



Reinventing and Reinvesting in the Local for Our Common Good

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Edited by Brian A. Hoey

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Doing Ethnography to Connect, Exchange, and Impact

Brian A. Hoey

Connecting: What is Ethnography?

The term ethnography has come to be equated with virtually any qualitative research project where the intent is to provide a detailed, in-depth description of everyday life and practice. This wide claim of “ethnography” as a label to categorize all such research may, in fact, be too liberal in its application. Within the field of anthropology, an attempt to authentically render culturally-informed lived experience in written account is often referred to as “thick description,” a term generally attributed to the anthropologist Clifford Geertz (1973) writing on what he described as an interpretive theory of culture in the early 1970s.¹ The term “ethnography,” in fact, is meant to convey how this methodology unavoidably entails the act of rendering—in all senses of that evocative word—varied cultural lives into the written word.² As suggested by Renée Fox (2004, 311), I tend to reserve the term *ethnography* for qualitative research that involves some manner of “prolonged immersion in the field and continuous, face-to-face interaction with informants [. . .] that results in the generation of massive amounts of ‘thickly descriptive’ data, in a potentially narrative form, that provide an intimate view of what is being studied . . . [and to] distinguish it from *non-ethnographic* qualitative research that employs observation and interviewing methods in more circumscribed, short-term, distant, and ‘thin’ ways.”

Use of the term “qualitative,” as above, is typically meant to distinguish this kind of social science research from those projects considered “quantitative” in nature. As we might expect, the *quantitative* label suggests research more fully dependent on numbers such as statistically-driven approaches to data collection and analysis. Within such research, numeric data allows for comparatively rapid collection from much larger samples (the individuals and/or groups in a given study) as well as swifter analysis and representation of data than would be practically possible (or even desirable) within ethnographic fieldwork, especially ethnographic fieldwork that is truly “thick” in its description. Quantitative and the qualitative approaches, while potentially complimentary in usage, have a broad range of differences that I will not be discussing.

While an ethnographic approach to social research is no longer exclusively that of the cultural anthropologist, I tend to seek an understanding rooted in ethnography’s disciplinary home. Anthropologists typically speak of ethnography as a particular qualitative research *process* (one conducts an ethnography) as well as a *product* (the written outcome is an ethnography), the aim of which is cultural interpretation. The ethnographer goes beyond simply reporting events and details of experience as we might, perhaps too simplistically, expect in the somewhat more (and deliberately) circumscribed role of a documentarian. Specifically, he or she attempts to explain how various observed and derived details of fieldwork represent what we might call “webs of significance,” as was famously suggested by Geertz (1973, 5) in his phrasing for the complex, interconnected, and historically contingent cultural constructions in which we all live our lives.

Ethnographers endeavor to generate understandings of culture through depictions of what we may call an *emic* perspective, or what is often described as an “insider’s point of view.” The emphasis in

this representation is on allowing critical categories and meanings to *emerge* from the ethnographic encounter rather than imposing these from pre-existing models. An *etic* perspective, by contrast, refers to a more distant, analytical orientation to the experience of fieldwork research. The linguist and anthropologist Kenneth Pike (1954, 8) devised these concepts by drawing on the linguistic terms of “phonemic” and “phonetic,” respectively. In so doing, he suggested an original focus on the meaning of sounds within a given linguistic and cultural milieu or system in the *emic* perspective and a focus on universal functionality of sounds, without reference to embedded meaning, in the *etic* perspective. Pike initially described how an *emic* approach was an attempt to discover and describe particular linguistic patterns in terms of the broader context of a given language or culture as encountered directly in the field. This is in contrast to a primary concern for generalizable statements about such data in an *etic* approach. Such generalizable statements are intended to provide truly broad (i.e., global) classification and, importantly as we shall see below, *comparison* to a system of knowledge created prior to the particular fieldwork encounter. Following this distinction between *etic* and *emic*, an ethnographic understanding is developed through close exploration of a variety of sources of and approaches to data, while always relying on a cultural frame of analysis and interpretation.

In considering Pike’s notable contribution to ethnographic vernacular and practice, we are introduced to what may well be an inevitable and, for many, necessary component of that practice. Anthropology is commonly described to undergraduate students in introductory textbooks as being fundamentally comparative in nature. Specifically, cultural anthropology is often defined as consisting of the fieldwork methodology of ethnography operationally paired with the analytical and theoretical work enabled by

cross-cultural comparison of ethnographically-derived data on specific cultural groups—termed “ethnology” by many authors. For those who adopt this particular understanding of the conduct of cultural anthropology, comparison is an explicit component, at least in its future application, of the conduct of ethnography and of the representation of the findings of fieldwork research in ethnographic literature. That is to say, the work necessarily entails the identification and some manner of reification of what are taken to be meaningful social, cultural, and conceptual categories (derived from the local) with the purpose of establishing the strategic grounds on which we will speak, as anthropologists, of “similarities” and “differences” between human groups and, in so doing, come to some better appreciation of what it means to be human. An appreciation of what some, in fact, might refer to as the “human condition.”

There is a persistent and widely-shared understanding that this is how the field is and should, generally, be portrayed, as well as an extensive history of purposeful comparisons in the literature of the discipline that are at times employed to create what are now long discredited rankings of “cultures” as well as relativistic comparisons of observed cultural patterns. However, the use of such explicit comparison between what are taken to be distinct cultural groups has for some time lost its appeal for many card-carrying anthropologists following the post-structuralist period of the 1970s and ’80s. Nevertheless, comparison arguably remains an inalienable part of the work of ethnography, even if the subjects and objects being compared have shifted and the comparison may now serve different purposes. For much contemporary ethnography, my own included, comparison today incorporates what may be either an implicit or explicit dependence on the embodied subject-position of the ethnographer him or herself that corresponds with his or her particular cultural knowledge and point of view in what may be understood

(if not exactly depicted) as an unavoidable (and, yes, comparative) place from which our research must begin. At the same time—in a manner further relevant given the particular theme of the volume to which this chapter contributes—much ethnography at the moment is framed in terms that decisively juxtapose what are taken, on the one hand, as “global” or at least broadly “non-local” and typically reified and disembodied forces such as “modernization,” against what may be described as, at times, resilient and, at others, accommodating strategies taken by persons observed at the “local” level where any impact of such otherwise abstract “forces” are experienced by everyday people. This is to say, in part, that in today’s ethnography, comparison tends to productively complicate rather than reductively simplify.

Long-term engagement in the ethnographic fieldwork setting is often termed “participant-observation.” We may think of ethnographic research as a continuum wherein there are more or less intense or committed relationships between the ethnographer and those persons who could call “home” what we as field researchers would refer to as “the field.” Some projects are intended only as “rapid assessment,” entailing only brief, highly focused, decidedly purposeful encounters in the context of what is typically policy-targeted, community-based engagements. Other projects, more typically “academic” in nature, may require months or even years of gradual relationship building and exploration to come to some kind of fulfillment, including applications beyond dissemination of findings alone. For the most part, I am trained in and speaking to particulars that more readily characterize projects consisting of lasting commitments. It is in the practice of participant-observation that more fully adheres to the long-term end of the ethnographic continuum that we find the primary source of rich ethnographic data and the thick description referred to earlier.

Bronislaw Malinowski, the British-trained anthropologist whose work in an early period of professionalization in the discipline during the first half of the twentieth century helped to define the practice, asserted that the ethnographer should not stand apart from those studied. He advocated for engaging participation at a time when some other scholars adhered more strictly to dispassionate observation—even avoiding, in some cases, contact with the field and the actual people under study in favor of using whatever data could be brought to them from sites both near and far. In so doing, these scholars practiced what came to be referred to generally, and pejoratively, as “armchair” anthropology for those most distant from the action and “verandah” anthropology for those who were in-country but happy to remain comfortably ensconced on the well-appointed front porches of current or former colonial powers from whom many anthropologists, of all stripes, very likely received funds for their work.

Writing in the forward to what might be his most well-known ethnography, Malinowski (1922, 3) asserts that “I consider that only such ethnographic sources are of unquestionable scientific value, in which we can clearly draw the line between, on the one hand, the results of direct observation and of native statements and interpretations, and on the other, the inferences of the author.” The term “participant-observation” is meant to convey what is generally understood to be the dual-role played by an ethnographer. In an explanation of ethnographic fieldwork common to anthropological textbooks, students are told that in order to develop an understanding of what it is like to live and work in a given setting—a particular cultural context—the researcher must become a willing, empathetic *participant* in the life of the setting even while maintaining what might be construed as a simultaneous stance of *observer*, or someone who ultimately describes and thus represents the experience with a

measure of what we might call “detachment.” Such a thoughtfully positioned researcher—operating in the moment of representation within lines clearly drawn—could, it is presumed, provide the sort of ethnographic account that Malinowski would find of undeniable scientific value.

What might not be clearly conveyed to the novice, or even appreciated by Malinowski, however, is that this position is founded on a dynamic, necessarily self-reflexive relationship that must be continually re-balanced by the ethnographer in an ongoing process of engrossing discovery that entails learning not only about people who may be thought to constitute “the Other,” and whose degree of “otherness” is purposefully diminished over time, but also learning about “the Self.” Barbara Tedlock submits that cultural anthropology since the days of Malinowski has shifted from a largely unexamined reliance on participant observation to a critical “observation of participation.” Whereas participant observant ethnographers, as I have suggested, have been tasked to attempt simultaneous engagement and dispassion, in the observation of participation “ethnographers both experience and observe their own and other’s *co-participation* within the ethnographic encounter [. . . in what constitutes] a representational transformation in which, instead of a choice between writing an ethnographic memoir centering on the Self or a standard monograph centering on the Other, both Self and Other are presented together within a single narrative ethnography, focused on the character and process of the ethnographic *dialogue*” (Tedlock 1991, 69; emphasis added).

I think that most contemporary cultural anthropologists, at least, would assert that a dualistic role for the researcher—involving at least some measure of the seeming obligatory “distance” that comes with endeavors of a “true” science—does not mean that ethnographers ultimately cannot also speak to the potentially transformative

nature of fieldwork or, in being transformed as persons while in the field, become willing advocates for people who are the “subjects” of their study. Speaking of a broad call within anthropology for greater perceived relevancy to the public, Barbara Rylko-Bauer (2006, 186) and her co-authors assert that, in order to achieve this goal, ethnographers and others must have “a willingness to take stands on pressing human issues, to be ethically and politically subjective while methodologically objective, and to accept advocacy (however it is being defined) as part of a disciplinary framework that already values theory and research excellence.”

For at least some of the students in my introductory classes, the concept of “cultural relativism” may be interpreted inappropriately as a *moral* relativism. In my attempts to clarify how anthropologists operationalize the concept in the field, I refer to how cultural relativism should serve as a *methodological* relativism that allows for greater empathy and understanding on the part of the researcher. This candidness further assists in creating the basis for an essential “rapport” between people—working together equitably—that comes with mutual trust. In ethnography generally, the “subjective” and “objective” need not be mutually exclusive. In what could be called *critical* ethnography, in particular, they cannot. In the critically-engaged ethnographer’s eye, concern for power, privilege and/or biases (what might be called the “positionality”) of the ethnographer in relationships with others in the field helps drive a self-reflexive interplay or engagement with participants in research (who may in this particular methodological context be called “collaborators” or “consultants”) that is manifest through an open, ongoing dialogue that shapes the meaning and direction of research (e.g., see Lassiter, Hoey, and Campbell 2020). This collaborative approach can extend further still to encompass the manner of dissemination and application of the products of research.

As suggested by Elizabeth Campbell and Luke Eric Lassiter “the explicitly intersubjective practice of contemporary critical ethnography [. . .] brings formerly partitioned processes into inevitable confluence” (2010, 378). Quoting George Marcus (1999, 18 in *ibid.*), Campbell and Lassiter note that for what might be a majority of ethnographers today, “having to shift personal positions in relation to one’s subjects and other active discourses in fields that overlap with one’s own, generates a sense of doing more than just traditional ethnography, and it provides a sense of being an activist in even the most ‘apolitical’ fieldworker.” Typically, ethnographers spend many months or even years in the places where they conduct their research, often forming lasting, even lifetime, bonds with people with whom they work in the field. The significance of what I take to be the unavoidable personal involvement of the ethnographer is something to which I return later.

Due to historical development and disciplinary biases, in the past many anthropological ethnographers conducted their research in foreign countries while largely discounting the potential for work at home. I experienced bias first hand as a graduate student in the mid-1990s at the University of Michigan. At least one potential committee member rejected my invitation to serve as advisor on my proposed research in the American Midwest on the grounds that my proposed research—among middle-class, white Americans—was inherently a “less than” form of ethnographic research when compared to what I was made to understand was innately more interesting and important work that could be conducted among an axiomatic Other in distant, self-evidently exotic lands abroad.³ If all else failed, a would-be cultural anthropologist might consider a domestic project, but only as might be established in some distinct “subculture” of broader American society in which the researcher could not claim membership. This is at least partly why much ethnographically-oriented

research in the United States has been done outside of its disciplinary home. Increasing numbers of cultural anthropologists, however, are now doing fieldwork in the communities where they themselves live and work—carrying on where earlier anthropologists have always been, but whose work, with some notable exceptions, has received less attention than those doing research abroad. Or, as I suggested, doing work among subcultural groups at home who are held as somehow mysterious or significantly different from either the putative mainstream of society or, potentially, from the researcher.

Ethnographers collect data that depend on the specific nature of the field setting and, to varying degrees, on the particulars of the project including initial objectives and orienting questions. In addition to such things as observations of behavior, recordings of conversations (including what may be formal, but typically “open-ended” or “unstructured” interviews), and photographs, data may take the form of government reports, newspaper and magazine articles, and representative artifacts that are interpreted to embody characteristics of a topic of interest. Although they may not be tied to the site of study, secondary academic sources may be utilized to “locate” the specific study in terms of theory, methods, population, or geography (among other aspects) within an existing body of literature. An essential source—and one that may go wholly or, at least, largely unacknowledged—is the ethnographer him or herself. I will now turn to the ethnographer as person and discuss more fully the unavoidable centrality of the researcher in the conduct of this particular form of fieldwork.

Exchanging: What is the Ethnographer’s Relationship to the Practice of Ethnography?

I like to start my undergraduate course in ethnographic methods by having students define what they believe constitutes a “method.”

A stimulating and free-wheeling discussion typically ensues. In the context of this conversation, having identified most of the elements that we would, as scientists, consider to be essential parts of a broad multi-disciplinary definition, we turn somewhat implicitly to thinking about the researcher positioned as “animator” in the conduct of fieldwork research that is often seen as a lifeless methodology. At this point, I have taken some pleasure in informing my students that they are, manifestly, the primary tool of their ethnographic research. At least one student has, understandably, taken issue with the implication that he was, in any way, a “tool.” Such things as “interviewing” or even “participant-observation” are often described as individual tools in a reputed methodological “toolkit” figuratively lugged by the ethnographer into the field to enable what might be envisioned as interpersonal procedures of a fundamentally mechanical nature. However, I am committed to the idea that the principal tool—if we are to speak at all of such a thing—must be understood as the ethnographer as a living, breathing, and feeling person engaged in meaningful relationships with other equally real persons. Typically, at about this time in our collective musings, I pull out my dog-eared copy of *Stranger and Friend*, the anthropological memoir of Malinowski’s student Hortense Powdermaker and read aloud the following passage from her opening, background chapter.

The anthropologist is a *human instrument* studying other human beings and their societies. Although he [*sic*] has developed techniques that give him considerable objectivity, it is an illusion for him to think he can remove his personality from his work and become a faceless robot or machinelike recorder of human events. It is important to accept that this human instrument is as much a product of biological, psychological, and social conditioning as are the people he studies (1966, 19; emphasis added).

During the last few decades of the twentieth century, interest has grown noticeably within anthropology for considering the close relationship between personal history, motivation, and the particulars of ethnographic fieldwork. This turn in the discipline was precipitated by several strands of critical self-examination and may as well have shared origins with a simultaneous movement to which Lewis Langness and Gelya Frank refer in the opening to their book *Lives: An Anthropological Approach to Biography*. In this work, Langness and Frank address the rising use of a life-history approach within a decidedly “person-centered” ethnography described as “a rigorous yet compassionate effort on the part of American scholars and others to portray the lives of ordinary individuals [. . .] with the kind of perceptiveness and detail that transform a stranger we might meet in our personal lives into a friend” (1981, 1).

While Langness and Frank are referring to the subject(s) of ethnographic inquiry, which is to say the people with whom we work in the field, they also speak to a larger turn in the discipline toward “reflexivity”—the principle that the same theories of knowledge used to understand others can be self-consciously applied to understanding the construction of those theories themselves, if not also ourselves as willful participants in this construction. A person-centered research focus is thought to reveal, through intimate personal details, broadly relevant features of the culture and society that shape the conditions that give rise to characteristic life histories. The approach is naturally *biographical* in nature. At the same time, Langness and Frank explain that “Getting to know any person in depth is a major experience [for both parties] because we have to admit that another way of structuring the world truly exists” (ibid.). Thus, in what must be acknowledged as a shared experience, we are presented with the fundamentally *autobiographical* as when the ethnographer him or herself turns to examining the significance of their own involvement in the lives of others and their positionality, at least partly, relative

to the persons with whom they are working. Here we understand that this encounter among persons, and any account that emerges as a distinct life history, for example, is a complex, self-constituting *negotiation* between people with their own variously shared and distinct needs and desires.

A volume suggestively titled *Anthropology and Autobiography*, edited by Judith Okely and Helen Callaway (1992), helped to frame an emerging debate about reflexivity and the professional ethical obligations of the ethnographer who, despite being long proclaimed *participant*-observer, had historically made only limited, formalized appearances in the products of those works. That is to say, the ethnographer may have been presented as one of the actors on the stage, but we were given little insight into his or her background (what might be going on “backstage”) or sense of an inner life in the manner that we have come to expect of “others” portrayed in an ethnographic account.

I first took seriously the relationship between life story, fieldwork, and scholarship when constructing an intellectual biography of anthropologist Roy Rappaport for a posthumous *American Ethnologist* article based on his fond, end-of-life recollections as well as tender regrets of fieldwork in Papua New Guinea as he succumbed to lung cancer in 1997 (Hoey and Fricke 2007). I came to understand that it is undeniably important to question and understand how these elements have bearing on the construction of theory and, ultimately, the conduct of an academic life. What I learned from my experience working with Rappaport and my co-author, Tom Fricke, was that unforeseen encounters along circuitous paths, personal and professional experiences, together with historical context, lead individual researchers to their particular topical foci and a set of methodological and theoretical approaches.

Roger Sanjek (2014; see also Sanjek 2015) also argues that the anthropologist as ethnographer and social theorist exerts an autobiographical agency by virtue of how one’s past motivates and thus

shapes present choices. These choices include what issues to study; how to interpret significance in conversations, observed events, and experiences while in the field (at least some of which come from the unique sociocultural “terrain” of the field site itself); and ultimately how to engage with one’s scholarly audience and a greater public. Sanjek holds that “ethnography is inescapably lodged in the social worlds of those who use it” (ibid., ix), but that this is appropriate given that ethnographers today work to reveal and, to whatever extent possible, control or at least account for and not deny, their possible biases. In his own case, Sanjek asserts a “cohort effect” associated with coming of age as an anthropologist in 1960s New York City at Columbia University surrounded by some of the most influential contributors to our field, including most conspicuously Marvin Harris—an effect shared, I will note, with Rappaport.

Theory too, Sanjek avows, is autobiographical as it is critical in shaping and molding the ethnographic process, just as fieldwork enables us as researchers to develop theory. In some ways, the most compelling aspect of *Ethnography in Today’s World* is Sanjek’s autobiographical tales of a prominent anthropologist born out of the urban, counter-cultural tumult of the civil rights era who matured to navigate and respond to the theoretical storms and impact of 1980s postmodernism—at least some of which he found agreeable, for example, in the call for more critically self-conscious approaches. However, he decries much of this turn toward reflexivity as leading to lost relevance for the discipline outside of (and even within) academia as a result of postmodernism’s most ardent proponents deciding to abandon traditions of broad contextualization (i.e., tracing layers of history and political economy in the setting of complex global flows) and comparative analysis (i.e., where an outstanding problem of theory is systematically addressed using ethnographic data from different places and times).⁴

In order to appreciate the extent to which the ethnographer is personally involved in long-term fieldwork engagement, I will at least begin to explore an intersection between the lives of those participating in the production of knowledge in and through ethnographic fieldwork—that is to say, both the ethnographer and those with whom he or she necessarily collaborates as voluntary participants in that work. Therefore, I must consider ways in which the researcher may be personally challenged and changed by the experience of fieldwork as well as how fieldwork can be informative to personal narratives—the life stories—of those engaged in it as participants.

As I have clarified here, my position has long been that ethnographic fieldwork is shaped by personal and professional identities just as these identities are inevitably shaped by individual experiences while in the field. Unfortunately, the autobiographical dimension of ethnographic research has been downplayed historically if not discounted altogether. More recently, so-called autoethnography has emerged as a response, perhaps, to this possible failing within the literature as well as to introduce new—though not uncontroversial—dimensions to the range of practice of ethnographic fieldwork (e.g., Reed-Danahay 1997). I take contributions of this approach to be at least partly representative of recognition by these scholars of the inevitability of the Self in fieldwork generally as well as a specific contribution to the literature on ethnographic methods regarding another potential “instrument” in the putative, shared methodological “toolkit.” Unlike “self-narrative” writings such as memoir, autoethnography explicitly applies a cultural analysis and interpretation of the researcher’s own behaviors, thoughts, and experiences while engaged with others in a given sociocultural context.⁵ That is to say, proponents of this method employ much the same means—at least analytically speaking—and have the intent to arrive at the same ends as more traditional ethnographic research.

For the most part, and despite the development of autoethnographic studies, the autobiographical continues to be restrained in accounts of ethnographic fieldwork. This is perhaps the consequence a perceived threat to the objectivity that most people expect of a legitimate science and to the supposed reliability of our data if we appear as researchers to permit subjectivity to intervene by allowing the ethnographer's encumbered persona to appear instead of adhering to the presumed role of a largely (if not wholly) dispassionate observer. But can it ever be said of any research—whether in the field or in the lab—that emotion is not constitutive of practice? That is to say, emotions are not simply a consequence of the practice of research—or, in the context of my discussion, at least, something that merely *happens* to us as researchers in the field (cf. Davies and Spencer 2010). As described by Robert Solomon (1978, 187), emotion is “a network of conceptual and perceptual structures in which the objects and people in our world, others' actions and our own, are given significance.”

This simple truth was—in the context of ethnographic fieldwork—brought to revealing and heart-wrenching light in the account of Renato Rosaldo's (1993) research among Ilongot people in the Philippines where he lost his wife, Michelle, to a horrific and sudden death by falling from a cliff while they were together in the field. Rosaldo conveys that it was only through his profound loss, the experience of what he describes as an “emotional force of bereavement,” and the subsequent change in his subject position relative to the Ilongot behaviors that he observed in the field (including the practice of grieving Ilongot men taking human heads), that he was able to grasp the significance of his own observations, the motivations of the Ilongot, and the need for cultural descriptions to seek out and convey qualities of emotional force as well as the representational “thickness” to which Geertz refers. For Rosaldo (1993, 2), a

“gradual thickening of symbolic webs of meaning” alone and without emotion, may not lead to sufficient elaboration and subsequent understanding.

With the simplicity of Solomon’s understanding regarding the formative nature of emotion in our understanding of the world and the force of Rosaldo’s illustration, we may reflect generally on how we interpret events and find meaning as researchers at least partly through emotion, whether boredom or surprise, fear or delight. That is, emotion is not merely a reaction to what happens to us. This is true of people generally, whether researchers or not. In the context of research, the very questions that we seek to answer through our work express both professional and personal desires, if even it makes sense to distinguish these as independent domains. In the case of ethnographic fieldwork, we seek so that we may find—or not find—answers to our open questions through our experience as human beings participating in relationships with other human beings. Our seeking unavoidably entails emotion and, thus, personal involvement such that, more so than with many other methodologies, the personal and the professional are only artificially and retroactively separated.

Most anthropologists today point to Malinowski as a kind of “founding father” to ethnographic fieldwork and the practice of participant-observation. Malinowski’s early twentieth-century ethnographies were written in a voice removed and largely unrevealing about the ethnographer in the context of his real or imagined relationships to people that he studied. Since Malinowski’s time, the personal account of fieldwork has been customarily hidden away in unpublished marginal notes and diaries. These “off the record” writings, however, document tacit impressions and emotional experiences without which we cannot, as ethnographers, fully appreciate and understand the project of our research itself. For many ethnographers,

then as today, “fieldnotes” (to which I return later) are composed of what might be thought of as that writing that is thought to constitute “data.” All other writing, including a “diary” of experiences, would be considered separately as the “non-data” elements of being human as a researcher in the context of doing scientific fieldwork.

My position has always been that the accounts we provide of our fieldwork in the form of our findings are based on information we have been able to gather only through investing ourselves in real, human relationships. These fieldwork relationships may not be entirely “normal” by the measures of any person’s everyday life, given the particular circumstances for their formation and continuance within the purposeful nature of a research project—just as an interview isn’t a typical conversation—but they are relationships just the same. Therefore, the emotionally informed nature of these relationships is arguably as significant to the ethnographic writing we produce as are the data we collect. Without one, we would not have the other. Despite what Malinowski contributed to the practice of cultural anthropology in terms of defining an intersecting, simultaneous role of participant-observer, his countervailing legacy to the ethnographic method is an artificial and retroactive separation between the “fieldwork” experience and the (often geographically, as well as emotionally, distant) experience of “writing up,” the results of that work in the professionally acceptable format required for scholarly dissemination.

Although, as I have stressed, what we know as ethnographers is inseparable from our relationships in and out of the context of fieldwork, much ethnographic writing does some harm to those relationships by imposing or, at least, re-imposing boundaries between self and other. This creates a tension to which we might, increasingly, expect a response by our coparticipants. Because of the very real familiarity of these relationships and the expectations that come

part and parcel in the context of such significant contact between people, those with whom we work in the field can and do, more and more, confront representations of our relationships and their lives and may find some fault. This confrontation may be born of a serious sense of broken confidence or, perhaps, a violation of complex realities of particular lived experience and relationships that we—as ethnographers and as human beings—claim to, at least partly, share.

As for Malinowski, his emotive diaries—replete with feelings of deep loneliness, self-doubt, sexual frustration, and fear—were published only after his death in a revealing autobiographical account of his inner life while in the field (*A Diary in the Strict Sense of the Term*, first published in 1967). No doubt these diaries were essential to Malinowski, even if he restricted them to tacitly shaping the form of writings and conclusions that would appear in publicly disseminated work. Among other things, we learn in these diaries that Malinowski longed to write great novels even as his scientific writing effectively defined the fieldwork approaches of cultural anthropology for much of the twentieth century. Malinowski was a storyteller in fact and at heart.

Of their possible lessons, Malinowski's diaries hold two of special relevance here. The first of these is that, fundamentally, ethnographic writing is a means of expressing a shared interest among human beings for telling stories—stories about what it means to be human. The second is that the explicit professional project of observing, imagining, and describing other people needs not be incompatible with the implicit personal project of learning about the Self. It is the dependable truth of fieldwork that these two projects—these two narratives—are always implicated in each other. Ethnographic fieldwork involves more than just the outward trials and tribulations of building rapport with “the locals” and getting to know “the local” whoever and wherever we find ourselves. In my ideal, at least,

it should entail an inward journey of self-discovery. Good ethnography recognizes the potentially transformative nature of fieldwork where as we search for answers to questions about other people we come to find ourselves in their stories.

What I am speaking to here is akin to what Carolyn Ellis, much as Tedlock, refers to as “interactive introspection” wherein “the researcher works back and forth with others to assist in their introspection, but the object of study is the emergent experiences of both parties. Interactive introspection provides self-introspection from subject and researcher, since a researcher must introspect about her own responses in reaction to experiences and feelings” of those with whom he or she works (Ellis 1991, 30). Regardless of the extent to which anyone is changed by the experience of their encounter, ethnography should be acknowledged as a mutual, exchanged product born of connected, intertwining lives of the ethnographer and those people on whom he or she come to rely while in the field. In this, as in much of what I am attempting to convey about contemporary ethnographic fieldwork, the person with whom we have a relationship in the field—who has generally been referred to as “Other,” to convey cultural distance, “subject,” to passively position relative to the act of research, or “informant,” to suggest a more active but still distant role—should rightly, in an inclusive and authentic way, be thought of as, at least, a “co-participant” in the process and very likely what Lassiter suggests should be a “co-researcher” (2005).

Despite pleadings of my undergraduate students for some kind of “formula” to save them from the apparent, near existential, angst associated with undertaking their own first ethnographic fieldwork projects, I have persisted in my conviction that ethnography is often less a method than it is what we might have to call an *anti-method*—at least insofar as we typically define a method as an organized plan that predetermines how something is done. That is to say, fieldwork

practice does not entail performing from a virtual script. Rather, it is more accurately and, arguably at its best, improvisational and dependent, in no small amount, on *serendipity*. In doing this work, then, we must be open and even “vulnerable.” Ruth Behar (1996) called for such vulnerability in her eloquently-stated call for a progressively more humanistic and impactful anthropology as compensation for what may have long been an excessive degree of abstraction and de-personalization in published ethnographic accounts.

Behar describes the practice of ethnography as an “irreversible voyage” where the ethnographer necessarily goes “elsewhere,” but never simply by making a physical trip to another place, and whose journey is captured in a reflexive portrayal in which the ethnographer “inscribes the self” into the account through the autobiographical. By being what we take as more “subjective” and, ultimately, “vulnerable,” Behar suggests a path toward a more truthful—or perhaps “authentic” in the manner most valued by presumptive objectivity in science—perspective from which to understand what we have come to learn from those with whom we work and, ultimately, from which to represent our own fieldwork experience. Importantly, there must be some kind of limits placed here in order for the product of such work to remain within the domain of social science. Speaking to such limits, Behar (1996, 14) rightly notes that “Vulnerability doesn’t mean that anything personal goes” but rather that “exposure of the self who is also a spectator [i.e., the observer] has to take us somewhere we couldn’t otherwise get to. It has to be essential to the argument, not a decorative flourish, not exposure for its own sake [. . . such that] a personal voice, if creatively used, can lead the reader, not into miniature bubbles of navel-gazing, but into the enormous sea of serious social issues.”

Ethnographic fieldwork is always a unique and emerging combination of the researcher and the particular circumstances and

personal relationships born of and in the field. So, I would counsel my students, you must embrace the serendipitous encounter that rouses subjective feelings more than you may feel the need to cling to what might seem reassuring, prescribed steps of a would-be dispassionate science. I contend that the emotional attachments we may make while in the field, together with our willingness to be honest with ourselves about the nature of our experience and their impact on our work, actually produce an understanding not only of others but also ourselves that is more sympathetic, humane, and ultimately accurate in terms of representing what it means to be human.

Impacting: What Kinds of Influence Does Such a Method Have?

Despite a broad public misconception about the discipline—perhaps born of partial understandings of the principle of cultural relativism—that ethnographers should have no lasting influence (either negative or positive) on those persons who are the subject of their studies, cultural anthropologists have long sought to have impact in the lives of others through varying degrees of collaboration in the conduct of fieldwork with the intent, for example, of creating action plans that may affect public policy and change the course of community development. But what of the unintended impact of our encounters on the lives of those with whom ethnographers work? These effects are typically unacknowledged given, perhaps, how they lie outside explicit research agendas. Do we take these effects to be simply things that may happen naturally in the dialogic exchange among mutually interested persons who may, over time, develop a relationship that goes beyond brief meetings contrived by the researcher to collect data in the context of research?

During my long-term project in Northern Michigan, I became (though only informally) interested in how the personal narratives of

project participants might be shaped through our relationship over time. My particular project may be largely responsible for this interest. As an examination of how people can deliberately use the act of relocation—often coupled with significant changes in work and family life—as a way of remaking self-identity, my study meant that I was working with people who were actively engaged in personal identity formation as a deliberative process. It seemed to me that as I worked with them, I became a part of their ongoing inner and outer conversations in this effort. While I clearly needed them for the purposes of my research, it seemed to me that they had a self-conscious need to engage in retrospective as well as prospective dialogue about their decision making. It seemed that I was serving a purpose in their lives—my role in the fieldwork relationship was valuable and valued.

I was purposefully seeking knowledge in a general, scientific way. I wanted to know why people were doing what they were doing. I was going about discovering what appeared to be the factors that shaped particular beliefs and behaviors over time. At the same time, the people with whom I worked were seeking personal insight and engaged in sense-making in a very purposeful way. Among other things, they wanted to know if what they were doing made sense to others and they wanted to learn what meaning their decisions might have in the broader social and cultural context that they believed I might—given my posture as a social scientist—understand somehow more fully or even, perhaps, dispassionately than them. That is, they appeared to seek what we might call, in everyday terms, “perspective.” At least initially, participants sought me out as some kind of impartial expert. More often than not, they told me that it was helpful to them to have someone outside family and friends who would listen, without judgment, to their stories of personal struggle. In not only listening but also in sharing my own personal struggles while in the field, I helped them to learn about others who made similar

decisions. This knowledge clearly helped to shape their continuously emerging sense of self (e.g., see Hoey 2005). As our encounters grew in number, they learned about my work with others and would tell me things like, “It’s good to know that we’re not alone.” We can easily find ourselves in this basic human desire for what could be called “belonging,” which was, itself, a basic motivation for many behaviors observed in my study population.

Without my suggestion, some came to see themselves while we talked over the course of many conversations as part of some kind of larger “movement” of people who were somehow challenging status quo assumptions about what it meant to live the “good life” in America at the start of the twenty-first century. I did not attempt to disavow them of this thought. For one thing, I wasn’t all that sure of my position; I was still learning. When I have followed up periodically over the years with these people who are now at least very good acquaintances if not true friends, they tell me that it is good to be reminded of their original plans and intentions as they set out to make lifestyle changes through relocation and remake work and family arrangements in the process. It seems that they have come to rely on me—in some small way, at least—to help find and maintain their bearings over time. I may provide a common thread as they go about the work of mapping out a trajectory for their present life, in part, out of what we have shared in the past. I know they found the times that we spent together meaningful and affirming to the narratives they have come to tell themselves about the purpose and direction of their lives. Indeed, accounts within the broad literature on qualitative research methods suggests that participants often find ethnographic interviewing provides opportunities for healthful introspection and what some might characterize as personal growth (e.g., Frank 2000; Ortiz 2001).⁶

The oral historian Valerie Yow (2005) speaks to meaning making in how people who are engaged in our research interpret their

experience and how researchers interject themselves in this process as both participant-observers and narrators. More broadly, any researchers—and perhaps especially ethnographers—by their very presence in the field help to shape the phenomena they observe. I have always contended that such “reactive effects” or “consequential practice”—as termed by Robert Emerson and his coauthors (2011) in their excellent book on the writing of fieldnotes—that is, how people respond to our presence, are themselves important forms of data that should not be seen as somehow “contaminating” what we may observe, experience, and learn in the field. Rather, these effects, as long as we become conscious of them, could well provide a source for our learning.

John Van Maanen reminds us that ethnographies are themselves narratives and—as with the narratives of individuals included within those texts—are experientially driven and purposefully shaped. The interpretive process entailed in going from fieldwork data to written account (i.e., what is ultimately disseminated to others in various ways) is about rendering the experiences of those participating in the research as well as those conducting that research into representative texts—a process that begins with capturing them in the context of our fieldnotes. As a broader context for this point, Van Maanen (1988, ix) has gone so far as to assert that ethnography “is the peculiar practice of representing the social reality of others through the analysis of one’s own experience in the world of these others.” This is very close to an observation by Clifford Geertz (1988, 10) that ethnography depends on “the oddity of constructing texts ostensibly scientific out of experiences broadly biographical.” Despite this odd or, perhaps, necessary tension, Geertz felt that ethnography rightly held a claim to truth about the nature of human life that research founded on exaggeratedly construed objectivity, characteristic of approaches akin to positivism, could not.

Although concerned specifically with documentary fieldwork,

Robert Coles has shown how any attempt at representation of lived experience is necessarily an interpretation despite the fact that there is a tendency among readers or viewers of such work to accept it at face value—to view it as a somehow autonomous reality. A child psychiatrist and author of numerous books concerned with human moral and spiritual reasoning, Cole suggests plainly that “objectivity” is a myth. Representation of life necessarily entails some subjective distortion given that the lived world is complex and ambiguous when compared with the relative simplicity and neatness required of an account of the research on which it is based. In the end, Coles (1997, 250) asserts that it is “Through selection, emphasis and the magic of narrative art, [that] the reader or viewer gets convincingly close to a scene, a subject matter and sees the documentary as one of many possible takes, not the story, but a story.”

Before turning, finally, to some practicalities of ethnographic fieldnotes upon which any account of the field is based, I would like to finalize my examination in this section by summarizing my point that, within this peculiar practice of ethnographic fieldwork, an entwining of narrative selves is arguably both necessary and desirable if we are to tell a story that convincingly reflects the reality of human social life. I understand that my position and the arguments for and against it are not entirely novel. Yet, it is helpful for me—if not those still in training—to be reminded that unique insight, however small, into what it means to be human may be found at the intersection of the biographical and the autobiographical in ethnographic fieldwork.

What is the Role of Fieldnotes in the Practice of Ethnography?

I have written this chapter both for my students (and they will likely hear what I have to say here in some form even if I don't hand them

a copy of this chapter to read) and for those who may be newly discovering or, perhaps, rediscovering the practice of ethnography. It is not meant to be a “how to” guide in any measure. Yet, I have intended to speak to how one might productively orient oneself to the practice and think about (as I tell my students) what it might “feel like” to do ethnography. My ideal is that this feeling should emerge in ethnographic practice that is fully engaged in what is likely (for my undergraduates) “the local” and in the particular manner that I have suggested in this chapter will capture at least some of its revealing (in the sense of discovering or recovering knowledge) and transformative (in the sense of having meaningful and practical impact that begins at the level of personal relationships born of the fieldwork encounter) potentials. Given that so much of ethnographic fieldwork depends on the researcher’s own experience and perspective, the “I” must be acknowledged. It really does matter where you as a researcher “stand” relative to the process of your own fieldwork and ultimately to the “subject” of your study. Such an understanding involves not only whether you might consider yourself an “insider” or an “outsider” to a group that may be your focus, but also the attitudes and/or preconceptions you bring to that study. This is true of any science regardless of whether a tension between “objectivity” and “subjectivity” is acknowledged or conveniently ignored as a non-issue after the proper rituals of research are performed. In any event, it is unavoidably true that there must be an acknowledged “I” in ethnographic fieldwork.

If you are judgmental in your treatment of what or whom you are studying, this will affect the product of your work by influencing the process—your capacity to accurately capture details thickly described in the fieldnotes that become your data, to interpret that data, and to represent (in some measure) the lives of others as well as the account of your fieldwork as something that you, yourself,

experienced. That much seems clear. However, it is more than this. I have found that many students in ethnographic training are reluctant at best and, at times, highly critical of the demands that ethnographic work places on them. Frankly, these students may resent the time and energy that doing this kind of fieldwork requires—especially within fragmented, overloaded schedules. Doing this work can disrupt one’s everyday life—not to mention a carefully manufactured plan for a semester. I must tell them that if they are judgmental of the process by being dismissive of the work that they are doing, this can be harmful and insidiously distortive. One needs to be open-minded and thus allow for possibilities for insight and discovery to emerge. If my students say “nothing happened” in their fieldnote journal for a given visit to a fieldwork site, they have likely shut off any possibility that there was, in fact, something there of significance to at least witness and even experience. We may attribute this rush to dismissive labeling of both observed phenomena and one’s own experience, at least in part, to not seeing what one has come to take for granted. This problem is likely compounded by familiarity with a local site and, in some measure, a contributing factor to the disciplinary bias for foreign sites to which I spoke earlier. That is to say, taking things for granted is an especially problematic tendency for those of us who work within our own culture(s) and communities.

Ethnographic fieldwork is challenging in a multitude of ways that are, frankly, not well understood to those who either know nothing of its practice or may know only enough to believe, wrongly, that it consists only of “hanging out, talking to people, and taking notes.” Ethnographic fieldwork is also immensely rewarding when we allow for its transformative potential. In the context of my undergraduate course in ethnographic methods, I ask students to keep certain things in mind. For example, while we can and should acknowledge our methodological and other challenges—e.g., these could become

at least some of the “limitations” of a study that are productively addressed within any report on that research—it is not a good idea to write in a consistently negative way about the work in which we are engaged. The real, emotionally-charged frustration experienced by students can lead to snap judgments and to thinking that tends to lump people and their beliefs and behaviors into stereotyped categories. It is entirely possible to have a less-than-stellar ethnographic fieldwork or fieldwork-training experience. This might be objectively measured by how well a student is able to practically collect sufficient data with which to work through cultural interpretation and analysis and whether they are able to draw credible, plausible, and possibly transferable conclusions from that work—that is, to produce an ethnography. At the subjective level, success may be measured by the student’s feelings about their fieldwork experience and, for example, whether it has lived up to their own expectations. When students arrive at the end of their time in the field (or at least in our semester-long ethnographic methods course together) and are weighed down by what are subjectively negative experiences, I reassure them that these experiences may still be analyzed for their potential contribution to a discussion about the emotional and practical challenges of ethnographic fieldwork generally. Simply stated, we can learn from challenging experience if it is examined for personal and professional insight.

One of the greatest challenges for students of ethnography is coming to understand that doing an ethnography is not at all like doing research based on books or articles—what is typically referred to as “secondary” research. Although as a student (and even a credentialed scholar) it is possible to neglect secondary research writing until the proverbial last minute, such a strategy is a simple recipe for disaster when doing ethnographic fieldwork. One cannot wait until the end of the process to “write up” an ethnography—a comprehensive

report or account of that work. Ethnographic fieldwork is *primary* research and is thus very different to what college students (and others) may be used to in doing *secondary* research.

Ethnographers in training are told to keep something with them at all times in which they can jot down observations and impressions. This can be a small (pocket-sized) notebook or even a folded piece of paper. I have made notes on any number of different scraps of paper on hand at the time that I realize that I need to begin the process of making sense of something that I have encountered. I continuously remind my students that they must work from such in-the-field jottings to create more detailed fieldnotes that “flesh out” what might be little more than bullet points. Some people nowadays use a small voice recorder to record impressions. I would still think it necessary to get that information out of the recorder (and also out of our heads) and into some textual form in order to make representations of experience in the field and effectively work with the data.

One of the most essential purposes for writing fieldnotes is, as Geertz would say, to turn the events of the moment into an *account* that can be consulted again (and again) later. Among other things, that account allows for the ethnographer to commit what he or she might not know is important in that moment to memory. We often will not know what is important until later, after other information and insight has been provided by further experience and exploration in the field. If one does not adequately document things now, they will likely not be available later apart from even more partial recollections than what is available to us in and through our necessarily limited fieldnotes. Immediately following from documentation is the opportunity to recognize *patterns*. Are there things that people say or do, for example, that suggest consistencies or relationships that are somehow ordered? Does something seem to be a “ritual,” for example? I tell my students that rituals are not far-out or exotic

things. They happen all around us—not only in churches but also football stadiums. They are apparent in town meetings and college classrooms. You can find them in the bathroom as well as the bedroom. They're everywhere. Here, I often suggest that my students take a look at Horace Miner's (1956) article "Body Ritual among the Nacirema" for mischievous insight into how we can make the familiar unfamiliar and therefore both noticeable and more readily subject to our analysis and interpretation.

As I have suggested, ethnographers can spend a good long time (months at least) working in the field so that they can, in much of this work, discover their purpose through lengthy participant-observation. This is why we so often hear ethnographic research referred to as "emergent" or as taking place "from the ground up." In most undergraduate courses in ethnographic methods, students should be given a set of training experiences that at least approach what would be typical of the professional ethnographer. In most cases, however, instructors cannot duplicate the full rigors of fieldwork for practical reasons—there is simply not enough time. Courses should be structured to allow for lots of exploration of the experience of participant-observation and the interactive and iterative process of revisiting what is collected in fieldnotes in order to continually refine one's understanding and approach. When a subject is raised—often as a question about a particular group or a cultural practice or belief—this begins to give focus and direction to the inquiry and writing. Both become increasingly purposeful. This is why it is so important that students undergo fieldnote reviews throughout the process of instruction.

Ethnographers depend on writing. In keeping with the open-mindedness that comes with the approach in the preliminary stages, ethnographers write about things that interest them generally about their fieldsite. They may even just begin writing about their own lives

as a way to raise questions about the world around them. Of course, this facilitates recognition of the relationship between biography and fieldwork—the entwining of narratives that I previously discussed. My first assignment in my undergraduate course in ethnographic methods is, in fact, for students to read the first two chapters from a book on writing memoir by Bill Roorbach. I take pride in the fact that I met Bill and came to know his work while I was a participant at the Bear River Writer’s Conference in Northern Michigan in the late 1990s. I was in attendance as an ethnographer in recognition of the fact that in order to do my work well, I needed to know how to write well. In particular, I wanted to write compelling stories. For me that meant not only “learning how to write” in particular ways but also developing a personally-engaged, creative relationship with the act of writing. In the chapters that I share with my students, Roorbach (1998) speaks to such things as the simultaneous centrality and faultiness of memory as source for identity and the necessity for having an acknowledged, ever-present “I” who constructs what must be—as I want them to recognize ethnography itself—a work of non-fiction that is necessarily “creative.”

My students then undertake, following Roorbach’s direction, a simple mapping assignment whereby they recall and explore their earliest memories of a place where they grew up and, using this graphical representation of their memory, craft a brief “map story” in which they seek their narrative “voice” and, hopefully, arrive at some recognition of the importance of their own history in coming to account for what it means to be human. At the very least, they have a glimpse into their own culturally- and socially-situated life history, particular and possibly unique as it may be, as an expression of a fundamental fact of our shared humanity—growing up. In this exercise, I intend to have students come to realize—through their writing of what usually appears to be the utterly mundane (what Malinowski

might have called the “imponderabilia of everyday life”) and, later, in the sharing of their stories—that significant realizations can be made about themselves in particular sociocultural and historical contexts as well as about the “human condition” generally—including, simply, our propensity for storytelling. To avoid simply ending up with detailed descriptions of maps or the real places that they are presumed to represent—that is to say “camera’s eye” depictions that lack interpretation of the possible significance of details—I remind students that the assignment asks for a “story” and thus, by my reckoning at least, something must *happen*.

I follow this “warm up” exercise in writing and thinking about our relationships to memory as well as writing as a representational act with a more overtly fieldwork-related but similar assignment on mapping a (city) block drawn from Paul Kutsche’s (1998) book on ethnographic fieldwork methods. My intent is to help students learn how fieldwork must be situated in a particular time and place, inspire them to overcome preconceived notions and perceptions about a given place and avoid judgmental shorthand in their descriptions, and, as always, to learn how to see what is familiar as if it were unfamiliar. As I suggested earlier, how we choose to see (or not see) the world is as important as how we choose to describe it. To help my students to think critically about their ability to observe ordinary things and everyday places in new ways—and to consequently open themselves up to genuine discovery through an enhanced visual and, ultimately, mental agility that facilitates productive, serendipitous encounter—I have them read the opening chapter—appropriately titled “Beginnings”—of John Stilgoe’s (1999) *Outside Lies Magic*.

Because fieldnotes chronicle our fieldwork encounters, they are where patterns are allowed to develop. Accordingly, ethnographers rely extensively on them to provide insight into what qualities may define members in a given group, for example. That is, ethnographers

depend on their fieldnotes to discover, to work toward preliminary understandings, to develop interpretations, and eventually to reach their conclusions. Ethnography, in large part, may be said to take place in and through fieldnotes. If it isn't in your notes—I like to say—you do not have it. From the beginning of their time in the field, ethnographers are constantly writing up observations and results, drawing at least tentative conclusions that they will continue to revisit in order to continually refine them.

As Geertz has said, ethnographic inquiry is the product of the field of cultural anthropology that is ultimately not an empirical science in search of immutable law, but rather an interpretative one in search of perennially emergent, intersubjective meaning. He further counseled that our understanding as ethnographers was always tentative and that as such we must aim, realistically, for what is productively a further refinement of debate rather than “the final word” on each of those myriads of subjects—collectively thought to contribute to an understanding of the human condition—to which we devote our attention as ethnographers. In this spirit, Renato Rosaldo (1993, 8) has said:

Although the doctrine of preparation, knowledge, and sensibility contains much to admire, one should work to undermine the false comfort that it can convey. At what point can people say that they have completed their learning or their life experience? The problem with taking this mode of preparing the ethnographer too much to heart is that it can lend a false air of security, an authoritative claim to certitude and finality that our analyses cannot have. All interpretations are provisional; they are made by positioned subjects who are prepared to know certain things [at any given point in time, including when they encounter someone or something in the field, as Rosaldo with the Ilongot,] and not others.

By remaining, as suggested by Rosaldo, “open” in multiple ways as the “human instruments” that we are, in a manner akin to Behar’s notion of vulnerability, ethnographers are best positioned for the revelatory and transformational impressions possible in the practice of our methodology and, ultimately, for building relationships through which we can connect meaningfully with others, exchange something important of ourselves, and have practical impact for a common good.

NOTES

1. In fact, Geertz should be credited with popularizing a notion originally described by philosopher Gilbert Ryle (1971) as an account of behavior that permits an understanding (in the reader) that goes beyond surface appearances to describe underlying patterns as well as broader cultural contexts that give that behavior its particular, culturally-informed meaning.
2. As I will explore in some greater detail later, the act of “writing culture” is no simple thing and certainly not one that should be taken for granted. Marking an especially important milestone in an emerging debate within the discipline of anthropology during the 1980s, the book *Writing Culture* (Clifford and Marcus 1986) tackled varied forms of ethnographic writing in terms of reflexivity and objectivity as well as what had been taken heretofore as the essential underpinnings of “ethnographic authority,” or what might be conveyed in the seemingly simple assertion of an ethnographer that “I was there,” in what had been and was becoming an increasingly and complexly interconnected world wherein, among other things, postcolonialism encouraged the examination of differentials of power between peoples in and from different places. This book helped mark what some refer to as a “turn” in anthropology described variously as “reflexive,” “literary,” “post-modern,” “deconstructive,” and “post-structural.” The years that I spent in graduate school during the 1990s were a time now considered the height of an ensuing “crisis of representation” provoked by such critical works as *Writing Culture*. Needless to say, the predicament made for stirring exchanges between older and younger faculty in the department who, at times, appeared to be speaking entirely different languages. For my part, contributions to the debate such as *Writing Culture* became helpful only insofar as they helped, over time, to bring attention to what might have been largely unexamined positions in anthropology—which is to say that the actions elicited by these positions might have become “mere” ritual enactments of enduring tradition with regard to basic questions of “who,” “what,” “where,” and “how.” By this I mean questions of who is doing the fieldwork (e.g., is the fieldworker “native” to the fieldwork context or not); what topics should be

studied; where should fieldwork be conducted (i.e., what constitutes an appropriate site); how should fieldwork be conducted (e.g., should it be thought of as a collaborative endeavor among equals); and, of course, how the results of fieldwork should be represented.

3. In fact, I ended up doing two dissertation fieldwork projects. The first was far more “traditional” in that I traveled halfway around the globe with a Fulbright fellowship to conduct fieldwork in a remote corner of Indonesia, relying on funds with origins in Cold War-era geopolitical concerns. The second, to which I refer here, was conducted in the state of Michigan through a grant from the Alfred P. Sloan Foundation, relying on funds with origins in the fortunes of the twentieth-century American automobile industry and General Motors. I defended the later project for my doctorate and produced a published ethnography on this work (Hoey 2014).
4. Contextualization and comparison are two sides of what Sanjek refers to as the “anthropological triangle” that serves as an operational system of knowledge construction of which ethnographic fieldwork is the final side and without which—all three aspects interacting—descriptive works of people and place cannot be said to be truly ethnographic. Sanjek traces ways in which, in the past century and a half, anthropologists have variously stressed or neglected different sides of the triangle. He notes, for example, how Franz Boas (in the United States) and Malinowski (in Europe) each declined to provide a larger context to their studies in order to create a fictive “ethnographic present” (i.e., a literary and temporal strategy employed to create a representation of a people prior to “contact” with Europeans) rather than an “ethnography of the present.”
5. See Gergen and Gergen (1983) for more on this concept.
6. At the same time, it should be noted, the purposely open-ended nature of many of these conversations allows for the possibility of what could in some instances amount to “unhealthy” introspection leading to a risk of psychological harm through what may be great “emotional distress” (see Corbin and Morse 2016). This possibility concerns not only IRBs but any properly trained researcher, as well. Fortunately, I cannot provide any personal commentary on the impact of such distress, as I have not—at least knowingly—witnessed it in my own fieldwork.

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