



PERSPECTIVE

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

Michel Anteby

Harvard Business School, Boston, Massachusetts 02163, manteby@hbs.edu

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 against 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 against telling one’s own stories. Second, I focus on the rationale set forth to justify not only 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.

Key words: fieldwork; research practices; distance; involvement; taboo

History: Published online in *Articles in Advance* September 10, 2012.

Introduction

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 alternative 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 stories. 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). 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 it is 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 against 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 because 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 against 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 it currently restricts our collective ability to generate new theoretical insights.

To make this argument, I start by discussing the general taboo against telling one's own stories. To highlight the prevalence of the taboo, I review common strategies used by scholars to handle, in writing, their personal involvement—strategies including the omission of involvement, downplayed disclosure of involvement, and desynchronized disclosures of involvement. Second, I focus on the rationale set forth to justify not only the taboo but also its limitations by discussing shifts in the broader social sciences toward more tolerance for telling our own stories. Third, 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 become so intense that, for example, other field participants confide their utmost secrets to the scholar (e.g., as when magicians teach a fieldworker how to perform their long-guarded tricks; see Jones 2011, pp. 34–76) or other field participants may become part of a scholar's dream (e.g., as when a fieldworker dreams she is chased by villagers she is studying because she stole their armor; see 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 (see Katz 2004 for such a discussion). Field researchers, particularly ethnographers, are expected to exercise considerable caution when interacting with other field participants, because too much perceived involvement with a field is typically viewed as a potential distraction from scientific inquiry. Whereas 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's (1986) writing on radiologists or Patricia Ewick and Susan Silbey's (1998) study 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 or 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 publishing one's findings seems to be to convey distance and suggests 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 Bronisław 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, Zelizer 2009). 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 may decide against revealing certain steps in the pursuit of their research (Sutton 1997). 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. Yet 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 coauthor 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), the author does not openly rely on his relationship to reach conclusions. Similarly, relatives often help authors gain access to research sites (Anteby 2008b, Morrill 1995), 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. For example, when studying her religious order's restructuring, Bartunek (1984, p. 357) writes that her interpretation is based, in part, on her "own experience as a member of the order since 1966." In a study of how professors on funding panels assess submissions, another 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.” A third example involves a study of banker socialization, where 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). Although 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, Nels Anderson’s 1923 *The Hobo*. The published study enjoyed wide readership, but it was not until nearly 40 years later that 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, published in 1943 as *Street Corner Society*. Fifty years after his study’s initial publication, he 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 detailed his interactions with other field participants and how they shaped his thinking; high involvement appears to have helped him develop his analytical argument, but it 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 after studies have gained sufficient legitimacy do some authors decide to detail the involved nature of their scholarship.

The above-mentioned three main writing strategies to handle personal involvement in a field underline the prevalence of the taboo against telling our own stories. Like longitudinal participant observation (Barley 1990), personal involvement can intensify the complications of maintaining professional distance, and the taboo might thus 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 nonrower. 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). 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 that the study’s author 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). The implication is that involved “insiders” might be partial and thus 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)—in particular, those rules that are best left unmentioned. Many academic readers view this phenomenon with concern as 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” creates problems for fieldworkers (Van Maanen and Kolb 1985, p. 24). Everett Hughes’s use of terms such as “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. Warren and Rasmussen's (1977) study of massage parlors provides an example of the limits of personal involvement. Rasmussen's role as the "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 (Warren and Rasmussen 1977). 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 with whom we are not personally involved. 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).¹

The taboo against telling one's own stories has a long history in the social sciences, but it 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, Bourdieu and Wacquant 1992, Rabinow 1977). What Raymond Firth, in his introduction to 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 notions 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 coauthoring 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, 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 against telling one's own stories seems particularly strong in organizational studies. The few exceptions, such as 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 Roy's (1952, 1959), Dalton's (1959), Barnard's (1968), and Burawoy's (1979) pieces on informal work relations, executives' discretion, organizational coordination, and labor relations, respectively—were explicitly informed by their authors' deep personal involvement in their fields.

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 because it distorts both distance and involvement and ultimately fails to capture the complexities of fieldwork.

Upholding Distance and 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.² 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 subfield (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, 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 Sköldbberg 2000). 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) helps 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 own story often feel a need to find alternative 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 such as, “Oh, that's right, you're not a cop. You're

just here to get your Ph.D.” (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.³ 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 (1956) reminds us, distance (and involvement) refers to a set of *mental activities* (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 nonhuman 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. Although 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) 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, Heaphy and Dutton 2008, Locke et al. 2008, 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 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). 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). This proposition has profound implications for how data are collected and analyzed, suggesting in particular a need for more reflexive approaches to fieldwork.⁴ 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 because involvement can occasionally also derail the research pursuit (Emerson 2001, 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 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.

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 to be conventional ethnography but rather 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.⁵ 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 they regard the form in which they present their written work only as a secondary consideration. This approach—sometimes called "experimental" or "autoethnography"—goes beyond the 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 begin with, the limited number of organizational scholars who want 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 (as 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 concerns not only those scholars who want to tell their own stories but 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 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. Embracing the telling of our own stories might particularly allow 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 5% 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, allow 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 multiethnic 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 (OECD) countries found that 36.3 million persons residing in one country come from another OECD country (Dumont and Lemaître 2005). Not all of these individuals are highly skilled (i.e., with a tertiary education) or what we might call traditional expatriates, 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 OECD 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 Van Maanen and 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; 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 microsocialization 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 to be a defensive reaction to a crisis. Mortgage lenders might have known the extent of the crisis and 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 when they 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 through which to study issues of power (Abu-Lughod 2000, Beaud and Weber 2010), and fieldwork explicitly reliant on personal involvement might prove particularly well suited

to unearthing 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). 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 on 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 toward the shaping of their identities. Modeling agency scouts' repeated attempts to emphasize the traditional female "beauty" of women (e.g., blond, fair skinned, tall) were often met by the models' desire to succeed. Yet these identities proved to be 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: the desired aspects of identities (e.g., Anteby 2008a, Ibarra 1999) or the imposed aspects (e.g., Alvesson and Willmott 2002, 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 strongly warns 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 in producing 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 list some key implications below, but I 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 tell theirs, and only afterward should we 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 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, of 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 to which readers might relate. In doing so, our stories might generate theoretical insights that are worth the challenges raised by telling them.

A second main implication of this paper relates to our academic training. Organizational scholars should perhaps learn a lesson 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 former hobo Anderson) were in essence converted into scholars. Such an infusion led to deep understanding of given settings. Faulkner and Becker (2008), however, make clear that one does not have to be a jazz musician to study jazz worlds or a woman to study women. (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 precondition 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. If subjects are assumed to be 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 could ultimately yield 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. Although 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 alternative 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 may also receive financial support or compensation in the form of consulting fees from the organization being studied. 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). Money might not corrupt, but it can radically change the nature of interpersonal interactions (Zelizer 2010). Money can also easily taint one's judgment. For instance, if a scholar's livelihood depends, in part, on other field participants' decisions, the ability to maintain distance can be constrained. Enforcement of 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, 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). To maintain distance, scholars must be able to withstand rejection. 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 above examples illustrate the multiple facets of distance, some of which are easier to assess (e.g., financial ties) than others (e.g., withstanding rejection).⁶ 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 of personal stories (explicit or not) 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 or not they are our own.

Acknowledgments

The author is very grateful to Nitin Nohria for encouraging him to write this piece and to his Harvard Business School colleagues for pushing him to clarify his methodological choices. He also thanks Martha Feldman and the *Organization Science* reviewers for their thoughtful guidance; Julie Battilana, Karen Golden-Biddle, Ann Goodsell, Pierre-Emmanuel Moog, Leslie Perlow, Metin Sengul, Steven Shafer, and Tieying Yu for their comments on earlier drafts of the piece; as well as Graham Jones, John Van Maanen, and Florence Weber for pointing him to relevant sources.

Endnotes

¹All 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).

²Like Elias, I prefer the terms “distance” and “involvement” to “objectivity” and “subjectivity” because 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.

³For instance, contrasts with a scholar's perceived ethnicity—e.g., Caucasian scholars studying African American or Puerto

Rican communities (Bourgeois 2003, Wacquant 2003)—can help make a case for distinction.

⁴The logical culmination of the proposition is the pursuit of autoethnography, a method of using one's own particular experience of a phenomenon to study it.

⁵Examples 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).

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

References

- Abu-Lughod L (2000) Locating ethnography. *Ethnography* 1(2):261–267.
- Adler PA, Adler P (2008) Of rhetoric and presentation: The four faces of ethnography. *Sociol. Quart.* 49(1):1–30.
- Adler PA, Adler P, Rochford EB (1986) The politics of participation in field research. *Urban Life* 14(4):363–376.
- Agar MH (1996) *The Professional Stranger: An Informal Introduction to Ethnography* (Academic Press, San Diego).
- Alvesson M, Skoldberg K (2000) *Reflexive Methodology: New Vistas for Qualitative Research* (Sage, London).
- Alvesson M, Willmott H (2002) Identity regulation as organizational control: Producing the appropriate individual. *J. Management Stud.* 39(5):619–644.
- Anderson N (1961) *The Hobo: The Sociology of the Homeless Men* (University of Chicago Press, Chicago). [Orig. pub. 1923.]
- Anteby M (2008a) Identity incentives as an engaging form of control: Revisiting leniencies in an aeronautic plant. *Organ. Sci.* 19(2):202–220.
- Anteby M (2008b) *Moral Gray Zones: Side Productions, Identity, and Regulation in an Aeronautic Plant* (Princeton University Press, Princeton, NJ).
- Anteby M (2010) Markets, morals, and practices of trade: Jurisdictional disputes in the U.S. commerce in cadavers. *Admin. Sci. Quart.* 55(4):606–638.
- Ashcraft KL (2007) Appreciating the “work” of discourse: Occupational identity and difference as organizing mechanisms in the case of commercial airline pilots. *Discourse Comm.* 1(1):9–36.
- Ashforth BE, Saks AM (1996) Socialization tactics: Longitudinal effects on newcomer adjustment. *Acad. Management J.* 39(1):149–178.
- Ashforth BE, Saks AM (2007) Socialization tactics, proactive behavior, and newcomer learning: Integrating socialization models. *J. Vocational Behav.* 70(3):447–462.
- Bailyn L (1980) *Living with Technology: Issues at Mid-Career* (MIT Press, Cambridge, MA).
- Barley SR (1983) Semiotics and the study of occupational and organizational cultures. *Admin. Sci. Quart.* 28(3):393–413.
- Barley SR (1986) Technology as an occasion for structuring: Evidence from observation of CT scanners and the social order of radiology departments. *Admin. Sci. Quart.* 31(1):78–108.
- Barley SR (1990) Images of imaging: Notes on doing longitudinal field work. *Organ. Sci.* 1(3):220–247.
- Barley SR, Kunda G (2006) *Gurus, Hired Guns, and Warm Bodies: Itinerant Experts in a Knowledge Economy* (Princeton University Press, Princeton, NJ).
- Barnard C (1968) *The Functions of the Executive* (Harvard University Press, Cambridge, MA).
- Bartunek JM (1984) Changing interpretive schemes and organizational restructuring: The example of a religious order. *Admin. Sci. Quart.* 29(3):355–372.
- Bartunek JM, Louis MR (1996) *Insider/Outsider Team Research* (Sage, Thousand Oaks, CA).
- Beaud S, Weber F (2010) *Guide de l'Enquête de Terrain: Produire et Analyser des Données Ethnographiques*, 4th ed. (Editions La Découverte, Paris).
- Becker HS (1963) *Outsiders: Studies in the Sociology of Deviance* (Free Press of Glencoe, London).
- Belmonte T (1979) *The Broken Fountain* (Columbia University Press, New York).
- Bourdieu P, Wacquant LJD (1992) *Réponses: Pour une Anthropologie Réflexive* (Seuil, Paris).
- Bourgeois P (2003) *In Search of Respect: Selling Crack in El Barrio* (Cambridge University Press, Cambridge, UK).
- Brief AP, Chugh D (2008) Introduction: Where the sweet spot is: Studying diversity in organizations. Brief AP, ed. *Diversity at Work* (Cambridge University Press, New York), 1–12.
- Burawoy M (1979) *Manufacturing Consent: Changes in the Labor Process Under Monopoly Capitalism* (University of Chicago Press, Chicago).
- Carlsen A, Dutton JE, eds. (2011) *Research Alive: Exploring Generative Moments in Doing Qualitative Research* (Liber, Copenhagen Business School Press, Copenhagen).
- Chatman JA (1991) Matching people and organizations: Selection and socialization in public accounting firms. *Admin. Sci. Quart.* 36(3):459–484.
- Collins R (2004) *Interaction Ritual Chains* (Princeton University Press, Princeton, NJ).
- Dalton M (1959) *Men Who Manage: Fusions of Feeling and Theory in Administration* (John Wiley & Sons, New York).
- Darr A (2006) *Selling Technology: The Changing Shape of Sales in an Information Economy* (Cornell University Press, Ithaca, NY).
- de Rond M (2008) *The Last Amateurs: To Hell and Back with the Cambridge Boat Race Crew* (Icon Books, London).
- Desjarlais RR (1992) *Body and Emotion: The Aesthetics of Illness and Healing in the Nepal Himalayas* (University of Pennsylvania Press, Philadelphia).
- Deslandes PR (2005) *Oxbridge Men: British Masculinity and the Undergraduate Experience, 1850–1920* (Indiana University Press, Bloomington).
- Desmond M (2007) *On the Fireline: Living and Dying with Wildland Firefighters* (University of Chicago Press, Chicago).
- Dumont J-C, Lemaître G (2005) Counting immigrants and expatriates in OECD countries: A new perspective. OECD Social, Employment and Migration Working Paper 25, OECD Publishing, Paris.
- Edmondson AC, McManus SE (2007) Methodological fit in management field research. *Acad. Management Rev.* 32(4):1155–1179.
- Eisenhardt KM (1989) Building theories from case study research. *Acad. Management Rev.* 14(4):532–550.

- Elias N (1956) Problems of involvement and detachment. *British J. Sociol.* 7(3):226–252.
- Ellis C, Flaherty M (1992) *Investigating Subjectivity: Research on Lived Experience* (Sage, Newbury Park, CA).
- Emerson RM (2001) *Contemporary Field Research: Perspectives and Formulations*, 2nd ed. (Waveland Press, Long Grove, IL).
- Emerson RM, Fretz RI, Shaw LL (1995) *Writing Ethnographic Fieldnotes* (University of Chicago Press, Chicago).
- Erikson KT (1967) A comment on disguised observation in sociology. *Soc. Problems* 14(4):366–373.
- Ewick P, Silbey SS (1998) *The Common Place of Law: Stories from Everyday Life* (University of Chicago Press, Chicago).
- Faulkner RR, Becker HS (2008) Studying something you are part of: The view from the bandstand. *Ethnologie Française* 38(1):15–21.
- Favret-Saada J (1980) *Deadly Words: Witchcraft in the Bocage* (Cambridge University Press, Cambridge, UK).
- Feldman MS, Bell J, Berger MT (2003) *Gaining Access: A Practical and Theoretical Guide for Qualitative Researchers* (AltaMira Press, Walnut Creek, CA).
- Fine GA (1993) Ten lies of ethnography: Moral dilemmas of field research. *J. Contemp. Ethnography* 22(3):267–294.
- Fine GA (1999) Field labor and ethnographic reality. *J. Contemp. Ethnography* 28(5):532–539.
- Flynn FJ, Gruenfeld D, Molm LDM, Polzer JT (2011) Social psychological perspectives on power in organizations. *Admin. Sci. Quart.* 56(4):495–500.
- Fontanarosa PB, Flanagan A, DeAngelis CD (2010) Implementation of the ICMJE Form for reporting potential conflicts of interest. *J. Amer. Medical Assoc.* 304(13):1496.
- Foucault M (1977) *Discipline and Punish: The Birth of the Prison* (Vintage Books, New York).
- Freidson E (1970) *Professional Dominance: The Social Structure of Medical Care* (Atherton, New York).
- Gans H (1967) *The Levittowners: Ways of Life and Politics in a New Suburban Community* (Vantage, New York).
- Garfinkel H (1967) *Studies in Ethnomethodology* (Prentice Hall, Englewood Cliffs, NJ).
- Geertz C (1988) *Works and Lives: The Anthropologist as Author* (Stanford University Press, Stanford, CA).
- Georges RA, Jones MO (1980) *People Studying People: The Human Element in Fieldwork* (University of California Press, Berkeley).
- Gieryn TF (1983) Boundary-work and the demarcation of science from non-science: Strains and interests in professional ideologies of scientists. *Amer. Sociol. Rev.* 48:781–795.
- Glaser B, Strauss AL (1967) *The Discovery of Grounded Theory* (Aldine, Chicago).
- Goffman E (1989) On fieldwork. *J. Contemp. Ethnography* 18(2):123–132.
- Golden-Biddle K, Locke K (1993) Appealing work: An investigation of how ethnographic texts convince. *Organ. Sci.* 4(4):595–616.
- Golden-Biddle K, Locke K (2007) *Composing Qualitative Research* (Sage, Thousand Oaks, CA).
- Hallett T, Ventresca MJ (2006) Inhabited institutions: Social interactions and organizational forms in Gouldner's patterns of industrial bureaucracy. *Theory Soc.* 35(2):213–236.
- Hayano DM (1979) *Poker Faces: The Life and Work of Professional Card Players* (University of California Press, Berkeley).
- Heaphy E, Dutton JE (2008) Positive social interactions and the human body at work: Linking organizations and physiology. *Acad. Management Rev.* 31(1):137–162.
- Hennion A (1997) Baroque and rock: Music, mediators and musical taste. *Poetics* 22(6):415–435.
- Ho K (2009) *Liquidated: An Ethnography of Wall Street* (Duke University Press, Durham, NC).
- Hughes EC (1974) Who studies whom? *Human Organ.* 33(4):327–334.
- Ibarra H (1999) Provisional selves: Experimenting with image and identity in professional adaptation. *Admin. Sci. Quart.* 44(4):764–791.
- Jackall R (1997) *Wild Cowboys: The Urban Marauders and the Forces of Order* (Harvard University Press, Cambridge, MA).
- Jeppesen LB, Lahkani KR (2010) Marginality and problem-solving effectiveness in broadcast search. *Organ. Sci.* 21(5):1016–1033.
- Jones GM (2011) *Trade of the Tricks: Inside the Magician's Craft* (University of California Press, Berkeley).
- Junker BH (1960) *Field Work* (University of Chicago Press, Chicago).
- Katz J (2004) On the rhetoric and politics of ethnographic methodology. *Ann. Amer. Acad. Political Soc. Sci.* 595(September):280–308.
- Kellogg KC (2009) Operating room: Relational spaces and microinstitutional change in surgery. *Amer. J. Sociol.* 115(3):657–711.
- Kenyatta J (1965) *Facing Mount Kenya: The Tribal Life of the Gikuyu* (Vintage Books, New York).
- Knee JA (2006) *The Accidental Investment Banker: Inside the Decade that Transformed Wall Street* (Oxford University Press, New York).
- Kunda G (1992) *Engineering Culture: Control and Commitment in High-Tech Corporation* (Temple University Press, Philadelphia).
- Lamont M (2009) *How Professors Think: Inside the Curious World of Academic Judgment* (Harvard University Press, Cambridge, MA).
- Leidner R (1993) *Fast Food, Fast Talk: Service Work and the Routinization of Everyday Life* (University of California Press, Berkeley).
- Lewis D (1973) Anthropology and colonialism. *Current Anthropol.* 14(5):581–602.
- Locke K, Golden-Biddle K, Feldman MS (2008) Making doubt generative: Rethinking the role of doubt in the research process. *Organ. Sci.* 19(6):907–918.
- Lofland J, Snow DA, Anderson L, Lofland LH (2006) *Analyzing Social Settings: A Guide to Qualitative Observation and Analysis* (Thomson Wadsworth, Belmont, CA).
- Luker K (2008) *Salsa Dancing into the Social Sciences: Research in an Age of Info-Glut* (Harvard University Press, Cambridge, MA).
- Malinowski B (1989) *A Diary in the Strict Sense of the Term* (Stanford University Press, Stanford, CA).
- Marcus GE, Fischer MF (1986) *Anthropology as Cultural Critique: An Experimental Moment in the Human Sciences* (University of Chicago Press, Chicago).
- Marcus GE, Kushman D (1982) Ethnographies as text. *Annual Rev. Anthropol.* 11:25–69.
- Mears A (2011) *Pricing Beauty: The Making of a Fashion Model* (University of California Press, Berkeley).

- Michel AA (2007) A distributed cognition perspective on newcomers' change processes: The management of cognitive uncertainty in two investment banks. *Admin. Sci. Quart.* 52(4):507–557.
- Mills MA (1990) Critical theory and the folklorists: Performance, interpretive authority, and gender. *Southern Folklore* 47(1):5–15.
- Mirvis PH, Louis MR (1985) Self-full research: Working through the self as an instrument in organizational research. Berg DN, Smith KK, eds. *Exploring Clinical Methods for Social Research* (Sage, Beverly Hills, CA), 229–246.
- Morrill C (1995) *The Executive Way: Conflict Management in Corporations* (University of Chicago Press, Chicago).
- Morrison E (2010) OB in AMJ: What is hot and what is not? *Acad. Management J.* 53(5):932–936.
- Morrison EW (2002) Newcomers' relationships: The role of social network ties during socialization. *Acad. Management J.* 45(6):1149–1160.
- Moskos P (2008) *Cop in the Hood: My Year Policing Baltimore's Eastern District* (Princeton University Press, Princeton, NJ).
- Moskos P (2011) In defense of doing nothing: The methodological utility of introversion. Zake I, DeCesare M, eds. *New Directions in Sociology: Essays on Theory and Methodology in the 21st Century* (McFarland, Jefferson, NC), 160–171.
- O'Mahony S (2007) The emergence of governance in an open source community. *Acad. Management J.* 50(5):1079–1106.
- Orr JE (1996) *Talking About Machines: An Ethnography of a Modern Job* (Cornell University Press, Ithaca, NY).
- Perlow LA (1999) The time famine: Towards a sociology of work time. *Admin. Sci. Quart.* 44(1):57–81.
- Powell WW, Colyvas J (2008) Microfoundations of institutional theory. Greenwood R, Oliver C, Suddaby R, Sahlin-Andersson K, eds. *Handbook of Organizational Institutionalism* (Sage, London), 276–298.
- Pratt MG (2000) The good, the bad, and the ambivalent: Managing identification among Amway distributors. *Admin. Sci. Quart.* 45(3):456–493.
- Pratt MG (2003) Access as relating: On the relationship aspects of different types of access. Feldman M, Bell J, Berger MT, eds. *Gaining Access: A Practical and Theoretical Guide for Qualitative Researchers* (Altamira Press, Walnut Creek, CA), 150–154.
- Pratt MG, Rockmann KW, Kaufmann JB (2006) Constructing professional identity: The role of work and identity learning cycles in the customization of identity among medical residents. *Acad. Management J.* 49(2):235–262.
- Rabinow P (1977) *Reflections on Fieldwork in Morocco* (University of California Press, Berkeley).
- Read KE (1980) *The High Valley* (Columbia University Press, New York).
- Richards CE (1972) *Man in Perspective* (Random House, New York).
- Roethlisberger FJ, Dickson WJ (1939) *Management and the Worker: An Account of a Research Program Conducted by the Western Electric Company* (Harvard University Press, Cambridge, MA).
- Roy D (1952) Quota restriction and goldbricking in a machine shop. *Amer. J. Sociol.* 57(5):427–442.
- Roy D (1959) "Banana time": Job satisfaction and informal interaction. *Human Organ.* 18(4):158–168.
- Sanday PR (1979) The ethnographic paradigm(s). *Admin. Sci. Quart.* 24(4):527–538.
- Scheper-Hughes N (2001) *Saints, Scholars, and Schizophrenics: Mental Illness in Rural Ireland* (University of California Press, Berkeley).
- Shelemay KK (1991) *A Song of Longing: An Ethiopian Journey* (University of Illinois Press, Urbana).
- Spradley JP (1979) *The Ethnographic Interview* (Holt, Rinehart and Winston, New York).
- Stacey, J (1991) Can there be a feminist ethnography? Gluck SB, Patai D, eds. *Women's Words* (Routledge, New York), 111–119.
- Strauss A, Corbin J (1998) *Basics of Qualitative Research: Techniques and Procedures for Developing Grounded Theory* (Sage, Thousand Oaks, CA).
- Suddaby R (2006) What grounded theory is not. *Acad. Management J.* 49(4):633–642.
- Sutton RI (1997) The virtues of closet qualitative research. *Organ. Sci.* 8(1):97–106.
- Taussig M (2006) *Walter Benjamin's Grave* (University of Chicago Press, Chicago).
- Thornborrow T, Brown AD (2009) "Being regimented": Aspiration, discipline and identity work in the British parachute regiment. *Organ. Stud.* 30(4):335–376.
- Van Maanen J (1975) Police socialization: A longitudinal examination of job attitudes in an urban police department. *Admin. Sci. Quart.* 20(2):207–228.
- Van Maanen J (1990) The smile factory: Work at Disneyland. Frost PJ, Moore LF, Louis MR, Lundberg CC, Martin J, eds. *Reframing Organizational Culture* (Sage, Thousand Oaks, CA), 58–76.
- Van Maanen J (2011a) Ethnography as work: Some rules of engagement. *J. Management Stud.* 48(1):218–234.
- Van Maanen J (2011b) *Tales of the Field: On Writing Ethnography*, 2nd ed. (University of Chicago Press, Chicago).
- Van Maanen J, Kolb D (1985) The professional apprentice: Observations on fieldwork roles in two organizational settings. Bacharach SB, Mitchell SM, eds. *Research in the Sociology of Organizations*, Vol. 4 (JAI Press, Greenwich, CT), 1–33.
- Van Maanen J, Schein EH (1979) Toward a theory of organizational socialization. Staw BM, Cummings LL, eds. *Research in Organizational Behavior*, Vol. 1 (JAI Press, Greenwich, CT), 209–264.
- Vaughan D (1990) *Uncoupling: Turning Points in Intimate Relationships* (Vintage Books, New York).
- Visweswaran K (1994) *Fictions of Feminist Ethnography* (University of Minnesota Press, Minneapolis).
- Wacquant L (2003) *Body and Soul: Ethnographic Notebooks of an Apprentice Boxer* (Oxford University Press, New York).
- Warren CAB, Rasmussen PK (1977) Sex and gender in field research. *Urban Life* 6(3):349–370.
- Weber M (2004) *The Vocation Lectures* (Hackett Publishing, Indianapolis).
- Weeks J (2004) *Unpopular Culture: The Ritual of Complaint in a British Bank* (University of Chicago Press, Chicago).
- Weiss RS (1994) *Learning from Strangers: The Art and Method of Qualitative Interview Studies* (Free Press, New York).
- Whyte WF (1993) *Street Corner Society: The Social Structure of an Italian Slum* (University of Chicago Press, Chicago). [Orig. pub. 1943.]
- Wrzesniewski A, Dutton JE, Debebe G (2003) Interpersonal sense-making and the meaning of work. Staw BM, Kramer RM, eds. *Research in Organizational Behavior*, Vol. 25 (JAI Press, Greenwich, CT), 93–135.

- Zelizer VA (2009) Intimacy in economic organizations. Bandelj N, ed. *Research in the Sociology of Work: Economic Sociology of Work* (Emerald Group Publishing, Bingley, UK), 23–55.
- Zelizer VA (2010) *Economic Lives: How Culture Shapes the Economy* (Princeton University Press, Princeton, NJ).

Michel Anteby is an associate professor in the organizational behavior unit at Harvard Business School. His research

examines organizational and occupational cultures, particularly their interplay with morals. Recently studied cultures include those of U.S. clinical anatomists and French aeronautics workers. He is the author of *Manufacturing Morals: The Values of Silence in Business School Education* (University of Chicago Press, 2013), a book that hopefully upholds distance and involvement.