologists have instead long been encouraged to embark on a journey into any other setting but home, and the dominant paradigm has been that the field is to be a foreign land (Desmond, 2007, p. 283). Fieldworkers more generally are advised to take up "positions in *other* people's lives in order to observe and understand them" (Emerson et al., 1995, p. 2) (emphasis added). Perhaps the title of a widely cited book on field interviewing, *Learning from Strangers* (Weiss, 1994), makes the point best: scholars are expected to learn from and write about strangers. Strangers are "other people" or individuals we are not personally involved with. Another methods expert argues that "a skilled, experienced ethnographer can often work with friends, relatives, or acquaintances," but notes that doing so will always create "certain difficulties;" thus "strangers make better informants" and, by extension, better protagonists in a study's written results (Spradley, 1979, p. 28). The same recommendations hold true for organizational scholars. Seemingly uncharted territory (from the researcher's perspective) appears to offer safer paths than does studying one's close setting. Thus, fire or police precincts (Desmond, 2007; Jackall, 1997; Moskos, 2008; Van Maanen, 1975) and medical settings (Anteby, 2010; Barley, 1986; Kellogg, 2009) all

make for suitable field choices. Disneyland, of course, ranks as the ultimate other land (Van Maanen, 1990).<sup>1</sup>

The taboo on telling one's own stories has a long history in the social sciences, but has also been repeatedly challenged. Cara Richards, writing about the involvement taboo in anthropology, notes that "a peculiar exception to this [taboo] is that both American and English institutions, when training foreign-born anthropologists, . . . often encourage them to study their own culture" (Richards, 1972, p. 299). Richards then asks: "Why allow foreign anthropologists to do something most American and English anthropologists are forbidden to do?" (See Kenyatta (1965) for an example of work resulting from an exception to the taboo.) Richards is not alone in raising this question. The colonial undertones of permitting some and prohibiting others to breach the taboo are in many cases difficult to ignore (Lewis, 1973). Other observers throughout the social sciences have similarly challenged the taboo and encouraged more reflexive or narrative field approaches that allow for more personal field involvement (Agar, 1996, p. 8; Bourdieu and Wacquant, 1992; Rabinow, 1977). What Raymond Firth, in his introduction to Bronislaw Malinowski's diaries, calls the modern vogue for "reflexive anthropology" emphasizing autobiographical elements gives scholars more license to tell their own stories (Malinowski, 1989, p. xviii). Meanwhile, the notion of conducting "at home ethnography" (Marcus and Fischer, 1986, pp. 22-23), of adopting more "subjective" approaches to field research (Adler and Adler, 2008, p. 3), and even of co-authoring publications with "ethnographic subjects" (Van Maanen, 2011b, p. 160) have slowly gained legitimacy. Anthropology and sociology initiated such a trend toward more personal involvement many decades ago (Emerson, 2001, p. 19), yet its echo in organizational scholarship has been more muted.

Despite many examples across the social sciences of research "starting where you are" (Lofland et al., 2006, p. 11), the taboo on telling one's own stories seems particularly strong in organizational studies. The few exceptions, such as Jean Bartunek's (1984) study of the restructuring of her own religious order, only confirm the norm. Moreover, studies clearly reliant in writing on involved data are noticeably absent from contemporary organizational scholarship. Aside from studies of the dynamics of corporate acquisitions (Mirvis and Louis, 1985), the shifting culture of investment banks (Knee, 2006), and team interactions in boat-racing crews (de Rond, 2008), examples are scarce. Yet our profession seems to have forgotten that many seminal works—including Donald Roy's (1955, 1959), Melvin Dalton's (1959), Chester Bernard's (1968), and Michael Burawoy's (1979) pieces on, respectively, informal work relations, executives' discretion, organizational coordination, and labor relations—were explicitly informed by their authors' deep personal involvement in their fields.

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<sup>1</sup> All the listed studies' settings sustain the illusion that their authors were not too involved, despite often working in these environments during summers (e.g., Van Maanen, 1990) or over longer periods of time (e.g., Desmond, 2007).

Much might therefore be lost by allowing the taboo to persist. Though personal involvement calls for paying extra attention to questions of professional distance, the “fix” of refraining from telling our own stories tries to circumvent the paradox of distance and involvement by eliminating one element from the equation. Such a fix misleads since it distorts both distance and involvement, and ultimately fails to capture the complexities of fieldwork.

### **Upholding Professional Distance *and* Personal Involvement**

The notions of professional distance and personal involvement are related, but independent concepts. A closer examination of what distance entails will help clarify the relation between the two concepts as well as highlight the necessity to uphold both. Professional distance should be understood as a scholar’s engagement in a set of mental activities that detach her from a field.<sup>2</sup> The distance that scholars are meant to maintain derives from and sustains the uniqueness of a scholar’s perspective. Like all professionals, scholars try to develop a perspective different from, and sometimes at odds with, that of the broader public (Freidson, 1970). More specifically, scholars aim to construct a general story from a particular context (Glaser and Strauss, 1967, p. 24). The general story tends to be distant and is usually what other scholars label a “contribution” to a given literature or an academic sub-field (Edmondson and McManus, 2007; Eisenhardt, 1989). Practices that enable professional distance include identifying patterns in given field data as well as comparing and contrasting them to other occurrences in the field setting (Glaser and Strauss, 1967, pp. 24-26; Suddaby, 2006). Also, maintaining an acute awareness of the assumptions that drive one’s data interpretations and being willing to critically explore these interpretations are practices that help build distance (Alvesson and Sköldbeg, 2000, pp. 5-6). A scholar’s ability to convey an awareness of “criticality” often helps convince other scholars of a study’s value (Golden-Biddle and Locke, 1993). More broadly, any practice that enables a scholar to engage doubt and see it as generative—for instance, keeping a reflexive field-journal or regularly confiding hesitations in a trusted colleague—contributes to sustain distance (Locke et al., 2008). Ultimately, the ability to create distance is what separates a scholar from (other) field participants and enforces a boundary between the two worlds (Gieryn, 1983).

Though all scholars are involved to some extent in their field, those who do not explicitly rely on their personal involvement can more easily maintain the impression (or illusion) of a boundary; by all accounts, they appear to perform different work and to conduct their daily lives in another realm than field participants—making them seem more detached from the field. By contrast, scholars who tell their

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<sup>2</sup> Like Elias, I prefer the terms “distance” and “involvement” to “objectivity” and “subjectivity” since the former terms can dynamically coexist whereas the latter ones suggest a static and unbridgeable divide between two entities (“subjects” and “objects”). See Elias (1956, p. 227) for a discussion of the terminology.

own story often feel a need to find alternate means of distinction. As an illustration, Peter Moskos's (2008) account of his stint as a newly minted police officer in Baltimore reports that his colleagues knew he was a graduate student and he would eventually move on. They sometimes made his distance explicit with comments like, "Oh, that's right, you're not a cop. You're just here to get your PhD" (Moskos, 2008, pp. 13-14). He knowingly reports the exchange. Similarly, Karen Ho appears involved with many of the bankers she studied, but she writes that she was once told by one of her bosses, "you're an anthropologist" (Ho, 2009, p. 16). An almost limitless variety of social dimensions—including contrasted demographical attributes—can be mustered to help a scholar convey social distance from other field participants.<sup>3</sup> Such reminders appear to reassure readers that the author is also a detached scholar, not merely one of the (overly) involved field participants. Yet social distance has little to do with professional distance.

As Elias reminds us, distance (and involvement) refers to a set of *mental activities* (Elias, 1956, p. 227). This means that a scholar can be socially close to other field participants and mentally detached from them (though usually not simultaneously). Moreover, Elias clarifies that mental activities can occur in relation to other field participants, to non-human field objects, and even to oneself. In that sense, professional distance need not always implicate other field participants. Distance is a state of mind, regardless of whether a scholar is socially proximate or not to other field participants. While being close to other field participants creates challenges to maintaining professional distance (e.g., raising the issue of axiological neutrality), these challenges are not insurmountable. Scholars, for example, can build distance from field objects such as cigarettes (Collins, 2004, pp. 297-345) and musical compact discs (Hennion, 1997), despite being close to other smokers/music lovers or even being smokers/music lovers themselves. Thus, it is not because a scholar appears socially distant from other field participants that professional distance is maintained. In addition, it is not because a scholar seems socially proximate to other field participants that professional distance fades.

When properly handled and regardless of social distance/proximity, involvement can be quite helpful to the research pursuit. In Loïc Wacquant's words, a fieldworker puts her "own organism, sensibility, and incarnate intelligence at the epicenter" of the field context she intends to dissect (Wacquant, 2003, p. xi). Our physical and affective sensations, including those shaped at the intersection between ourselves and our fields, help generate insights from our data (Barley, 1990, pp. 237-238; Heaphy and Dutton, 2008; Locke et al., 2008, p. 916; Mirvis and Louis, 1985). Though the shaping is probably more salient for ethnographers, I would argue that it holds true at various degrees for all field

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<sup>3</sup> For instance, contrasts with a scholar's perceived ethnicity—e.g., whites scholars studying African-American or Puerto-Rican communities (Bourgeois, 2003; Wacquant, 2003)—can help make a case for distinction.

researchers and is integral to all research pursuits. Although “the information obtained, rather than the experience lived, remains the focus of most field-based studies” (Georges and Jones, 1980, p. 3), the framing of a study’s argument can often be traced to such experiences (Carlsen and Dutton, 2011; Feldman et al., 2003; Vaughan, 1990, pp. 3-9). The researcher can legitimately be seen as an “instrument” or “device” of her craft, to employ Peggy Sanday’s terms, and can use her own reactions to capture observations that might otherwise go unnoticed (Sanday, 1979). In this sense, lived experiences can be actual data points for insightful analysis (Ellis and Flaherty, 1992; Emerson, 2001, p. 19). This proposition has profound implications for how data are collected and analyzed, suggesting in particular a need for more reflexive approaches to fieldwork.<sup>4</sup> If anything, involvement can only generate more experiential data and sensations, and ultimately better theoretical insights.

In telling her own story, a discerning scholar can build on her personal involvement to develop insights that can significantly contribute to and sharpen the analysis. But distance also needs to be upheld for such insights to emerge since involvement can occasionally also derail the research pursuit (Emerson, 2001, pp. 113-152; Faulkner and Becker, 2008). In that sense, involvement creates only a foundation for constructing insights. For example, an involved researcher’s faux pas in a field coupled with self-doubt can help reveal deeply held local norms (Van Maanen, 2011a). One of Robert Desjarlais’s (1992) faux pas during his study of healing ceremonies in Nepal illustrates such an opportunity. Desjarlais became a shamanic apprentice to learn about healing and, after much training, was able to reach states of trance. During trances, he saw visions and grew curious about their meanings. After describing his visions of “caves, tigers, and elfin creatures” to his shaman-mentor, he asked what they meant (p. 16). The mentor’s answer made clear that others did not see any visions akin to those he described and underlined the scholar’s relative “incompetency.” Yet this instance led Desjarlais to later specify and identify what other shamans saw, thus, uncovering the healers’ “imaginary gardens.” Involvement coupled with doubting his own experience allowed him to construct a general (distant) story of these imaginary gardens.

For scholars engaged in fieldwork or students considering a field setting for their theses, upholding distance and involvement is what will allow them to gain new insights. To conflate these two dimensions as belonging to one spectrum and refrain from telling one’s own story is, however, misguided. It presumes too rapidly that personal involvement inexorably signifies loss of professional distance and that social distance equates with professional distance. This view not only fails to distinguish distance and involvement; it also fails to recognize the necessity for *both* distance and involvement. When properly handled, telling one’s own stories can prove quite generative.

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<sup>4</sup> The logical culmination of the proposition is the pursuit of auto-ethnography, a method of using one’s own particular experience of a phenomenon to study it.

### Implications for Organizational Scholarship

Only a few organizational scholars, I suspect, will want to publish field-research explicitly reliant on personal involvement, but their pursuits should be deemed as legitimate as those adopting other more mainstream approaches. In doing so, these scholars will follow a path that has been traveled by others. Among the first social scientists to embrace this path was the anthropologist Kenneth Read, whose *The High Valley*, a highly engaged first-person account of clan rituals based on his time in New Guinea, was published in 1965. As Read later explained, the book was not meant as conventional ethnography but as a “record of a dialectic” between individuals (Read, 1980, p. ix). Since Read, other scholars have embraced an explicit reliance on involvement to conduct their research while also maintaining distance.<sup>5</sup> Unlike “confessional” fieldworkers, whose primary interest is to write within the established literary form of the confessional tale (Van Maanen, 2011b, pp. 73-100), scholars who rely on their involvement use their experience of the field primarily to inform their analysis, and regard the form in which they present their work in writing only as a secondary consideration. This approach—sometimes called “experimental” or “auto-ethnography”—goes beyond confessional tale because it typically treats “fieldwork experiences as vital techniques for structuring their narratives of description and analysis” (Marcus and Kushman, 1982, p. 26). What counts in these approaches is how the lived experience explicitly contributes to the analysis, almost regardless of the writing form.

The above statement might leave us thinking that enforcing the taboo or not on telling one’s own story is fairly inconsequential to mainstream organizational scholarship. To being with, the limited number of organizational scholars wanting to tell their own stories seems to render the debate somewhat confined. In addition, the few who are really committed to relying on their lived experiences to inform the analysis can still do so (like most scholars already do)—regardless of the availability and legitimacy of any particular writing form. Yet by enforcing a taboo on a specific written form (here, telling our own stories) we are not only closing down potential forms of written expression for organizational scholars, but more importantly perpetuating false notions about what fieldwork entails. The taboo therefore not only concerns those scholars who want to tell their own stories, but also all scholars who engage in and think about fieldwork, i.e., almost all organizational scholars. Upholding distance and involvement concerns us all.

Aside from clarifying an epistemological misunderstanding, what might be gained by relaxing the taboo on telling our own story? By relaxing it, I mean that we as a scholarly community should no longer

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<sup>5</sup> Examples of studies explicitly reliant on personal involvement include studies of communal life in Sicily (Belmonte, 1979), poker players’ routines in the United States (Hayano, 1979), and witchcraft’s persistence in rural France (Favret-Saada, 1980).

systematically discourage our members (or ourselves) from occasionally telling our own stories. I would argue that, at a minimum, issues of diversity, socialization, and power might become more salient in our pursuits if the taboo is relaxed. An embrace of telling our own stories might allow, in particular, for: (a) the emergence of previously unheard voices from the field; (b) a better understanding of organizational socialization; and (c) a stronger grasp of power relations in organizations.

First, with respect to diversity, the option to tell one's own story might appeal to voices from the field less often heard in academia, namely, those of less represented organizational members. The contrast these scholars could provide with the majority of other field participants' experiences could be quite valuable (Becker, 1963). For instance, the depiction of more reflexive approaches to fieldwork as a "feminist" methodological form—well suited to conveying women's experiences in a male-dominated world—illustrates the way in which telling one's own story can offer such members opportunities to be heard (Stacey, 1991; Visweswaran, 1994). A recent review of scholarship on diversity notes the scarcity of research on diversity in organizations: only five percent of articles published in management journals in 2000-2008 included *race* or *gender* among their keywords (Brief and Chugh, 2008). Given this profile, the benefits of telling one's own story for management scholarship might prove quite high. Allowing for greater field involvement might permit more "polyvocality"—that is, for more participants to speak in their own voices (Marcus and Fischer, 1986, p. 15), thus potentially generating data on forms of diversity that have not yet reached the threshold to constitute a domain of study in their own right. If qualitative research in general is well suited to examining the meaning of relatively common experiences, such as chronic illness and divorce (Strauss and Corbin, 1998, p. 11), research explicitly reliant on involvement could lend visibility to less common experiences, such as those of disabled or transgender members of organizations.

Kay Kaufman Shelemay's (1991) work provides a nice example of the polyvocal power of relaxing the taboo: by telling her own story as an expatriate in Ethiopia, she brought to light the unique dynamics facing expatriates (a community hardly studied at that time) and the centrality of longing in diasporas. To date, and despite representing thousands of workers worldwide, expatriates are still rarely viewed as a cohesive group and hardly register as different from other (local) organizational members. A recent survey of 29 Organization for Economic Cooperation and Development (O.E.C.D.) countries found that 36.3 million persons residing in one country come from another O.E.C.D. country (Dumont and Lemaître, 2005, pp. 12-13). Not all of these individuals are highly skilled (i.e., with a tertiary education) or what we might call traditional expats, but a significant number still are. For example, the United Kingdom, Germany, Mexico, and Poland together have more than 1.9 million highly skilled citizens living in other O.E.C.D. countries. In that sense, telling one's own story might be a way of telling a story yet uncovered that also has universal appeal.



Second, relaxing the taboo on personal involvement in writing might invigorate discussions of socialization. Personal involvement often entails intensive mediation of the field by the researcher: selecting what one “sees” or “keeps track of” reveals a lot about the mediating effect of an organizational context, or the ways in which a context shapes a person (Mills, 1990; Van Maanen and Kolb, 1985). In other words, telling one’s own story can reveal a lot about socialization. Aside from novel insights related to social networks (e.g., Morrison, 2002) and identity dynamics (e.g., Pratt et al., 2006), advances in our understanding of how organizations socialize their members have been fairly slow since John Van Maanen and Edgar H. Schein’s (1979) early work. Studies relying on cross-organizational design have identified many discrete elements of the process (e.g., Ashforth and Saks, 1996, 2007, and Chatman, 1991), but they have also focused less on the comprehensive experiences of participants. An explicit reliance on personal involvement might allow scholars to better understand, for example, the neglected micro-socialization processes occurring in institutions and organizations (Hallett and Ventresca, 2006; Powell and Colyvas, 2008), and further refute the view of members as “cultural dopes” (Garfinkel, 1967, p. 68).

Robert Faulkner and Howard Becker’s (2008) reflections on the jazz world (in which they are personally involved as musicians) illustrate the theoretical traction gained with respect to socialization from telling our own stories. They identify numerous tacit socialization dynamics in jazz, some previously rarely discussed. For instance, they note the difference between “false ignorance” and “false knowledge” (p. 16). False ignorance entails a person not thinking she knows something, but in fact actually knowing it. False knowledge entails a person thinking she knows something, but in fact not actually knowing it. The concepts could be highly applicable to organizations involved in the recent U.S. mortgage crisis. For instance, false ignorance might prove a defensive reaction to a crisis. Mortgage lenders might have known the extent of the crisis yet convinced themselves that they did not know what was happening. By contrast, the same lenders might have thought they knew about the extent of the crisis, but actually did not know enough. The distinction is crucial to organizational ethics: excusing false knowledge is probably easier than excusing false ignorance. Arguably, less involved authors could have discussed these dynamics as well. Yet, Faulkner and Baker’s insights were partly generated by their deep personal involvement in the field. Relaxing the taboo on telling our own stories might help us gain a deeper understanding of these and other complex socialization dynamics.

Finally, telling our own stories might yield new insights into power issues in organizations. Ethnography is often seen as a valuable lens with which to study issues of power (Abu-Lughod, 2000, p. 266; Beaud and Weber, 2010, pp. 6-7), and fieldwork explicitly reliant on personal involvement might prove particularly well suited to surfacing such power dynamics. Being subjected to power is a situation often difficult to measure and articulate, yet power dynamics are generally experienced very intimately

(Foucault, 1977). As an editor of a mainstream organizational journal recently noted, the dearth of papers in organizational behavior dealing with power (and status) is especially salient at both the submission and publication stages in mainstream outlets (Morrison, 2010). This sentiment is echoed by other editors who write that research on power rarely appears in management journals (Flynn et al., 2011, p. 495).

Scholarship explicitly reliant on involved data might help us regain ground in this arena.

Ashley Mears's (2011) study of fashion models and the pricing of beauty showcases the potential benefits of bringing one's own story to bear into discussions of power. A former model who spent a total of seven years working in the fashion world (two of which were mainly dedicated to her research), Mears draws on her own experiences and doubts to document models' ambivalence towards the shaping of their identities. Modeling agency scouts' repeated attempts to emphasize the traditional female "beauty" of women (e.g., blond, fair skinned, and tall) were often met by the models' desire to succeed. Yet these identities prove contested terrain. Mears wanted to fit in, yet resisted being fully shaped. Whereas identity and identification in organizational scholarship have mainly been discussed in terms of gradients (from low to high levels) or in terms of multiplicity and complexity (e.g., hybrid identities), the notion of a *simultaneously* desired and imposed identity is rarely examined in organizational scholarship. Past scholarship has mainly emphasized one of the two facets, namely the desired aspects of identities (e.g., Anteby, 2008a and Ibarra, 1999) or the imposed ones (e.g., Alvesson and Willmott, 2002 and Thornborrow and Brown, 2009). Much more research is needed on the combined dynamics of identity desire and imposition, as well as on their associated power implications. In that respect, telling our own stories by drawing on such moments of ambivalence can help advance our general knowledge of experiencing power.

These topics are only a few of the scholarly arenas that might benefit from relaxing the taboo on telling our own stories. In the meantime, we should nonetheless make clear the costs and the benefits of such relaxing. For instance, Gary Alan Fine warns strongly against the temptation to transform "the intensive labor of field research into the armchair pleasures of 'me-search'" (Fine, 1999, p. 534). Similarly, John Van Maanen warns of the limitation of "do-it-yourself" ethnographies (Van Maanen, 2011b, pp. 164-165), specifically when carried out by involved insiders. The main danger is to produce empirically unsound and conceptually empty scholarship. A scholar's main task is to extract a general story from a field, and telling one's own story can easily lure an author away from generality and down the slippery slope of narcissism. Most of us dread reading work in which the author's ego eclipses the phenomenon being studied. That said, the benefits of enforcing the taboo have been assumed to outweigh its costs. Unless we try relaxing it while still upholding distance, we will never know what we might be missing.

## Practical Implications

This paper raises many practical implications for our profession. I will list, below, some key implications, but will also leave it to readers to add their own as a way, perhaps, to start experimenting with telling one's own story. The first and main practical implication of this paper is that, as an experiment at least, we should try telling our own stories more often, or allow others to do so, and only afterward compare their value to that of "other people's" stories. It is becoming increasingly obvious that "fieldwork practices are biographically and contextually varied" (Van Maanen, 2011b, p. 151). Telling our own stories contributes to this diversity. When in doubt about the representativeness of a particular analysis reliant on personal involvement, we can take solace in E. E. Evans-Pritchard's words: "If allowances are made for the personality of the writer, and if we consider that in the entire range of anthropological studies the effects of these personal differences tend to correct each other, I do not think that we need worry unduly over this problem" (cited by Geertz, 1988, p. 62). The experiment would consist, therefore, in generating more studies explicitly reliant on personal involvement to build a more robust and varied understanding of a given context or issue. We should also try finding "other people" in our own stories and generalizing from our experiences to a level of abstraction that readers might relate to. In doing so, our stories might generate theoretical insights that are worth the challenges that telling our own story raise.

A second main implication of this paper relates to our academic training. Organizational scholars should perhaps learn a trick from the playbook of the Chicago School fieldworkers. As Robert Emerson notes, early Chicago School fieldworkers came to academia from established places in the world they studied (Emerson, 2001, p. 16). Individuals involved in fields (like the former hobo, Nels Anderson) were in essence converted into scholars. Such an infusion led to deep understanding of given settings. Faulkner and Becker, however, make clear that one does not have to be a jazz musician to study jazz worlds or a woman to study women (2008, p. 19). (Being part of the story is not a necessity for studying it, but being *involved* in it matters.) That said, organizational scholars should also consider it legitimate for a scholar to be part of the story and to study it. In the same way that being part of the story is not a necessary condition to study it, *not* being part of a story should not be a pre-condition to study it. Both conditions raise unique challenges for scholars and should be equally legitimate as long as the challenges are properly handled.

A third and perhaps more prospective implication of this paper is that it might also help to tell one's own story in non-field research settings. There is no reason to believe that scholars reliant on experimental or archival data might not also benefit indirectly from field involvement. Assuming subjects are blind to the manipulation, debriefing an experimental design with involved subjects from the scholar's perspective (e.g., a close friend) might yield higher quality data than debriefing with strangers (e.g., an

unknown undergraduate student), and ultimately stronger theoretical insights. Similarly, an archival discourse scholar could examine a text produced by a close family member and juxtapose the analysis with the family member's clarifications. While such approaches carry obvious risks, they might also prove rewarding and allow scholars to probe new domains of inquiry. Again, only experimentation through a relaxation of the taboo on telling one's own story will allow us to assess these potential benefits.

A final implication of this paper concerns our handling of professional distance. In particular, we should pay more attention to maintaining professional distance and not let the taboo obscure alternate and potentially more problematic distance-reducing dynamics in our profession. As illustrations, I will focus, on two separate dynamics—a scholar's financial dependence on a field and her inability to sustain social exclusion—that can also prove challenging with respect to distance. Financial relations between a scholar and a field can take many forms. Besides working directly for an organization, a scholar can also receive financial support or compensation in the form of consulting fees from a studied organization. In recent years, medical scholars have been particularly sensitive to such issues, imposing strict guidelines on conflicts of interest (Fontanarosa et al., 2010). In organizational studies, the adoption of new types of disclosure attests to similar concerns. For instance, the disclosure that an author joined the advisory board of an organization he had studied *only after* the completion of data analysis suggests that doing so earlier might have proven problematic (Jeppesen and Lahkani, 2010, p. 1031). While money might not corrupt, it can radically change the nature of interpersonal interactions (Zelizer, 2010). Money can also easily taint one's judgment. If a scholar's livelihood depends, for instance, in part on other field participants' decisions, the ability to maintain distance can be constrained. Enforcing stricter conflict of interest disclosures in our journals would be welcome.

Similarly, a scholar's inability to socially deviate from field norms can also reduce professional distance. For instance, a given researcher might be subject to too much normative social pressure to be able to fully report findings for fear of social exclusion. Like ideal informants, field researchers often occupy insider/outsider or liminal positions in the contexts they study (Adler et al., 1986; Bartunek and Louis, 1996; Emerson, 2001, pp. 122-123; Luker, 2008). The ideal field researcher or informant is often deeply familiar with the context but not fully bound by its traditions; she can partly escape social control without too much fear of exclusion because she already operates at the group's margins (Rabinow, 1977, pp. 31-69). To maintain distance, scholars need to be able to withstand rejection. Howard Becker alluded to this obligation when he advised fieldworkers facing resistance to act as though all they can hear is yes or maybe, but never no (cited by Leidner, 1993, p. 236). The ability to socially deviate illustrates the multiple facets of distance, some of which are easier to assess (e.g., financial ties) than others (e.g.,

withstanding rejection).<sup>6</sup> These and other distance-reducing dynamics require much closer attention in our profession.

In conclusion, compelling academic stories and learning from the field require both personal involvement *and* professional distance, regardless of how explicit such involvement is made in writing. In that sense, the most advanced art of storytelling consists in (explicit or not) personal stories that go beyond a scholar's experience: stories that are universal and speak to many readers. Erving Goffman's (1989, pp. 155-156) advice for a scholar to be fully immersed in a setting while also cutting life "to the bone" as much as she can afford captures well the paradox of learning from the field. The way to make sense of a social world, he adds, is to not "hold on" to anything of our own. Our main challenge is to tell stories without ever being tempted to own them, regardless of whether they are or not our own.

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<sup>6</sup> Though one's ability to withstand rejection is hard to assess, a small test of introversion offered by Peter Moskos (2011) might prove a starting point. (Moskos claims that introversion can be an asset for fieldworkers.)

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