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Abstract

Embracing the trope of ethnography as narrative, this chapter uses the mythic story of Bronislaw Malinowski's early career and fieldwork as a vehicle through which to explore key aspects of ethnography's history and development into a distinct form of qualitative research. The reputed "founding father" of the ethnographic approach, Malinowski was a brilliant social scientist, dynamic writer, conceited colonialist, and, above all else, pathetically human. Through a series of intervallic steps—in and out of Malinowski's path from Poland to the "Cambridge School" and eventually to the western Pacific—I trace the legacy of ethnography to its current position as a critical, historically informed, and unfailingly evolving research endeavor. As a research methodology that has continually reflected on and revised its practices and modes of presentation, ethnography is boundless. Yet minus its political, ethical, and historical moorings, I argue, the complexities of twenty-first-century society render its future uncertain.

Key Words: anthropology, colonialism, epistemology, the field, intersubjectivity, Malinowski, methodology, writing culture

During my final weeks working on this chapter, I happened to watch the documentary *The Black Power Mixtape 1967–1975*—a contemporary collage of rarely seen Swedish television footage of the Black Power cultural revolution of the 1960s and 1970s. In the 1973 chapter of the DVD, there is a brief scene from inside a Swedish tour bus traveling around New York City. As the bus passes through Harlem, the tour guide—speaking in Swedish but translated as subtitles—describes the upper Manhattan neighborhood as “undoubtedly the Black man’s ghetto” where “large amounts of narcotics are circulating”; he goes on to remind the tourists of how their “welcome letter” had instructed them that the tour company did “not want anyone to visit Harlem for *personal studies* . . . because [Harlem] is only for Black people” (Olsson, 2011—emphasis added).

This human desire for personal studies, the traveler’s yearning to get off the tour bus, the curiosity

to move beyond the pretense of staged representations of life and to discover what it is really all about, underlies the post-Enlightenment project of apprehending the world through physical force, cognitive classifications and containments, and, at times, empathetic pretensions. The same impulses anticipated among Swedes in 1970s New York inspired a generation of European explorers to penetrate the dark continent of Africa (Thornton, 1983) and continue to compel turn of the (twenty-first) century visitors to Chicago to sift and sort through a sliding scale of authentic venues in search of “the real” Chicago blues experience. But, as David Grazian (2003) has effectively shown, even the most seemingly authentic of these late modern cultural products are fabricated commodifications, banking on the city’s global popularity as a blues destination.

Such realizations have implications for how we think about the history, current state, and future of

ethnography. More than merely embracing Erving Goffman's (1959) mid-twentieth-century declaration that "all life is a stage"—though its connotations are perhaps more profound than some recognize—the staging of the ethnographic project is acutely linked to an invasive mix of privilege and inquisition that sprouted in the garden of Western modernity and spread throughout the colonial hinterland. To make sense of this deep history one must begin with questions like: what does it mean to study the life of someone else? What gives anyone the right to initiate research on another community (even when they sincerely and passionately believe it is for the community's betterment)? And, pressing beyond the expected, pedestrian answers, what larger goals are we working towards or working in the service of when we undertake qualitative social fieldwork?

I can imagine our Swedish tourist being just as curious about the dealings of Wall Street investment bankers (Ho, 2009) but less inclined to consider *going there*, not necessarily out of a conscious awareness of Wall Street's inaccessibility, but due to a doxic (Bourdieu, 1977) inability to even acknowledge it as a possibility. Then again, social researchers and cultural commentators from W. E. B. DuBois (1903/1996) to Norman Mailer (1957) to Jon Cruz (1999) have observed the racially loaded fascinations that people of European descent have about those they (a) have had unproblematized access to and (b) view as most distinct from themselves, either physically, culturally, or both. Explanations for this range from the allure of the exotic and presumed primal drives towards straightaway satisfaction and survival that govern those at the other end of the civilization spectrum (here Mailer and perhaps Malinowski) to empathy with the romanticized innocence that such closeness to nature and freedom from civilization's repressive shackles offers (here Margaret Mead and perhaps Malinowski). Anthropology—the discipline to which ethnography is most historically bound—came of age as a legitimate academic field through these Western impulses while simultaneously fueling their popular interest (Thornton, 1983).

Like the threat of Swedish tourists undertaking personal studies, ethnography as a research practice is, in many respects, renegade. That is, it refuses to follow strict conventions and achieves virtue and vitality through its lack of prescription. Ethnography straddles structured research design and improvised inquisitive adventure, constantly moving betwixt and between theory, data, and

analysis (O'Dell & Willim, 2011). Although it is non-linear, it is profoundly narrative.

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This chapter introduces ethnography, as a specific type of qualitative research methodology, through an historically conscious narrative of its principal and principled approaches. Much has changed in ethnography since the classic era when researchers such as Alfred R. Radcliffe-Brown (1922) and E. E. Evans-Pritchard (1940/1969) traveled to far-away places with names like the Andaman Islands and Nuerland. Their charge was to plot the topography of human cultural difference and to identify, via conditions of isolation and theories of unified wholes, the systems and processes through which social life successfully functioned. Today, most observers regard ethnography as fitting within a more sophisticated project of making sense of social life through the ways of knowing that are most meaningful and potentially most consequential to social actors themselves. Yet I caution against the tendency for each coming-of-age generation to selectively disconnect itself from those that came before.¹ Ethnographers trained in fields such as anthropology, sociology, cultural studies, and folklore recognize the importance—or have experienced the rite-of-passage mandate—of knowing their history. Still, mere knowledge of past right- and wrongdoings combined with a critical disposition neither empowers contemporary ethnographers to make the most of their approach's unique virtues nor alleviates them from its most primordial problematics. Moreover, as ethnography has propagated into such fields as organizational studies, planning, management, and industrial engineering (to name just three) concerns over research efficiency and tangible outcomes tend to eclipse the historically informed and critical perspectives that have defined its fundamental modes of understanding. What is called for, then, is an accounting of ethnography that situates it contemporarily while simultaneously integrating historical actors and the social forces they at times conformed to and at others contended with.

One of the more damaging consequences of ethnography's spreading popularity has been the propensity to view it as a method rather than a methodology.² This difference is significant. A method is simply a technique or tool used to collect data. Ethnographers often utilize a variety of tools and techniques during the course of their research, including but not limited to: establishing rapport; selecting informants; using a range of interview and

focus group forms; making observations—both participatory and non-participatory—and writing field notes based on them; conducting surveys, genealogies, and domain analyses; mapping fields; transcribing texts; and coding data.³ In contrast, a methodology is a theoretical, ethical, political, and at times moral orientation to research, which guides the decisions one makes, including choices about research methods. This distinction between method and methodology is crucial to my effort to differentiate ethnography from qualitative field research more generally. Much of what is included in this chapter will be useful to qualitative researchers on the whole. However, my primary purpose is to describe and delineate ethnography as a communally engaged and historically informed early twenty-first-century research practice.

Much like *culture*, ethnography is one of those social scientific abstractions that is readily deployed to mark out what we—as anthropologists, sociologists, and an increasing range of researchers in other fields—do as unique, yet is difficult to capture in a single precise and thoroughgoing definition.⁴ Part of the difficulty is that the term refers to both a research process and the written product of those research activities. While not losing sight of the important revisions to come out of its “crisis of representation” that have pushed scholars to acknowledge, and in fact prioritize its ultimate textual character (Clifford & Marcus, 1986; Marcus & Fischer, 1986), in this chapter, I mostly treat ethnography as a processual approach to *doing* a particular kind of qualitative research.

To begin, I present a few basic definitions of ethnography. Carol A. Bailey (2007) quite simply explains it as “a type of field research that requires longterm engagement in a natural setting” (p. 206). In a more detailed description, Martyn Hammersley and Paul Atkinson outline the ethnographic project as:

participating, overtly or covertly in people’s daily lives for an extended period of time, watching what happens, listening to what is said, asking question . . . [and] collecting whatever [other] data are available to throw light on the issues that are the focus of the research. [1995, p. 1]

Lastly, Clifford Geertz (1973), in his classic treatment, defines ethnography as “an elaborate venture in . . . ‘thick description’” (p. 6). Etymologically, ethnography combines *ethno*, meaning “culture (or race),” and *graphy*, meaning “to write, record, and describe.”⁵ Thus ethnography, which Barbara

Tedlock (2000) refers to as an “inscription practice” (p. 455), can be thought of as the process and product of writing, recording, and describing culture.

Building off of these different understandings, my treatment of ethnography is simultaneously broad and narrow. During the late twentieth century and now into the twenty-first, ethnography moved from the confined ranges of anthropology and sociology to a tremendous number of disciplines and fields, including (in addition to those listed earlier) psychology, geography, women’s studies, history, criminology, education, political science, communications, leisure studies, counseling, nursing, psychiatry, medicine, social work, and law (see Tedlock, 2000; Denzin & Lincoln, 2008; Jones & Watt, 2010), just to name a few. Attempts to put narrow disciplinary restraints on ethnography are, in my view, shortsighted and possibly even disciplinarily egocentric. Similarly, the variety of practices involved with ethnography is expansive and continually expanding. These include several traditional qualitative research methods (such as those listed earlier) as well as more recent innovations that cross into visual and sensory studies (Pink, 2006, 2009), the arts (Leavy, 2009; Schneider & Wright, 2010), action-oriented research (Kemmis & McTaggart, 2000), autoethnography (Ellis, 2004; S. H. Jones, 2008), and collaborative ethnography (Lassiter, 2005). This is not the place to explicate the multifaceted dimensions of these varied approaches, but I want to be clear in stating that all cohere (or have the potential to) with the understanding of ethnography that I put forward.

At the same time, there has been a tendency among some scholars to define almost any qualitative research project—and particularly projects involving traveling to a field site—as ethnographic. On this matter I am more stringent in explaining that ethnography involves more than just going somewhere to conduct research *on* or *within* a community. It involves a certain frame of mind, or, I will even say, historically aware sensibility that is very much its own. Ethnography is often equated with the practice of (or practices surrounding) participant observation. I agree to the extent that ethnography fits within a participant-observation framework, yet to highlight what I see as a key difference, let me return to the definition from Geertz, which is premised on his notion of *thick description*. In his classic illustration of thick description, Geertz (1973) discusses Gilbert Ryle’s (1971) distinction between the involuntary contracting of the eyelid associated with a twitch and winking. While as a physical description of action the two are the same,

properly contextualized—in the case of the wink, involving such things as impetus, intention, and success in communication—they are drastically different. Ethnography, as I am defining it (as a methodology), involves degrees of impetus, intention, and conviction that are different from simply having a participant-observatory perspective and standpoint. Although many of its characteristics have changed since the days when Margaret Mead first traveled to Samoa, like the origins of ethnography itself, these changes have been as much a gradual, reflective, and historically mediated evolution as a radical shift. Thus, a solid grounding in the history of ethnography is important to understanding how current ethnographic research differs from what we might broadly call qualitative field research.

My approach involves reviving, interrogating, and embarking on a narrative journey via ethnography's most pervasive origin story. That is the chronicle of Bronislaw Malinowski's pioneering field research in the Trobriand Islands, which, within the core fields listed earlier, is commonly held up as the ethnographic archetype (Strathern, 1987). In doing this, I attend to the multiple trajectories of development and enlightenment that follow from these mythic origins. This is complex terrain since, as most researchers now recognize, ethnography was birthed out of colonialist impetuses that included "territorial expansion, the pursuit of military power, commercial greed... the need to find raw materials and investment opportunities for accumulated capital, [as well as] an emerging 'media industry' in search of stories to sell" (Fabian, 2000, p. 4; see also Thornton, 1983). Retrospectively, the history of ethnography is comprised of hardly heroic heroes (see Sontag, 1966/1978). While I do not shy away from the intellectual temptation of unpacking the possible fictions surrounding Malinowski as a mythic figure, I ultimately treat representations as real—meaning, they are products of contested political processes that have real consequences (Hall, 1996). Thus these historical trajectories are shaped as much by what is represented and remembered, which is never fixed, as by what actually might have been.

Building on the trope of ethnography-as-narrative-journey, this chapter uses the narratives of Malinowski's early life and career as vehicles through which to present important aspects of and issues facing contemporary ethnography. This involves a series of intervallic, temporal steps out of the early twentieth century into broader historical and present-day contexts. I begin by discussing

Malinowski's mythic status in relation to some of his ideas regarding the social functioning of myths. I next review his early life experiences and education in Poland and Germany as a means to introduce key paradigmatic and epistemological underpinnings of the ethnographic enterprise. Malinowski's travels to England and association with the Cambridge School provide an opportunity to present the transition in social research practices during the late nineteenth and early twentieth centuries, which the myth of his methodological revolution belies. His initial research experiences on the island of Mailu illustrate the colonial legacy of the ethnographic project as well as the interpersonal dynamics of its research practices, and his transition from Mailu to the Trobriand Islands offers an opportunity to contemplate the changing notion of the ethnographic "field." The 1922 publication of *Argonauts of the Western Pacific* marks a watershed moment in the history of ethnography and Malinowski's career. It was here where he first presented his "modern sociological method of fieldwork" (Stocking, 1983b, p. 111). My reflections on the impact of this book segue into some important considerations surrounding what has been referred to as (among other things) ethnography's "literary turn." Finally, a consideration of Malinowski's reputation gives rise to some conclusionary remarks regarding ethnography's historical legacy and future. Journeying through the life of the man whose idealized image, more than anyone else, came to epitomize ethnography and whose divulged human frailties contributed to its reorientation highlights a degree of sophistication that is frequently omitted in deference to (too often self-congratulatory) how-far-we-have-come framings of history.

Malinowski's Myth

The history of ethnography is replete with its own myths, superstitions, and survivals. As the countless ethnographers who have studied these topics over the last century-plus have taught us, such aspects of culture should not be dismissed lightly but rather interrogated for the important purposes, both functional and symbolic, they serve. In ethnography's most prominent origin story, Polish-born, British-educated⁶ Bronislaw Malinowski is cast as its progenitor. Though the "Malinowski myth" has been discussed in several anthropology-specific treatments of methods, theory, and the history of the field,⁷ as ethnographic research has diffused into other areas, Malinowski the man, the myth, and the heuristic value of both have become dispensable.

This chapter—as much for a non-anthropological readership as for a distinctly anthropological one—aims to correct this.

Viewed through the lens of some of his own theoretical findings, Malinowski's early life and career, that is, his circuitous journey to "inventing" the ethnographic method, becomes an instructive hagiography—part travelogue, part founding fable. In developing his own version of (psychological) functionalism, Malinowski did groundbreaking work on the topics of myth, magic, and superstition. Contrasting early views that interpreted myths as "idle rhapsody" or "aimless outpourings of vain imaginings" (Malinowski, 1926/1948, p. 97), he forcefully put forward the position that myths actively affect the conduct of members of a community by exercising "a living reality, believed to have once happened in primeval times, and continuing ever since to influence the world and human destinies" (p. 100). Through myths individual reputations are made and sustained and important lessons and understandings of cultural practices are carried over time.

According to Malinowski's myth, the young Pole first became fascinated with cross-cultural study when, during a period of illness, his mother read him sections of Sir James Frazier's *The Golden Bough* (1900). After receiving his doctorate in physics and mathematics in Poland, Malinowski, as the story goes, traveled to England in pursuit of education and romance. Once there, he converted to the budding science of anthropology and in 1914 set off to do field research in the southwest Pacific where, as a consequence of the outbreak of war in Europe,⁸ he found himself stranded for several years. During this time—after realizing the importance of the anthropologist getting "off the verandah" (Singer & Dakowski, 1986b) and, instead, living among the natives—he established what he claimed was "an entirely new academic discipline" (Leach, 1957/2000b, p. 49), now known as ethnography.

Foundations of a Man and His Methodology

Like an onion, the layers of Malinowski's myth can be peeled back to reveal numerous inconsistencies, resulting from selective embellishments, missing details, lacks of contextualization, and perhaps just plain concoctions. Adopting a weighty ethnographic tag popularized by James Clifford (1986), the various versions of Malinowski's story are at best *partial truths*. Although divining the correct version of this story is not my goal, interrogating some of

its factual bases opens a didactic narrative pathway along which to contextualize the famed "father of fieldwork" (Thornton, 1985, p. 8).

Both Malinowski's class background and the role of his mother in introducing him to the work of Frazier have been scrutinized.⁹ The question of class is notable if for no other reason because early ethnography—with its demands of traveling to faraway places and associated reprieve from everyday economic necessities—was thought to be an elite profession (Nash & Wintrob, 1972; Tedlock, 2000). By the early years of his post-secondary education, Malinowski was undoubtedly familiar with *The Golden Bough*. The book's focus on the worship of Diana at Nemi in southern Italy in all likelihood resonated with Malinowski, who as a sickly youngster, upon the orders of his doctors, had traveled throughout the Mediterranean with his mother (Wayne, 1985);¹⁰ and reading Frazier's cross-cultural comparisons with "exotic" customs from around the world most certainly nourished the exceedingly ambitious Malinowski's desire to conduct his own *personal studies*.

Malinowski's journey to England was preceded by two years at Leipzig University in Germany where he was directed toward *Völkerpsychologie* through the work of the university rector and future "father of experimental psychology," (Kess, 1981, p. 126) Wilhelm Wundt. As with his earlier path to Jagiellonian University in Poland—where his father was "a renowned professor of Slavic philology... [with] a lively interest in Polish ethnography and folklore" (Pulman, 2004/5, p. 126)—Malinowski's decision to study at Leipzig was quite literally following paternal footsteps. While at Leipzig in the 1860s, Lucjan Malinowski had "broke[n] new ground in methodology" with his doctoral dissertation in Silesian dialectics (M. W. Young, 2004, p. 12). Yet the younger Malinowski, who by all reports was never close to his father (Kubica, 1988, p. 89; Wayne, 1985, p. 529), apparently also chose Leipzig because of its reputable program in thermodynamics (M. W. Young, p. 128).

The decision to travel to England was indeed motivated by romantic interests. Shortly after arriving in Leipzig, Malinowski met the widowed South African pianist Annie Brunton—described by his daughter as a woman "considerably older than him" (Wayne, 1985, p. 531)—and the two began a stormy affair. In December 1909, when Brunton moved to London, Malinowski soon followed. He once said that "if [he] hadn't met Mrs. Brunton [he] would never have taken up sociology"

(Wayne, p. 532). Though likely an example of his characteristic hyperbole and flare for the dramatic, Brunton undoubtedly influenced the much younger “Bronius’s” intellectual growth in at least two ways. First, by pulling him from Leipzig—an institution that “represented the best of German science” (M. W. Young, 2004, p. 130) where he had the opportunity to work with a venerable master in the field¹¹—to Britain, which by 1910 was a hotbed for ethnology and home to prominent figures like Edward Burnett Tylor, William H. R. Rivers, Charles Seligman, and Malinowski’s old friend Frazier. The second influence came through Brunton’s role in (re-)exposing Malinowski to music, and, by extension, to the arts in general.

One oft-cited tension in Malinowski’s psyche was the opposition between the scientist and the artist, reason and intuition, rationality and emotion (Thornton, 1985; M. W. Young, 2004). The productive off-play of these two temperaments would serve him well—in terms of both methodological process and written product—as an ethnographer.¹² Upon arriving in Leipzig, with the intention to study the thermodynamics of liquids and gasses at “the renowned centre in Europe” for such study (M. W. Young, p. 128), one could surmise that Malinowski’s pendulum had swung sharply towards science. Annie Brunton’s greatest influence on the aspiring young scholar may have been to bring him back into balance—as turbulent as a Malinowskian balance would have been—and to open his eyes to the possibilities beyond the “best of science” that had so intrigued him years before.¹³

Ethnographic Science, Ethnographic Humanity

Ethnography can take many forms and guises. Despite some commonalities in practices and politics, ethnographers adhere to multiple epistemologies and paradigmatic understandings of what constitutes good research. This creates a troublesome tension: whereas different researchers and research activities may appear the same, and may be guided by similar politics and sensibilities, they nevertheless may be foundationally grounded in different philosophies of knowledge. Malinowski, fittingly perhaps, straddled ethnography’s prime epistemic divide. Anthropology has been referred to as the social science that is closest to the humanities (Redfield, 1953; Aunger, 1995). Ethnography, as its chief mode of research, is firmly situated at these crossroads. Yet this position is never fixed.

As ethnographic practices have spread into other disciplines, the potential outcomes and misunderstandings resulting from epistemological differences, although not always discussed, have become more pronounced. When people undertake ethnographic research in the fields of, for instance, architecture, marketing, and/or women’s studies, what are their goals and what are considered legitimate means of attaining these goals? Thomas Schwandt (2000) highlights three areas of concern surrounding qualitative inquiry, which are instructive for a discussion of ethnography in particular. I adapt them here:

1. Cognitive concerns surrounding how to define, justify, and legitimize claims to understanding, which might or definitively might not include questions of validity, transferability, and generalizability.

2. Social concerns regarding (in this case) the goals of ethnography: should they be emancipatory and transformative? Should ethnographers seek solutions/answers to problems/questions that are of direct interest to their own academic communities and/or to the communities they study? Or should they seek to understand the situations in which, and the social processes through which, human actions take place in the ultimate interest of working towards a better understanding of sociality in general as well as in the particular? Questions such as these are neither all encompassing nor mutually exclusive but they do point towards potentially stark divergences in the ethnographic enterprise.

3. Moral concerns as to how to “envision and occupy the ethical space” between ethnographers and those they research in responsible, obligatorily aware, and status conscious ways. (see Schwandt, 2000, p. 200)

The first of the three areas—specifically ethnographers’ epistemological embeddedness and paradigmatic adhesions—is of most immediate concern here. Nonetheless, for the ethnographer, cognitive concerns are not neatly separated from social and moral ones. Although I save discussions of social responsibility and ethics until later in the chapter, an awareness of both their impact on, and how they are impacted by, foundations of knowledge and understandings of legitimate research are important.

Before briefly outlining the guiding paradigms surrounding ethnography, I offer a few additional caveats. Whereas defining and labeling these various epistemological and methodological frameworks

is useful, it would be a mistake to give too much attention to trying to fit a particular researcher or even an instance of ethnographic research neatly into one category. Ethnographic experience is perpetually ephemeral, meaning that at times ethnographers are prone to move, transform, and shape shift between different paradigmatic classifications. Attempts to categorize also tend to highlight differences over time and disciplinary space. While differences do exist, the need to place individuals or projects in particular boxes closes down the possibility of also seeing commonalities and furthermore belies the nuanced nature of ethnographic inquiry. Nonetheless, in what follows, I label some of the traditions that ethnographers might move between and draw on variably as paradigmatic resources.

I begin, quite straightforwardly, by separating inclinations towards science and inclinations towards the arts and humanities. This can, by and large, be cast as a binary between positivism and what I will broadly call interpretivism. Although few if any contemporary ethnographers would define themselves as strict positivists, it is nonetheless necessary to discuss positivism as foundational to any social scientific enterprise. To some extent, outlining the tenets of strict positivism may be useful in explaining what most ethnographers are not. However, before dismissing it too quickly, I should point out that, particularly with regard to the mandates of certain gatekeepers of credible research reporting, ethnography is not as far removed from its positivist principles as some of its practitioners would like to think. Furthermore, there is an important post-positivist paradigm that continues to carry weight.

POSITIVISM

Positivism is premised on a belief in what is referred to as *naïve realism*—that is, the notion that there is a reality “out there” that can be grasped through sensory perception. As such, it holds empirical data—that which is produced through direct observations—as definitive evidence through which to construct claims to truth. In doing so, positivism prioritizes objectivity, assuming that it is possible for a researcher to detach his or herself from values, interests, or the clouding contamination of bias and prejudice. Following this formula, good research is achieved through conventional rigor—that is, dutifully following a prescribed, systematic, series of steps surrounding data accumulation and analysis. With this being the most scientific frame of reference that ethnography potentially occupies, standards of hypothesis testing and deductive reasoning

are principal to its practices. In that positivism recognizes a fundamental (capital “T”) Truth, which it is believed researchers can apprehend, ethnographers anchored in this tradition are more prone to concern themselves with questions of transferability (i.e., can the findings from one setting be applied to another?) and generalizability (i.e., can the findings from a particular context be generalized on to the whole?) on the assumption that such Truth has potential relevance for a broad range of social circumstances and cultural contexts. Today all ethnographic researchers recognize the role of culture and socialization in shaping social realities; thus, strict positivism has fallen out of favor. However, post-positivist orientations towards valuing empirical evidence, making efforts toward detached objectivism, and deductive reason continue, even if researchers are less confident about the conclusions.

INTERPRETIVISM

If the positivist epistemological branch, with its post-positivist paradigmatic inclinations, supports Malinowski the scientist, Malinowski the artist is perched on the interpretivist (or constructionist) alternative. This position, which issues from an acknowledgement of the constructed nature of all social reality, recognizes no single all-encompassing Truth, but rather multiple (small “t”) truths that are the products of human subjectivities. As such, cultural and contextual specifics are critical to understanding, and inductive reasoning becomes the privileged path to making sense of unwieldy social realities. Reality, which is shaped by experience, thus becomes something to be interpreted. Such interpretivism sees human action as inherently meaningful with meanings being processual, temporal, and historically unfinished.¹⁴

The subjectivity of the ethnographer is quite consequential here. Under any form of interpretivism, the outcomes of researcher bias are acknowledged. Sometimes efforts are made to mitigate researchers’ subjectivities. Such techniques might involve reflexive journaling, inventorying subjectivities, and other attempts to manage and track bias (Schwandt, 2000, p. 207 n. 11). Yet increasingly interpretivist approaches accept that within ethnography the human is the research instrument and as such, cultural, social, and personal frames of reference are inescapable.

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To repeat myself, I do not think particular researchers or specific research projects should

necessarily being categorized along the broad epistemological strokes that I am painting. Although I acknowledge that many are, I think it is important to appreciate how both positivist and interpretivist foundations impact all ethnography. Indeed, I would question if a researcher with inclinations and sensibilities fully saturated in post-positivism would even fit into my rather scrupulous definition of ethnography—a confirmatory approach to assessing one's hypothesis via the accumulation of empirical data through long-term fieldwork living as a member of a community strikes me more as a non-ethnographic form of participant observation. Nonetheless, it would be limiting to not recognize how the significance of positivist and post-positivist tenets impact ethnography.

Since Malinowski's early-twentieth-century articulation of ethnography as a proper research method, there have been two general movements, which have overshadowed an assortment of counter-currents and inter/intra-disciplinary variations. The earlier of the two, which dominated anthropology up until the Second World War, was the movement towards legitimizing ethnography as a rigorous scientific method on par with those practiced in the supposed "harder" natural sciences. The latter part of the twentieth century witnessed the rise of a more humanistic acceptance of ethnographic research. Dennison Nash and Ronald Wintrob (1972) have suggested this may have more to do with what is institutionally accepted as legitimate research and how that shapes what aspects of the research process the researcher is willing to disclose than with what researchers themselves believe. As evidenced in his early ethnographic writings and actualized through the posthumous publishing of his field diaries (see "Malinowski's literary (re)turn"), Malinowski, although very much a researcher of this earlier era, personified this crucial ethnographic binary.

In concluding what has been outlined, I think it is useful to highlight two pervading (non-exclusive) sets of questions that are at the core of these paradigmatic tensions: one surrounds the basis of truth, and the second is concerned with the positioning of the researcher in respect to the research endeavor.

1. Is truth something that exists independently to be discovered by researchers? Are truths the products of subjectively authored realities to be grasped by researchers? Or are these subjective "truthful realities" to be engaged with the researcher as part of the truth-making process?

2. Ethnography is defined in part by its participant-observation mandate of researcher involvement. Yet should this constitute taking up an inside/involved standpoint from which to make detached observations? Should it be based on a deeply engaged experiential understanding? Or should researchers understand themselves as active participants in shaping the social world they conduct research in?

The answers to such questions may look very different depending on the disciplinary, institutional, and personal groundings of the researcher; the standards of the outlets where they are seeking to publish, publicize, or apply their work; and/or the specific uses to which the findings of a particular project will be put. For example, commercial ethnographers working under the dual pressures of time and a need to communicate applicable findings, both customary in the business world (Ehn & Löfgren, 2009), will feel compelled to adopt a more scientifically precise mode of inquiry and reporting that steers clear of the theoretical complexities and deliberations commonly found within academia.

Malinowski Encounters the Cambridge School

In addition to his pursuit of Annie Brunton, Malinowski had a second romantic interest in England. Since a childhood visit with his mother, young Bronius had cultivated an intense attachment to anything having to do with Britain. While crossing the English Channel by ferry, he wrote an essay-letter to a Polish friend in which he confessed to having "a highly developed Anglomania" and "an almost mystic cult of British culture" (Wayne, 1985, p. 532).

It appears that his interests in anthropology were firmly set while making this journey, for once in England, he wasted little time traveling to Cambridge and introducing himself to Rivers and Alfred Cort Haddon—two men who had brought ethnological acclaim to the school by way of their 1898 Expedition to the Torres Straits (see Kuper, 1996; Stocking, 1983b; Urry, 1972). Either through these men or his own initiative, Malinowski soon got to know the other members of England's leading circle of ethnologists¹⁵ who collectively came to be called the "Cambridge School."¹⁶ He arrived in March 1910 and by that summer, presumably on Haddon's advice (M. W. Young, 2004, p. 68), Malinowski was registered for classes at the London School of

Economics. There he would study under Charles Seligman, who became both mentor and something of a supportive older brother to him (M. W. Young, p. 160).

The first two decades of the twentieth century have been described as a period of re-orientation away from “the Tylorian domination of anthropology,” with its focus on culture and custom,¹⁷ and towards a serious investment in ways of going about collecting and using data (Urry, 1972, p. 48). This was a time when, on both sides of the Atlantic, the field of social/cultural anthropology formally crystallized around specific sets of prescribed methods and the conferring of degrees. Malinowski entered the world of British anthropology soon after embarking on his Pacific islands research, at precisely the moment when the decades-long clamorings for a definitive method were reaching a cusp. In a 1909 meeting of the principals from Oxford, Cambridge, and the London School of Economics, it had been decided that “ethnography” would be the term used for “descriptive accounts of non-literate peoples”—as distinct from the historical and comparative-based ethnology (Radcliffe-Brown, 1952, p. 276; see also note 15).

The cutting-edge movements of the day were toward “intensive work,” which had been outlined thoroughly (against the older standard of survey work) by Rivers in 1913:

A typical piece of intensive work is one in which the worker lives for a year or more among a community of perhaps four or five hundred people and studies every detail of their life and culture; in which he comes to know every member of the community personally; in which he is not content with generalized information, but studies every feature of life and custom in concrete detail and by means of the vernacular language. It is only by such work that one can realize the immense extent of the knowledge which is now awaiting the inquirer, even in places where the culture has already suffered much change. It is only by such work that it is possible to discover the incomplete and even misleading character of much of the vast mass of survey work which forms the existing material of anthropology. [quoted in Kuper, 1996, p. 7]

This passage is significant in demonstrating the extent to which Malinowski’s “research revolution” was already in the thoughts and minds—if not practices—of many of the Cambridge School scholars who mentored him (see Urry, 1972; Langham,

1981). Since returning from the Torres Straits expedition in 1899, Haddon had “busily propagandized” the need for “fresh investigations in the field” conducted by trained anthropologists (Stocking, 1983b, p. 80; see also Haddon, 1903).

Writing in 1912, Robert Marett had stressed that a “conscious method” was needed in anthropology and sociology. Described by Adam Kuper (1996) as “one of the last of the armchair anthropologists” (p. 7), even Marett recognized the merits of *intensive work* and intimate research. Indeed, Marett could have been dictating to his future “secretary Malinowski” (see the following section), just weeks before the latter embarked on his own field research, when he wrote:

[It is] most important at the present juncture that some anthropologist should undertake the supplementary work of showing how, even where the regime of custom is most absolute, the individual constantly adapts himself to its injunctions, or rather adapts these to his own purpose, with more or less conscious and intelligent discrimination. The immobility of custom, I believe, is largely the effect of distance. *Look more closely* and you will see perpetual modification in process. [quoted in Wallis, 1957, p. 790—emphasis added]

As with many myths, Malinowski’s serves the euhemeristic function of deification (see Stocking, 1983b), whereas a thorough examination of the intellectual environment in which he came of age strongly suggests that his pioneering work was more straightforwardly a product of the social forces and prevailing ideas on how to best research, document, and understand (and in many instances ultimately manage) human difference. This minimization of his agency and foresight gets magnified through the facts of how he came to New Guinea and eventually the Trobriand Islands, yet in surprisingly different ways from how the well-rehearsed myth of ethnography’s origins represents it. What is perhaps most telling is the extent to which, although he may have strived to, Malinowski was never successful in separating himself from the colonial impulses that characterized his upbringing and training.

Malinowski’s Journey to the Western Pacific

Even at its most scientific, ethnography is resolutely a human science conducted in a real-world laboratory. As such, the ethnographic enterprise is saturated with circumstances, situations, and personalities that are less anticipated and controllable

than its research reporting typically presents. Tedlock elaborates:

No matter how much care the ethnographer devotes to the project, its success depends upon more than individual effort. It is tied to outside forces, including local, national, and sometimes even international relationships that make research possible as well as to a readership that accepts the endeavor as meaningful. [2000, p. 466]

Often the messiness involved when one (or more) human beings commits to long-term research living among a community of human beings, who ideally and inevitably are continuing along the unforeseeable journeys that are their lives, is either managed through a series of entertaining, at times instructive, but usually incidental anecdotes or kept completely out of the research report. Again, this probably has more to do with accepted conventions of academic legitimacy than it does with particular ethnographers' lack of sophistication in recognizing the variability of their research subjects' lives. Nevertheless, conceived of in this way, the ethnographic project with its unwieldiness and unanticipated turns, has some notable parallels to the tradition of nineteenth-century travelogue reporting that the Cambridge School had been so interested in moving away from. One of the first great episodes along this adventure involves the miscellaneous twists and turns that lead ethnographers to their chosen field sites.¹⁸

In many respects, Malinowski would play the role of "bemused bystander" (M. W. Young, 2004, p. 245) in the sequence of events that led to the start of his 1914 western Pacific fieldwork. He had expressed to Seligman that he was willing to spend up to two years in the field, and, perhaps more diplomatically than intellectually, seemed content to let his various academic patrons—among them Haddon, Rivers, Seligman, and Marett—wrangle over his ultimate destination. It appears that Seligman, with the backing of Haddon, did the legwork of securing two years' worth of funding. The combination of Haddon's influence and Seligman's initiative held sway, and Malinowski's fieldwork was designed as a follow-up study of Seligman's earlier expedition to British New Guinea. Marett is widely credited with securing Malinowski's passage to the Pacific by enlisting him as secretary to the anthropology section of the British Association for the Advancement of Science meeting, which took place in Melbourne that year (Kuper, 1996,

pp. 11–12)—a position that brought with it travel funding.¹⁹

Stocking (1992, p. 242) has outlined the precarious position that Malinowski found himself in following the outbreak of war in Europe. Whereas the Malinowski myth focuses on his "enemy alien" status as a citizen of Austria-Hungary, the most consequential outcome of the Great War's outbreak for Malinowski appears to have been a lack of access to personal funds back in Poland, which placed him at the mercy of local officials and made him dependent on the good will of members of the Australian scientific community.²⁰ The myth of being stranded appears to be a fabrication, for Kuper (1996) contends that "all enemy scientists... were allowed to return to Europe" (p. 12).

Getting Off the Veranda

Malinowski's celebrated epiphany that

the anthropologist must relinquish his [*sic*] comfortable position in the long chair on the veranda of the missionary compound, Government station, or planter's bungalow, where armed with pencil and notebook and at times with a whisky and soda, he has been accustomed to collect statements from informants, write down stories, and fill out sheets of paper with savage texts [Malinowski, 1926/1948, pp. 146–147]

appears to have been inspired by both scholarly ambition²¹ and an interest in preserving indigenous customs that he quite literally saw as threatened by the civilizing mission. The early sections of Malinowski's published *Diary* (1967/1989) illustrate his growing distaste for the missionaries he lived among during his initial field stay on Mailu:

These people destroy the natives' joy in life; they destroy the psychological *raison d'être*. And what they give in return is completely beyond the savages.²² They struggle consistently and ruthlessly against everything old and create new needs, both material and moral. No question but that they do harm. [p. 41]

Malinowski's ire was chiefly directed towards Reverend William J. Saville, with whom he lived as a paying guest.²³ Saville, who with his wife had served on Mailu since 1900, at one point wrote Haddon with his own impressions of Malinowski:

You ask me about Malinowsky (I forget how you write his name)... I must candidly confess that I hope we shall never have to entertain that gentleman again... I admire his enthusiasm for his work, but he spoiled that altogether by not being intelligibly able

to understand that other people also might have a right to interests in which they are much justified and just as likely to be quite enthusiastic as he was about his... Dr. Malinowsky seemed unfortunately to think that our time and that of our people should be given up to him. He very likely did not mean this, but his experience with men seemed to be of the smallest and he was pretty much like a child with a new toy. The problems he was trying to work out were of the keenest interest to me, but the minds of some of us must have relaxations from one subject, by the tackling of others. *Had he been a man, who would enter into the position and minds of another*, whether native or white, he could have got twice as much information in one twelfth of the time. A native is not a class room student, and a native likes a bit of fun and a game, Dr. M. seems to understand neither, nor could he understand anybody who did.

[M. W. Young, 2004, pp. 357-358—emphasis added]²⁴

The described intensity and implied ambition are certainly in line with what we know about Malinowski's personality. Although Saville's account likely contains some embellishments, this early documentation of an observer observed (Stocking, 1983a) is enormously illuminating and offers important lessons for any young, zealous ethnographer. As a beginning researcher, "Malinowsky" made several flawed assumptions. Even prior to the decision to "camp...right in their villages" (Malinowski, 1922/1966, p. 6), his ethnographic fervor would have motivated him to "push research beyond its previous limits in depth, in width, or in both" (p. xvii).

Throughout his early research, Malinowski was regularly reading *Notes and Queries in Anthropology* as well as works directly authored by Rivers (see Malinowski, 1967/1989, p. 30, 64). *Notes and Queries* was the classic Royal Anthropological Institute field guide, by then in its fourth edition, designed to promote "far greater accuracy of detail...in the description of the social institutions of savages and barbarous races" in order to "enable those who are not anthropologists themselves to supply the information which is wanted for the scientific study of anthropology at home" (Urry, 1972, p. 46, 47). It had been produced largely under the direction of Edward Burnett Tylor—the monumental figure of nineteenth century British anthropology—and, in the words of Tedlock, was "filled with ethnocentric ideas and leading questions" (2000, p. 456). Early editions of the handbook were primarily intended for travelers,

merchants, colonial officials, and missionaries, but by the start the twentieth century, as Rivers and others were advocating for an end to "armchair" theorizing and the need for trained investigators conducting long-term field stays (J. L. Myers, 1923), *Notes and Queries* was in increasing demand within academic circles. The 1912 edition, the one that Malinowski brought to the field with him, had been the first to include a general chapter on methods. Thus "Malinowsky," being both ambitious and new to field research and making the critical mistake of thinking that natives represented "walking data," might have earnestly followed the direction of this research guide and, as Saville's note suggests, immediately sought to question the Magi (people of Mailu) on anything and everything possible.²⁵

Rivers, who introduced many of the methodological innovations into the 1912 edition of *Notes and Queries*, was progressive enough in his thinking to advocate the importance of narrative inquiries that allowed interviewees "to talk freely on subjects or independently to volunteer information" as opposed to direct questions and answers (Urry, 1972, p. 51).²⁶ Yet there was a conspicuous gap between Rivers' ideas regarding best research practices and what he actually did in the field. For example, Rivers' most recognized contribution to anthropology, then and now, is a highly structured genealogical method—used by Malinowski (see 1922/1966, p.14)—which most certainly encouraged direct questioning and answering (Stocking, 1983b). Furthermore, many key tenets of Rivers' "intensive study," for instance the importance of studying native customs "by means of the vernacular language" (see "Malinowski encounters the Cambridge School"), were practices he did not follow himself. Rivers also very much stayed on the verandah. For example, in his celebrated "several months" (M. W. Young, 2004, p. 162) of field research among the Todas of Southern India, which Stocking (1983b) regards as his only research attempt that verged on "intensive study" (p. 89), Rivers stayed in the resort station house, which "catered to the needs and past times of colonials" (Singer & Dakowski, 1986a). One can imagine the "Rider Haggard of anthropology"—as Malinowski referred to Rivers (Stocking, 1998, p. 268)—sipping whisky and soda as he went about filling his many sheets of paper with "savage texts."

Rivers' failure to act upon his own ethnographic innovations, in my reading of this history, justifies his secondary status. Marilyn Strathern (1987) warns us that ideas alone can be deceptively

ambiguous; what matters is practice, or the “effectiveness of the vision [and] the manner in which an idea [is] implemented” (p. 253). This insight is no less true today than it was a hundred years ago. You could even, quite easily I believe, make the case that, with the expansion of higher education and most particularly academic publishing, the pressure to present a new idea, to say something different from what has come before, has increased exponentially. Thus the need—every five years it seems—to announce a new “historical moment” along the qualitative research timeline (Denzin & Lincoln, 2008; see also note 1). Without neglecting or condoning the now well-documented and discussed wrongs of ethnography’s past,²⁷ the most novel of practices for moving forward may involve the reinvestment in and scholarly extrapolation of the merits of the pioneers.

At the start of his time in Mailu, Malinowski would have likely been situated somewhere between the ideals his mentor preached and the actualities that he practiced. The novice researcher’s colonial temperature can be gauged from the inventory of supplies he purchased prior to leaving Europe, which (among the expected medicines, first-aid and camping supplies) included tins of sliced bacon, jugged hare, roast turkey, kippered herring, lobster, oysters, Swiss cheese, Dutch beans, Spanish olives, Suchard’s vanilla chocolate, Peter’s milk chocolate, six different jams, dried fruit, biscuits, and morning tea, two bottles of French brandy, an “oil-cotton coat with special collar and sou’wester,” a “Cawnpore sunhelmet complete with oilskin cover,” two pairs of “light-coloured puttees,” two pairs of “colonial boots,” two Norfolk jackets and breeches, two-dozen “custom-made” notebooks, nine writing pads, three-bottles of ink, six dozen wax cylinder records, a quarter-plate Klimax camera, and a single toothbrush (M. W. Young, 2004, pp. 264–267).²⁸ One should take care to consider this list in its proper historical context—that is, early-twentieth-century England—and certainly Malinowski’s mentors had a hand in advising him on what to take. The point is that coming from a context that represented the pinnacle of coloniality, despite his dislike for missionaries and misgivings about the colonial enterprise, it would have been impossible for Malinowski to be anything but colonial.²⁹

Ethnography’s Colonial Impetuses

Malinowski’s list of fieldwork necessities gives us pause to consider what tools and luxuries ethnographers take with them to the field. More than

a delineation of specific items—although certainly the technologies of research demand some consideration of these—this issue is more productively explored by reflecting on the relationship between researcher and research communities, and how what ethnographers choose to take comes to define them.

In Malinowski’s time, ethnography was unmistakably a colonial project with the quality and distribution of ethnographic knowledge conforming to the borders of empires (Thornton, 1983). Its continuities with European expansion are unmistakable. According to C. Loring Brace (2005), perceptions of categorical differences between groups of people—which we can consider in terms of both physical and cultural differences—emerged with advancements in nautical technology and navigational capabilities starting in the fifteenth century. Where prior travel, whether by land or coast-hugging ships, occurred in increments of twenty-five miles or less, developments in maritime machinery and knowledge enabled travelers to set out from a port in one location and arrive in destinations where people and lifeways looked drastically different. Magnified through Age-of-Exploration demands for increased trade to support Europe’s growing populations and industries as well as Enlightenment emphases on rationality and scientific understanding (Robinson, 1983/2000), accounting for human difference became an important vocation.

This effort to understand and explain differences in how people looked and lived is very much at the heart of what was thought of as anthropology during the eighteenth and most of the nineteenth century. By the close of the latter, with the project of colonial conquest reaching its apex in Africa, Asia, and the Pacific, the endeavor to document the lifeways of different social groups was seen as serving the multiple purposes of mapping human social evolution—primarily as a means of rationalizing imperial dominion—recording rapidly changing cultures, and figuring out how to better administer colonial subjects. Through the efforts of members of the Cambridge School and cross-Atlantic counterparts associated with the Bureau of American Ethnology in the United States (Judd, 1967), travel and voyages were scientificized as expeditions, and explorers, once the instruments of data collection about supposed “savage” ways of life, were replaced by ethnologists, ethnographers, and other types of anthropologic fieldworkers. Likewise the travelogue gave way to the ethnographic manuscript (Urry, 1972; Thornton, 1983).

The fact that in many cases—Malinowski’s certainly being one—there were enough colonial

agents already present in a remote field site to cast most ethnographers as familiar (i.e., a typical white person), and only circumstantially as oddities, is a telling comment on the lack of field work isolation even during this early period.

Indeed Robert Lowie gives an amusing account of once being accosted by a young Crow Indian about his business on their reservation. When Lowie, attempting to explain the business of anthropology with childlike simplicity, said, "I am here to talk with your old men to find out how they used to hunt and play and dance," the young man—who apparently had never been off the reservation—replied, "Oh, I see, you are an ethnologist" (Lowie, 1959, p. 60).³⁰ This can be contrasted against situations in which community members have no understanding of what an ethnographer is or does and therefore make sense of a researcher's presence through their own cultural frames of reference (Pouwer, 1973; McLaren, 1991). Although Lowie's work on the Crow reservation took place long after the (idealized) first-contact situation, it speaks to the extent that ethnographic researchers were in many cases fixtures of a larger imperial apparatus.

The emphasis on studying small-scale "non-Western" societies—either in the interest of documenting what were erroneously thought to be rapidly disappearing cultures (Hallowell, 1960/2002) or as a means of offering profitable cross-cultural comparisons through presentations of values and practices that were sufficiently distinct from the researcher's own—curtailed ethnographers' interest in fitting in. For such societies were usually located on the frontier of imperial expansion: for nineteenth-century America, they were communities of native peoples in the manifest destinations of the territories to the west; for Europeans (most notably the British), they were in Africa, India, and the islands of the Pacific.

Reflecting on the rational standpoint that, at the time, was considered essential to these cross-cultural investigations, Johannes Fabian (2000, p. 7) remarks on the varying amounts of "protective equipment" that aided pseudo-scientific travelers in maintaining physical and intellectual distance. Certainly the "necessities" that researchers take with them into the field and the decisions they make about how to present themselves should be considered legacies of this endeavor. Malinowski's list shows an obvious lack of concern with integrating and perhaps the intention of presenting his colonialist superiority, possibly even to the other Westerners who were already there. He might not be blamed since

the level of integration—or more precisely the level of isolation from the contaminating influence and company of white men—he ultimately aspired to was unprecedented within the Cambridge School.³¹ Malinowski was not interested in presenting himself as a native. He was interested in "wak[ing] up every morning to a day, presenting itself to [him] more or less as it does to a native" (Malinowski, 1922/1966, p. 7). He had no desire to become a Trobriander but rather an intense desire to take on a native standpoint. What he appears to have understated is any consideration for the extent to which his self-presentations hindered his efforts to cease being "a disturbing element in the tribal life" (p. 8).

This is in stark contrast to later ethnographies, particularly in the postcolonial era, where researchers and the communities they study do not look, and in fact might not be, all that different. Today we see more conscious efforts on the part of researchers to present themselves in fashions that facilitate their fitting in, and, one may presume, to conceal those aspects of their personalities or those day-to-day "necessities" that most strikingly mark them as different. For example, in his research among Arab professionals in turn-of-the-twenty-first-century Brazil, John Tofik Karam (2007) took several intentional steps to polish his appearance in the interest of meeting the expectations of the people he worked among, these included upgrading his wardrobe and cutting his dreadlocked hair.³² Comparatively, in my own research among underground hip hop musicians (Harrison, 2009), I very consciously wore my hair in dreadlocks and purchased a book bag displaying a fashionable hip hop label, which helped mark me as someone involved in the scene.

An Intersubjective Science

As research practitioners, ethnographers intrinsically operate in the physical, social, and psychological spaces of the in-between. This position is reflected in ethnography's guiding vantage point, participant observation, which is regularly (although erroneously) equated with the methodology itself. Ideally, the classic ethnographer was at once a participant and an observer. Such liminality extends from personal situatedness to the realm of societal belonging. Although ethnographic writings frequently celebrate instances of researchers being accepted by, and thus belonging to, the communities they study, these relationships are in most cases conditional. In fact, in classic ethnographic discourse it was just as common for a community to be represented as belonging to the researcher (i.e., "my village" or

“my people”) or for there to be suspicions surrounding an ethnographer “going (too far) native.” Even in instances where researchers choose to study the communities they belong to—referred to as native ethnography³³—the acts of conducting research can serve to extract the researcher from their community in meaningful and potentially consequential ways (see Brayboy & Deyhle, 2000). Furthermore, there is a popular anthropological wisdom, which I believe has more than a shred of truth to it, suggesting that those most drawn to the discipline have difficulty fitting in within their own societies.

I mention all this to shed greater contextual light on the interpersonal negotiations that ethnographers must persistently grapple with. The everyday practice of ethnographic participation, observation, inquiry, and engagement marks another zone of in-betweenness where relationships, understandings, and methodological scripts are never settled. In this regard, the ethnographer is a perpetual improviser and social bricoleur, both “adept at performing a large number of diverse tasks” (Lévi-Strauss, 1962, p. 17) and cobbling together a social role out of whatever unexpected rapids the stream of ethnographic experience holds. As such a strict set of prescribed methods simply does not suffice.

Ethnographic research is dialogic, intersubjective, and intrinsically incomplete (Kondo, 1986). Its multiplex methods start from an act of intervention into the fabric of daily life in which the researcher—their presence and behavior—is continuously being interpreted by the fashioners of the social world they wish to examine (Williams, 1996). At times this negotiation of observation and presentation compels researchers to subordinate certain aspects of their identities (Tsuda, 1998) or to embrace the idea that the research process can be transformative for both ethnographers and members of the communities they work within (D’Amico-Samuels, 1991/1997). Peter McLaren insists that contemporary field researchers must consider the conditions and ends to which they “enter into relations of cooperation, mutuality and reciprocity with those whom [they] research” (1991, p. 150). Questions of who the ethnographer is and what their business is within the community are part and parcel to this process. This can lead to specific inquiries regarding sources of funding and institutional affiliations, which have the potential to betray ethnography’s more benign characteristics.

Technological Rapport

Another kind of “protective equipment” frequently deployed by ethnographers in the field is

the technologies of recording that they take with them into research. In Malinowski’s case, we see the instruments of writing field notes, namely ink, writing pads, and notebooks, as well as wax cylinders for making field recordings.³⁴ For both their material presence and role in data collection and analysis not to mention their use in maintaining communication with the world beyond “the field,” these instruments can significantly affect the depth and texture of ethnographic relations. Even the activity of field note writing (typically) marks participant researchers as different from members of the community where research is conducted. That is, although the researcher might take part in all the same activities as “natives,” at the end of the day—when “natives” retire to do whatever it is they do—the ethnographer goes home to write about culture (Clifford, 1986; Emerson, Fretz, & Shaw, 1995).

Over recent decades this process of documentation has evolved to include possibly more conspicuous technologies (depending on the setting). Malinowski’s Klimax camera was certainly one of his heftier purchases. The introduction of new technological machinery—for instance iPads or do-it-all smart phones—has the potential to disrupt the everyday life today’s ethnographers seek to observe. Erica Brady (1999) explains how, just as the ethnographer of the early twentieth century became a common part of the typically observed community landscape, these technologies of recording should be understood as things that ethnographic subjects respond to and form relationships with, often as a means of serving their own interest (see Menon, 2010). The miniaturization and global proliferation of technologies (Appadurai, 1990) over the course of the twentieth century has made them increasingly more familiar in all “fields.” Even so, their notable introduction into everyday social settings in which one would not typically find them tends to highlight the researcher–subject dichotomy and extraction-of-data agenda in ways that many contemporary ethnographers would rather minimize. In this interest, various strategies are employed. These range from using jottings as a technique of clandestine field note writing to efforts towards familiarizing research subjects with a piece of recording technology by making it available to them for non-research purposes—for example, allowing children to play on one’s laptop computer prior to using it to record an interview or using a camera to take family photos in addition to more intentionally ethnographic ones.³⁵

In an effort to prioritize equitable social relationships over extractive research ones, some ethnographers

choose to participate more and record less. This is done with the awareness that experiences of recording (for instance witnessing an event through a video camera lens) are distinct and atypical forms of participation with potentially distorting effects. Indeed, even Malinowski recommended that:

it is good for the Ethnographer sometimes to put aside that camera, note book and pencil, and to join in himself [*sic*] in what is going on. . . Out of such plunges into the life of the native . . . I have carried away a distinct feeling that their behavior, their manner of being, in all sorts of tribal transactions, became more transparent and easily understandable than it had before. [1922/1966, pp. 21–22]

Of course, this dichotomy gets collapsed within paradigmatic outlooks that recognize the researcher as having a role in actively constructing the social environment they study (see the earlier discussion).

At the same time, many sites of contemporary ethnography are increasingly saturated with technologies of recording—such as smart phones that allow for photography, video, and sound recording all at one time—making the activity of recording and the introduction of a technology nothing particularly out of the ordinary. On the surface this ubiquity of recordings may be viewed as benefiting the project of documenting native life without having the documentation process or technologies disturb its rhythms and fabric. However, this simultaneously introduces new sets of issues. These particularly concern the minimization of traditional ethnographic authority, the extent to which ethnographic research and researchers have become surveillable, and possible conflicts and contradictions surrounding who must (and who must not) adhere to institutional regulations. Ultimately, such developments have the potential to augment, jeopardize, and transform the ethnographic project, perhaps all at once.

At the height of anthropology's "crisis of representation" (see "Malinowski's literary (re)turn"), Geertz astutely commented that, traditionally:

[t]he ability of [ethnographers] to get us to take what they say seriously has less to do with either a factual look or an air of conceptual elegance than it has with their capacity to convince us that what they say is a result of their having actually penetrated (or, if you prefer, been penetrated by) another form of life, of having, one way or another, truly "been there."
[1988, 4–5]

An historical arc can be drawn starting from an era when ethnographic accounts, by names like Malinowski and Mead, were seldom challenged on the basis that, quite simply, no other trained researcher had *been there* to a period of ethnographic proliferation where multiple researchers had worked within the same societies. Even accounting for the half century between their studies and the shifts in styles of ethnographic reporting, Annette Weiner's (1976) Trobriand Islands ethnography is notably different from Malinowski's (Jolly, 1992). A few years later, Derek Freeman (1983) was attacking Mead's work in Samoa (1928/1961) on the basis of both her methods and findings.³⁶

In addition, during the post-World War II decades, members of what for lack of a better term might be called "traditionally studied communities" began having a greater presence in anthropology.³⁷ Though there had been a long disciplinary history of native community members working closely with ethnographers, and in some cases being encouraged to publish their own work and/or enter the discipline (Lassiter, 2005), initially such key informants were regarded primarily as tools who through their organic insider-ness could get "the inside scoop" (Narayan, 1993, p. 672). In contrast, the native anthropologist who came of age during that latter half of the twentieth century brought with them "a set of theories based on non-Western precepts and assumptions" (D. Jones, 1970, p. 251) with the critical politics of post-colonialism to support them. Even outside these trained professionals, the one-time omniscient voice of the lone ethnographer who had "been there" was additionally challenged by community members who were often Western educated and had both access to the research that had been conducted on them and avenues for talking back.

These late-twentieth-century challenges to ethnographic authority are magnified in the early twenty-first century context of widespread social and data-based documentation, social networking, and what Andrea Fontana and James H. Frey (2000, p. 647) refer to as "interview society." Social media—for instance, a YouTube video of an event that has been posted and commented on for months prior to the time necessary for a peer-reviewed publication—makes it possible for virtually anyone to feel as if they have *been there*.³⁸ As John L. Jackson (2012) has recently pointed out, under many of today's ethnographic conditions it is quite easy to follow a researcher's backstage activities. Furthermore, from blogs to online (customer)

reviews of ethnographic texts, the possibilities for public comment have enabled research subjects, as well as everyday people, to engage in public dialogues about research. In short, modes of ethnographic inquiry and reporting are no longer the exclusive province of trained academics (Holmes & Marcus, 2008), with both the process of research and the scrutiny of research reporting open to wider circles of participants.

Jackson describes the “internet as a mechanism for humbling ethnographic voyeurism” (2012, p. 495). Indeed, the emergence of these new modes of dialogue may mark the future of ethnography, but the multitude of voices and the potential for rhetoric (particularly among those with little to no social research background) to trump careful reflection and grounded analysis within the public domain may signal the amplification of what some already regard as an unproductive methodological quagmire.

That academic ethnographers, on the basis of their training, disciplinary identities, and institutional affiliations are required to follow not only important ethical principles but also institutional regulations—most notably in the form of institutional review board (IRB) compliance—which often appear to be more interested in protecting the institution from lawsuits than in protecting human subjects (Lincoln & Tierney, 2004), creates further complications in an age when the conducting and broadcasting of personal studies is so pervasive. Following the 1970s “Belmont Report” (1979), IRBs were set up to “ensure freedom from harm for human subjects, to establish the likelihood of beneficence for a larger group (of similar research participants), and to ensure that subjects’ consent to participate in research is fully and authentically informed” (Lincoln, 2005, p. 174). Where human subjects’ protections were initially directed towards research in health, they were soon applied to all interactive research on people. Among qualitative researchers there have always been question regarding IRB regulations’ applicability to studies as benign as oral histories or as unpredictable (i.e., difficult to outline in an IRB protocol) as ethnography,³⁹ as well as concerns about the ability of IRB members—most of whom come from the “harder sciences”—to understand and appreciate what ethnographers do. One constant case for comparison, which perhaps most effectively brings to light many of the grievances of contemporary ethnographers operating in environments of ubiquitous social documentations and media, is with journalists who in many ways operate similarly to qualitative researchers but are

not bound to the same ethnical principals or, more importantly, regulatory constraints.

Malinowski “Checks Out” the Trobriands

Malinowski’s regulatory constraints seem to have been few. He appears to have arrived at his ultimate ethnographic destination—the Trobriand Island of Kiriwina—somewhat serendipitously. What started as a one-month stop along the way to New Guinea’s northern coast—“to get an idea of what was going on [in the Trobriands],” he reported to Seligman (who presumably wanted him to go elsewhere), assuring him that the stay was only temporary (Stocking, 1992, p. 249)—resulted in “about two years” (Malinowski, 1922/1966, p. xvi) of field research. Yet this escape from colonially infested Mailu to the uncontaminated Trobriands was not as isolated as the “off the verandah” legend and Malinowski himself portray it. Early in *Argonauts’* famous first chapter, Malinowski outlines the *proper conditions for ethnographic work*:

It must be far enough away [from the company of other white men] not to become a permanent milieu in which you live and from which you emerge at fixed hours only to “do the village.” It should not even be near enough to fly to at any moment for recreation. *For the native is not the natural companion to a white man*, and after you have worked with him [*sic*] for several hours... you will naturally hanker after the company of your own kind. But if you are alone in a village beyond reach of this, you go for a solitary walk for an hour or so, return again and then quite naturally seek out the natives’ society, this time as a relief from loneliness, just as you would any other companionship. [pp. 6–7—emphasis added]

Stocking (1992) refers to Malinowski’s “aleness” among the Trobrianders as “relative rather than ‘absolute’” (p. 251). Should he have had a hankering, Malinowski could seek the company of his “own kind” just a few miles away. At the time Malinowski arrived on Kiriwina looking to pitch his tent, the largest Trobriand Island had both a hospital and jail; moreover, its resident magistrate had recently “persuaded” the Kiriwinians to line the paths of the island with 120,000 coconut trees by “imposing stiff penalties for failure to do so” (Stocking, 1992, p. 249). Seligman had already conducted some preliminary fieldwork there and, as Michael W. Young explains, the Trobriands had developed quite a reputation among colonial observers for its unique virtues—not the least of which surrounded the burgeoning popular image

of its "chiefly aristocracies and exotic dancers" as "part noble savage[s], part licentious sybarite[s]"⁴⁰ (quoted in Stocking, 1992, p. 249). One of these early observers was travel writer Beatrice Grimshaw, who nominated Kiriwina as "among the most civilized" places in British Papua New Guinea (M. W. Young, 2004, p. 380). Malinowski has been credited for shifting the anthropological lens from searching for and trying to represent pure cultural forms to understanding societies in the context of colonially induced change (Kluckhohn, 1943; Fardon, 1990). Yet from his impetus to get away from missionaries to the appeal of "Trobriand beauties," Malinowski's efforts to extol the virtues of his new methodology appear to be lodged in the allure, albeit a fabricated one, of the pure and untouched exotic.

Alternative "Fields"

Traditionally the ethnographic "field" has been conceived of as remote, non-Western, and to some degree exotic. This was largely a remnant of evolutionary anthropology's emphasis on comparative (cross-cultural) analysis through holistic examinations of small-scale societies that differed significantly from the West. Yet there are important ethnographic traditions, mostly coming out of sociology, that were notably closer to home. W. E. B. DuBois's late nineteenth century resident study of Black life in Philadelphia, published as *The Philadelphia Negro* (1899/1973), should be considered one of the earliest examples of urban ethnographic study.⁴¹ Though much of DuBois's research consisted of detailed questionnaires to residents of Philadelphia's Seventh Ward, his taking up residence "in the heart of the community to be studied" (Aptheker, 1973, p. 6), his regular house to house visits to virtually all the homes in the ward, and his propensity to align with the Black people of Philadelphia and, at times, stand in militaristic opposition to what was at best a stance of paternalistic benevolence held by the project's sponsors, retrospectively marks the Philadelphia study as profoundly ethnographic. DuBois would go on to do similar field research throughout the South while at Atlanta University (1898; 1903/1996).

Far and away the most celebrated ethnographic traditions practiced outside of anthropology came from a collection of researchers associated with the University of Chicago department of sociology. The "Chicago School,"⁴² in a general sense, formed around the combined influences of Malinowskian fieldwork methodologies and German

phenomenological theory (J. S. Jones, 2010). Through their conceptualization of urban life as an assemblage of "natural areas" or "little communities," researchers affiliated with the Chicago School, under the direction and/or influence of scholars like Robert E. Park, W. I. Thomas, E. W. Burgess, and later Everett Hughes and Herbert Blumer (Becker, 1999; Vidich & Lyman, 2000) imagined the city as a social laboratory through which to examine secular differences—primarily oriented around ethnicity and various forms of "civic otherness." Between the 1920s and the early 1960s, the Chicago School released a series of ethnographic studies of specific aspects of urban life. Among the most notable were Nels Anderson's (1923/1961) sympathetic account of the life of the hobo, Frederick Thrasher's (1927) pioneering work on the urban geography of gangs, Louis Wirth's (1928) historically informed study of the social isolation of ghetto life among Jewish immigrants, several important studies of Black urban life by E. Franklin Frazier (1932; 1939; 1957) and St. Claire Drake & Horace Clayton (1945/1993), and William Foot Whyte's "participant observation" among Italian American youth residing in Boston's North End (1943/1981). Despite their more proximate ethnographic settings, most of these works conformed to the anthropological tradition of otherizing by focusing on "urban groups whose ways of life were below or outside the purview of the respectable middle class" (Vidich & Lyman, 2000, p. 49).⁴³ Indeed, when Howard Becker described the virtues of the "Chicago way" as having "all the romance of anthropology but [you] could sleep in your own bed and eat decent food" (1999, p. 8), we can imagine a romance different from Malinowski's with Annie Brunton and all things British, and rather resembling the intrigues which drew him to Kiriwina or, for that matter, might draw a Swedish tourist to attempt personal studies in 1970s Harlem.

Other notable studies that employed "the approach of the cultural anthropologist" to what could be described as closer-to-home communities in more than just a geographic sense include Helen and Robert Lynd's (1929/1956) study of a compact, homogenous, representative American city—"Middletown," also known as Muncie, Indiana (see also Lynd & Lynd, 1937); August B. Hollingshead's "typical midwestern community," "*Elmtown*" (1949/1975); and W. Loyd Warner's *Yankee City Series* (see Warner, 1963). Despite the classic place of these middle-of-the-road American ethnographic studies in sociological history (Gillin, 1957), both the Lynds' study of Muncie and Warner's "Yankee

City,”—which was known to be Newburyport, Massachusetts—received considerable criticism.⁴⁴

One of the more remarkable critiques of the Middletown studies came from Dr. Hillyer Hawthorne Straton, minister of the First Baptist Church of Muncie and a neighbor of one of the families that was prominently featured in the Lynds' study. Straton's ten-page, typewritten manuscript, written in 1937 and eventually published by Robert S. La Forte and Richard Himmel (1983), I believe, is consistent with many of the later “native criticisms” of anthropology. Straton chides Robert Lynd for “fail[ing] to live up to... [the] standard of [t]he social scientist,” citing a local columnist comment that “[The Lynds] came here with a preconceived notion of what Middletown should be.... Blind to everything else” (La Forte & Himmel, 1983, p. 255). He is particularly critical of the Lynds’ “propensity for anything that is radical, ‘new-dealish,’ or liberal” (p. 261) and in one telling passage questions the credentials of a critic who hailed the book for its sociological accuracy, arguing “How he knows is a puzzle for he has never *been here*” (p. 255—emphasis added). The critical lens brought to many of these early-to-mid twentieth century ethnographic studies of middle America anticipated the critiques from abroad that emerged as more “traditional” ethnographic subjects gained knowledge of how they were being represented and had the platforms and impetuses to say something about it.⁴⁵

Disappearing “Fields”

Several of the previously outlined historical developments that impacted relationships between ethnographers and members of the communities they study also worked to collapse the once comfortable division between “home” and “the field.” Time and space compressions (Harvey, 1991), accelerated by heretofore unconceivable levels of global interconnectness and telecommunications ubiquity exposed the lines separating the field, the academy, and everyday life as artificially imposed classifications (Wilk, 2011). Whereas previous ethnographic conventions foregrounded the significance of place—especially when activated through the classic “arrival story”—as essential to establishing the identity and authority of ethnographer as having “been *there*,” which had to be *somewhere*,⁴⁶ by the close of the last century, innovations in how ethnography was being conceptualized, particularly within anthropology, sought to dislocate and deconstruct the traditional notion of a discreet ethnographic “field” (Gupta & Ferguson, 1997). George Marcus (1995; 1998), for example, advocated mobile, multi-site ethnography

as a way of both rethinking methods and theories within globalized contexts and accounting for life ways that were fundamentally embedded within global systems (see also Appadurai, 1990; Stoller, 1997; Hannerz, 1998). In doing so, Marcus was particularly attentive to the strides that had been made within interdisciplinary fields like media studies, cultural studies, science and technology studies, and migration/diaspora studies.⁴⁷

Certainly the notion of a traditional, fixed “field”—itself a product of a colonial worldview—obscured many of the realities of contemporary fieldwork. Thus, many scholars (including several cited earlier) argue that clinging to such spatialized understandings is not only limiting but potentially nonproductive (Caputo, 2000, p. 29). Politically, the notion of a traditional “field” produces and sustains the role of academia and other at-home institutions as the “exclusive site[s] of shaping, directing, and informing the research agenda” (Rogers & Swadener, 1999, p. 437); the “out there” field remains as the place where those directives get carried out. In challenging this history, Akhil Gupta and James Ferguson assert that ethnography’s once well-established sense of location “becomes a liability when notions of ‘here’ and ‘elsewhere’ are assumed to be features of geography, rather than sites constructed in fields of unequal power relations” (1997, p. 35).

A decade into the twenty-first century, we see not only a blurring of distinctions between home and the field but, for many researchers, corresponding collapses between research and everyday life. Whereas quite recently these disappearing physical and mental spaces were thought to engender a schizophrenic existence (Hoodfar, 1994; see also Caputo, 2000), many ethnographers today, schooled in the vocabulary and conception of multitasking, would agree with Richard Wilk’s assertion that ethnography “takes the unruly business of life through a series of operations which produce an orderly narrative”:

It is not so much a stage as a process, and in reality it is always going on, because we are never simply recording what we see like cameras or voice recorders. We are interpretive instruments, and we are engaging with ethnography when we move any experience from our senses to our pen or keyboard. [Wilk, 2011, p. 24]

An Ethnographer of Ethnographic Practice

In 1922 when *Argonauts of the Western Pacific* was published, it was hailed by none other than Frazier himself as a “remarkable record of anthropological

research" by someone who had "lived as a native among the natives" (J. G. Frazier, 1922/1966, p. vii). For his part, Malinowski was exceedingly deliberate in foregrounding his methodological "innovations." Despite mixed reviews, most notably some unfavorable ones coming out of England (Leach, 1965/2000a), the myth of Malinowski—as the first field researcher to voluntarily remove himself from colonial quarters, (essentially) cut off all ties with "civilization," and immerse himself in the world of savages as a methodological imperative for understanding both their world and worldview—soon took legs. His oft-quoted summation, found on the penultimate paragraph of *Argonauts'* first chapter, stated that the ultimate goal of the ethnographer was "to grasp the native's point of view, his [sic] relation to life, to realize his vision of his world" (Malinowski, 1922/1966, p. 25). The prescriptive methods for doing this included long-term residence by a trained researcher, learning the local language rather than relying on interpreters, collecting as much data as possible on as wide a range of activities as possible—from the spectacular and ceremonial to the everyday and mundane—and taking copious field notes, and, when possible, partaking in social activities as a "participant-observer." From all that I have outlined already, it should be apparent that Malinowski's status as the "inventor" of these practices is disputable if not improbable. But more than anyone in England at the time, he took up the challenge of theorizing them through practice and was, furthermore, immodest in broadcasting his achievements. Together Malinowski's prescriptions amounted to a methodological manifesto (Strathern, 1987, p. 258; see also Stocking, 1992, p. 62) that championed contextualization, holism,⁴⁸ and the distinction between ideal and actual behavior as signaling the capacity for agency within social structures.

In this respect, Malinowski's title as the progenitor of ethnography is in some ways legitimate. Where scholars like Rivers and Marett were forthright in producing ideas regarding the correct methods for conducting qualitative research across cultures (see "Malinowski encounters the Cambridge School"), Malinowski more so than any Cambridge School scholar before him formulated his ideas through involving himself in activities of participant observation. In other words, his understandings of proper ethnography were experientially informed in the same way that ethnography as a methodology requires experiential realizations.

* * *

In the early pages of *Argonauts*—dedicated to "Subject, Method, and Scope"—Malinowski (1922/1966) made several prescient dictates that re-emerged during the late-twentieth-century ascendance of postmodern, poststructural ethnographic practices and orientations. These included:

- *Methodological transparency*: "an ethnographer, who wishes to be trusted, must show clearly and concisely . . . which are his [sic] own direct observations, and which the indirect information that form the bases of his account."⁴⁹ (p. 15)

- *Researcher subjectivity and (his solution) the importance of keeping a diary*: "As to the actual method of observing and recording in fieldwork these *imponderabilia of actual life and of typical behavior*, there is no doubt that the personal equation of the observer comes in here more prominently, than in the collection of crystallised ethnographic data . . . An ethnographic diary, carried on systematically throughout the course of one's work in a district would be an ideal instrument for this sort of study." (pp. 20–21—emphasis original)

- *Embodied knowledge cultivated through engaging the rhythm of research*: In order to "get . . . the hang of tribal life" (p. 5), "I had to learn how to behave and to a certain extent, I acquired 'the feeling' for native good and bad manners. With this, and with the capacity of enjoying their company and sharing some of their games and amusements, I began to feel that I was indeed in touch with the natives, and this is certainly the preliminary condition of being able to carry on successful field work." (p. 8)

Aside from the unintended publication of his *Diary* (1967/1989), which made previously veiled aspects of his field experiences transparent, I hesitate to champion Malinowski as a researcher who practiced all that he preached. Nevertheless, students of ethnography would be wise to note that these important aspects of how ethnography has been conceived of and conducted were articulated by Malinowski only after his informative experience conducting fieldwork.

Malinowski's Literary (Re)turn

Richard Fardon notes how following a period—which he dates to the 1970s—when emerging trends in critical and radical ethnography treated Malinowski as "definitively superseded or encompassed" (1990, p. 573), a new wave of scholarship, much of it coming out of the United States,

resurrected his significance. For this next generation of ethnographers, Malinowski's value, or more precisely the value of his "charter myth" (M. W. Young, 1988, p.1), lay in the braided inheritances of the Malinowskian method of research, theory of culture, and style of ethnographic reporting (Fardon, p. 574). The most recognized of these "Malinowskian children" (Geertz, 1988) were collectively cast under the label "postmodern ethnographers" with their craft deemed, alternately, "the new ethnography," "reflexive ethnography," "critical ethnography," or simply "postmodern ethnography."⁵⁰ Though the postmodern label, which has been criticized for obscuring more than it says (Pool, 1991), was not always embraced by those who felt it imposed on them, these scholars generally shared a number of orientations to their ethnographic practice, including an interest in deconstructing, decentring, and juxtaposing the coherence of established ways of knowing (Fardon, 1992, p. 25); a reflexive outlook on the position of the researcher relative to the community of study; concern for the constructed nature of ethnographic authority (Clifford, 1983); and attention to language, texture, and form in modes (primarily literary) of ethnographic representation (Clifford, 1986).

These paradigmatic shifts, which significantly impacted how ethnography today is thought of and practiced, have been credited to various late-twentieth-century "moments" including the publication of Malinowski's field diaries (1967/1989), important interventions from feminists and indigenous researchers (Mascia-Lees, Sharpe, and Cohen, 1989; Wolf, 1996; Harrison, 1997),⁵¹ as well as the arrival of seminal works such as Dell Hymes' *Reinventing Anthropology* (1972) and Geertz's *The Interpretation of Cultures* (1973). In contrast to this revolutionary moment model, Nash and Wintrob (1972) document how, as early as the 1950s, within anthropology an ethnographic self-consciousness emerged that challenged the discipline's naively empirical aspirations towards attaining "full-fledged scientific status." Several significant works—such as Claude Lévi-Strauss's autobiographical *Tristes Tropiques* (1955/1992) and later Gerald Berriman's *Behind many Masks* (1962)—reflected the integration of symbolic interactionist thinking into conceptions of research as process. They credit these shifts to: (a) global forces that resulted in the crumbling of the colonial regime's that anthropology had come of age under and the creation of globally-informed and post-colonially-critical (traditional) ethnographic subjects; and (b) changes

within the discipline of anthropology, including multiple studies of the same culture and a greater range of people and "personality types" becoming ethnographers (p. 530).

The ascension of the postmodern—which reached its zenith in 1986 around the publication of Marcus and Michael Fischer's *Anthropology as Cultural Critique* (1986) and Clifford and Marcus's edited volume *Writing Culture* (1986)—coalesced around a political stance concerned with questioning the terms of Western hegemony, an appreciation for the (inter)performative nature of ethnographic research and the (inter)subjective nature of ethnographic analysis, and a focus on ethnographies as texts (Marcus & Cushman, 1982).

Literal Postmodern Politics

Responding to what Mascia-Lees et al. (1989, p. 8) describe as "the need to claim a politics in order to appeal to an anthropological audience," the harbingers of postmodernism adopted (or appropriated) critical stances previously propagated by feminist, (to some extent, native⁵²) and indigenous ethnographers. Their insufficiency in crediting these positioned influences was striking given that so many of the key scholars associated with postmodernism were white males. This critique was most poignantly raised by feminist scholars who observed that "[l]ike European explorers discovering the New World, [postmodernists] perceive a new and uninhabited space where, in fact, feminists have long worked" (Mascia-Lees et al., p. 14). Indeed, where women and native ethnographers have always occupied marked positions along the axes of gender and ethnicity/race, white male researchers, as unmarked, have historically enjoyed the privilege of claiming objectivity and, quite notably, had their claims accepted by their audiences (Alsup, 2004). Postmodern skepticism about the constructed nature of truth claims coincided with a recognition of researcher subjectivity and research serendipity that was, for lack of a better way of putting it, "old news" within feminist and native ethnographic traditions. Both traditions had long questioned the assumption of political allegiance on the basis of common identity ascriptions (see Kondo, 1986; Narayan, 1993), thus compelling their adherents to critically examine the politics and experiences of fieldwork. Far from detached scholars, feminist and native ethnographers recognized their role in shaping the social worlds they participant-observed and described (Geertz, 1988). Such revelatory acknowledgements—not from the margins of ethnographic

practice but, with the rise of postmodernism, coming from its mainstream—supplied the platform for more collaborative, participatory action-based, and arts-based approaches that were to follow (Lassiter, 2005; Finley, 2005; Leavy, 2009).

Writing in the Postmodern Momentum

The most distinguishing aspect of this new ethnography—or the topic that has received the most attention—is the emphasis on the rhetorical processes involved with ethnographic production and, ultimately, the view of ethnographies as writerly projects. This literary turn was not without precedent.⁵³ Malinowski certainly thought of himself as a writer. Writing just after the “founding father” of ethnography’s death, Clyde Kluckhohn speculated that Malinowski’s “capacity for expression” would be one of the key things upon which his reputation would rest (1943, p. 209).⁵⁴ Indeed Clifford (1986) in arguing the partial and constructed nature of truth claims, and advancing the artistic dimensions of ethnography as a project profoundly situated between systems of meaning making, invoked Malinowski on the very first page of his seminal text. Even though the once-dominant aspirations for “hard science” status—marked by formalized methods leading to timeless truths—had been waning for decades, this nod to the humanities and the constructed and interpretive nature of all research was viewed by many as a “crisis” in the field.

Ethnography constructs culture through texts of contexts, which to a certain degree are valued based on their effective presentations. Arguing for what she called an anthropology of “persuasive fictions,” Marilyn Strathern suggested that ethnographers impact imaginations through relationships *internal to the text*: “the kind of relationship that is set up between writer and reader and writer and subject matter” (1987, p. 256). Stephen Tylor expressed it somewhat differently in asserting that “the critical function of ethnography derives from the fact that it makes its own contextualization part of the question” (1986, p. 139). Inspired by this wisdom, my conviction for some time now has been that ethnography, both as research and representational practice, operates in an adverbial mode (see Hammersley, 2008). It contextualizes transmutable and transposable social processes through transcriptions of the dynamic social interactions of community members and researchers. As such, the experiences of ethnographic fieldwork are (re)constructed through the process of writing first field notes (Emerson et al.,

1995) and later ethnographic monographs. Such recognition, of the mediated expressions of social processes and meanings, through acts of composition (literal and otherwise), has sprouted into a tremendous range of experimental ethnographic forms and new political possibilities—thus leaving ethnography’s horizons promising and bright.

His(torical) Legacies

Constructing a complete picture of Malinowski—the man, the field researcher, and the scholar—presents special difficulties, not the least because he was a creative intellectual with “an open and lively mind” (Flis, 1988, p. 123) whose scholarly career can be characterized as much by evolution as by stasis (Murdock, 1943). He furthermore had a penchant for flamboyance in both representing himself and the world around him. Part of this involved embracing the great storytellers’ wisdom that the context of a telling dictates the text of the tale. In this vein, it would not be too much to characterize Malinowski as having a loose interpretation of the “facts” regarding his own personal history, which he would strategically adjust to delight or in some other way influence his audience (M. W. Young, 2004). He was a master of the sketchy, revisionist memoir, which, combined with an erratic temperament that made even his journal entries and personal correspondences knavishly unreliable (Rapport, 1997), resulted in an enigmatic and elusive biography fitting of mythic status.

Malinowski was obviously aware of the pioneering nature of his work—or at the very least the potential to frame it that way—and quite concerned with his legacy. He was in essence what sociologist Gary Fine (1996) would refer to as a self-entrepreneur of his own reputation. Fine’s notion of *reputation entrepreneurs*—that is, “self-interested custodians” of someone’s historical reputation (p. 1162)—is useful for contextualizing Malinowski’s historical import and for making sense of how and why the myths surrounding him have been so enduring. As an analytic concept, reputation entrepreneurship is premised on a constructionist model of history that frames it as the outcome of sociopolitical struggles over power, prestige, and resources (Fine, 2001, p. 8). Fine specifically investigates the role of social agents in shaping the collective memory and settling discourses that surround historical figures. This can involve recognition within one’s field—in Malinowski’s case, anthropology and other scholarly fields that position ethnography at or near their core—and renown outside of it.

In addition to his achievements and how he represented them, Malinowski also laid the groundwork for future custodians of his reputation despite his untimely death from a heart attack at age fifty-eight.⁵⁵ For example, his propensity to keep journals provided the source materials for future biographers—although it is widely believed that his *Diary* (1967/1989) was never intended for publication. Young (2004) recounts how during his days in Leipzig, Malinowski exhorted himself to “Keep a diary!”; adding, “Everything that passes through me must leave a lasting trace” (p. 131). His published *Diary* similarly includes statements to this effect. In 1926 Malinowski wrote that myths “record singularly great achievements...redound to the credit of some individual and his [*sic*] descendants or of a whole community; and hence they are kept alive by the ambition of those whose ancestry they glorify” (1926/1948, p. 106). The extent to which this was true for a lot of Malinowski’s student-descendants was evident by their support of him following the controversial publication of his diaries in 1967.⁵⁶

Had Frank Hamilton Cushing had better reputation entrepreneurs, or been more organized (Brady 1999) and less prone to making enemies (Kolianos & Weisman, 2005), he might hold a status comparable with Malinowski’s. In the United States, where the objects of anthropological study—minimally defined by William S. Willis Jr. as “dominated colored peoples...living outside the boundaries of modern white societies” (1972, p. 123)—were closer at hand, research expeditions along the order of Torres had a longer history. Thirty-five years before Malinowski, Cushing had “developed” his own “reciprocal method” of field research (Mark, 1980, p. 123), when he decided to forsake his position as the Smithsonian Institute representative on the 1879 Bureau of (American) Ethnology’s first-ever southwestern expedition, in order to take up residence with the Zuñi Indians. Apparently, after becoming frustrated “at how little he could learn as an outsider” camping outside the pueblo, he “soon abandoned the tents of his colleagues and...moved in with the Indians” (Green, 1979, p. 5). Cushing lived among the Zuñi for four and half years, during which time he dressed like a Zuñi, was given a Zuñi name, became proficient in the language, took an active part in both ceremonial events and daily life, was adopted into the Dogwood clan, became a member of the tribal council, and was initiated into the Priesthood of the Bow (Pandey, 1972; Hinsley, 1983). Dubbing him the “original participant observer,” Jesse Green adds:

Cushing was the first anthropologist to have actually lived with his subjects over an extended period—and the only man in history entitled to sign himself, as he once did at the end of an official letter, “1st War Chief of Zuñi, U.S. Asst. Ethnologist.” [1979, p. 5–6]

This list of legendary feats may look somewhat different if subject to the same scrutiny as Malinowski’s.⁵⁷ Yet clearly Cushing was involved in a project that in many respects—duration of field stay, wardrobe (see any of the handful of classic photos of Malinowski in the field), formal recognition of community roles, and even acculturation, since it has been suggested that Cushing “felt more at home among the Zuñi than among his own people” (Pandey 1972, p. 322; cf. Malinowski 1967/89)—outpaced ethnography’s recognized founder.⁵⁸

What is perhaps most special about ethnography as a research tradition is its propensity to perpetually and critically assess, and at times reinvent, its methodological, theoretical, and epistemological foundations. More than anything else, what marks the ethnographer as distinct from researchers who engage in (seemingly) identical methods and activities of qualitative field research (or participant-observations) are the sensibilities that led them to research, inform them during its unanticipated courses of experiences, and, ultimately, sustain meaningful legacies thereafter.

Conclusion

Today’s ethnographers inherit the burdens of Malinowskian methodological precepts but are privileged in their ability to construct their own projects in strategic juxtaposition to those that came before them. In Malinowski’s example, both legendary and personal, the metaphors of travel and narratives of revision enact and sustain discourses that are crucial to understanding ethnography’s journey through a century of practice over epistemological, theoretical, and methodological grounds.

Among the several functions that Malinowski attributed to myths and legends, his claim that they open up historical vistas (1926/1948, p. 107) is perhaps the most apt point to close on. Mythic narratives “reflect the circumstances and perspectives of their narrators” and provide context for contemporary commentary (Fardon, 1990, p. 570). Malinowski then, through his status as ethnography’s “most mythicized” figure (Geertz, 1988, p. 75), serves as a beacon for whatever future turns ethnography’s journey into its second century as a

professionalized practice takes. His legend supplies knowledge of where modern ethnography emerged from, highlighting both its enduring value and what has thankfully been left to the past, and simultaneously inspires the need for constant criticality, revision, and above all else, contextual awareness of how far this ethnographic field has yet to go.

Future Directions

- What can historical methodological documents teach us about the development and evolution of ethnography (and about the attitudes, political views, and underlying epistemological assumptions of researchers during a particular period)?

- What are the limitations of field notes and other forms of on-the-spot ethnographic record keeping? As with tape recorders or video cameras, in some instances, might field note documentation be viewed as negatively impacting ethnographic relationships? Can ethnography exist without field notes? What recent technological innovations or modes of ethnographic inquiry and analysis could potentially substitute for them?

- Should ethnographers, on the whole or within specific disciplines, have a collective position on institutional review board compliance? Is it fundamental to what ethnographers do, or is it an unnecessary encumbrance that the increasing numbers of ethnographers outside the academy (and “everyday” ethnographers) do not have to deal with?

- In a context of ubiquitous media interconnectedness, viral news streams, and big data, how must ethnography adjust to issues of timely publishing, accountability, and the erosion of ethnographic authority in a highly mediated, data-based “interview society”?

- As the lines between ethnography and everyday life become increasingly fuzzy, what new modes of ethnographic understanding and representation should be acknowledged and embraced?

- In ethnography’s post-postmodern reformulations and trajectories, how should ethnographers map the boundaries of the field (epistemologically and in terms of the various interests which ethnographic study can serve)?

- Ethnography’s foundations are in writing culture, yet historically ethnographers are deeply implicated in the project of literatizing non-literate societies. Given this paradox, what non-literal forms of ethnographic representations might a

contemporary, critical, and historically informed ethnographic project take? How can we move beyond *writing culture*?

Notes

1. For instance, Norman K. Denzin and Yvonna S. Lincoln identify “at least eight historical moments” in qualitative research history; writing in 2008 (p. 3), they list these as: the traditional (1900–1950), the modernist (1950–1970), blurred genres (1970–1986), the crisis of representation (1986–1990), the postmodern (1990–1995), the postexperimental (1995–2000), the methodologically contested present (2000–2004), and the fractured future (2005–). While I see value in their effort to assign broad themes to various time periods, I am less comfortable with the accelerated momentum of their model. To define the four year period of 2000–2004 as an “historical moment” on par with the first fifty years of the twentieth century strikes me as peculiar—something like a historiographic version of the old social evolutionist claims that non-literate peoples had been living the same way for the last thousand years. More to the point, to place six “historical moments” between qualitative research as practiced in 1948 and that practiced in 2008, from where I sit, misleadingly magnifies the impression of how far it has come.
2. Arthur J. Vidich and Stanford M. Lyman view this conflation as unwise and unserviceable, arguing that the ethnographic “data gathering process can never be described in its totality because . . . [it is] part of an ongoing social process that in its minute-by-minute and day-to-day experience defies recapitulation” (2000, p. 38).
3. Several very good overviews of ethnographic qualitative field research methods exist, including Hammersley & Atkinson (1995), Bernard (1995), Bailey (2007), and Emerson et al. (1995).
4. The classic definition-of-culture example comes from Alfred L. Kroeber and Clyde Kluckhohn (1952), who compiled 162 different definitions of the term.
5. *Ethno* is derived from the Greek *ethnos*, which refers to “people, nation, class, caste, tribe; a number of people accustomed to live together;” and *graphy* is derived from the Greek *graphia*, meaning “description of.” These etymological definitions came from the Online Etymology Dictionary: <http://www.etymonline.com/> (Retrieved July 16, 2012). Similar breakdowns can be found in Jones (2010).
6. Of course, Malinowski had already received a doctorate from Jagiellonian University in Cracow before he went to England (Ellen, Gellner, Kubica, & Mucha, 1988), but because that degree is typically listed as in physics and mathematics, it is regarded as incidental to his later work.
7. See for example Stocking (1983a), M. W. Young (1988), Geertz (1988), and Kuper (1996).
8. By one popular account, Malinowski’s status as an “enemy alien” (Wayne, 1985, p. 533) prevented him from returning to Europe (see Kuper, 1996; J. S. Jones, 2010). By another—first relayed to me as an undergraduate—Malinowski’s journey to the southwest Pacific was engineered in part to dodge the outbreak of war in Europe. To the extent that this alleges an avoidance of military service, it seems untrue since owing to his health troubles, most notably issues with his eyesight, Malinowski was deemed unfit to serve (M. W. Young, 2004, p. 38).

9. Regarding class, Malinowski's daughter Helena Wayne (1985) writes that both of her paternal grandparents belonged to a class that to her knowledge had "no exact equivalent" in Europe—"between landed gentry and nobility, but certainly not aristocracy" (p. 529). The story of young Malinowski being read *The Golden Bough*—which is contradicted by at least one testimony from Malinowski himself regarding his first "read[ing] [emphasis added] this great work" (Leach, 1965/2000a, p. 26)—can be traced to a 1923 letter written to Frazier (cited in Stocking, 1983b, p. 93). It is clear that Józefa Malinowska read a good deal to her son during his secondary-school years and beyond when trouble with his eyesight forced him out of school and to spend significant time with "his eyes bandaged" (Wayne, p. 530). By one account, she even forbade him to read, opting instead to "read everything to him herself" (M. W. Young, 2004, p. 38).
10. Stocking (1992) also cites these "preadolescent experiences at the cultural margins of Europe" as inspiring young Malinowski's "romantic fascination with the culturally exotic," adding that his father's interest in folklore (see below) and Malinowski's perspective of having grown up in a "subjugated nation" may have also contributed to his turn towards anthropology (p. 241).
11. Young (2004) has suggested that Malinowski's opportunities to work with Wundt might have been truncated by the latter's age and career stage, not to mention his responsibilities as university rector.
12. Robert Redfield writes in his introduction to Malinowski's *Magic, Science, and Religion*, "Malinowski's gift was double: it consisted both in the genius given usually to artists and in the scientist's power to see and to declare the universal in the particular" (1948, p. 9).
13. By some accounts of the Malinowski myth, it was his sickness that caused him to break from his path to science (Kuper, 1996, p. 9). To the extent that this may be partially true—and both his extracurricular readings and Mediterranean travels *could* be construed as a product of illness—it might be extended to also include love-sickness.
14. Of course, this is a highly simplified explanation. For a thorough discussion of the various paradigms and epistemologies surrounding qualitative inquiry, see Lincoln & Guba (2000) and Schwandt (2000).
15. I use "ethnology" to reference the more theoretically informed, historically speculative, and comparative form of researching (mostly) non-literate societies that dominated the emerging field of anthropology during the late nineteenth century and first decades of the twentieth (see Radcliffe-Brown, 1952). Ethnology was "less intensive" than ethnography and often involved "armchair" theorists who adhered to evolutionist models of understanding human diversity. Initially Malinowski called his work ethnology (Firth, 1988). However, by the 1922 publication of *Argonauts of the Western Pacific*, he was clearly referring to it as "ethnography."
16. Prior to leaving Leipzig, Malinowski had already begun writing several ethnological projects including what would become his first book, *The Family among the Australian Aborigines* (see Barnes, 1963). In addition to his enthusiasm and notable intellect, these works enabled Malinowski to make an immediate impression on his eventual mentors.
17. Edward Burnett Tylor's (1871) classic definition of culture as "that complex whole which includes knowledge, belief, art, morals, law, custom and any other capabilities and habits acquired by man [*sic*] as a member of society" (p. 1) is still widely used and taught today.
18. There are countless stories of arbitrary, serendipitous, and unforeseen circumstances that led ethnographers to particular fieldwork topics and destinations. Two of the more celebrated within American anthropology are: (1) Margaret Mead's path to studying adolescence in American Samoa, which resulted from a negotiation between her desire to study culture change in the Tuamotu Islands and her advisor Franz Boas's desire to have her study adolescence among American Indians (see Mead, 1972); and (2) the story of Lewis Henry Morgan, who came to work with Iroquois leaders after a chance meeting with a young Seneca, Ely Parker, in an Albany New York bookstore (see Lassiter, 2005).
19. There is also evidence suggesting that Haddon may have secured a travel grant for Malinowski (M. W. Young, 2004, p. 245).
20. Much of Papua New Guinea, including the Trobriand Islands, was under Australian control. The rest of it was controlled by Germany. Stocking (1992, p. 242) hints at the possibility that, with the outbreak of war, Malinowski also had to negotiate this evolving imperial scramble.
21. Malinowski had one of his most fruitful periods of early research during a time when the missionary couple he stayed with, the Savilles, left Mailu for an extended period of time. In 1915 he wrote that he found this experience working among the natives "incomparably more intensive than work done from white men's settlements" (quoted in Stocking, 1992, p. 246); and again in 1922 he wrote, "it was not until I was alone in the district that I began to make some headway" (Malinowski, 1922/1966, p. 6).
22. Malinowski's continued use of "savage" throughout his career has been, at times, presented as evidence of deep-seated racism. During this time, however, the term was a common descriptor for non-Western peoples. Its association to cultural evolutionism could certainly be used to help make the cases that Malinowski was a career long evolutionist (see Kuper, 1996, p. 8).
23. Shortly after arriving in Mailu, Saville sent a letter to his brother in England in which he listed his ten "laws in dealing with Mailu-speaking natives" (or what Stocking [1992] refers to as his "ten commandments" [p. 246]); they went as follows: "(1) Never play the fool with a native; (2) Never speak to a native for the sake of speaking to him [*sic*]; (3) Swear at a native when he is alone; (4) Never call a native, send someone for him or go inadvertently to him; (5) Never touch a native, unless to shake hands or thrash him; (6) Always let a native see you mean what you say; (7) Never let a native see you believe his word right away, he never speaks the truth; (8) Rarely argue with a native and then only when he is alone; (9) Warn once, afterwards proceed to action; (10) Don't try to be funny, a native can never see a joke. He possesses one joke and that is beastly talk" (M. W. Young, 1988, p. 44).
24. The note, found among Haddon's papers, was typed and, intriguingly, neither signed nor dated—Young (2004, p. 357) is nonetheless "almost certain" that it was written by Saville.
25. Indeed, in the opening pages of *Argonauts of the Western Pacific*—the major publication introducing his New Guinea/Trobriand fieldwork and announcing his revolutionary

- method—Malinowski describes the beginnings of his field research on Mailu as “making [his] first entry into the village... in the company of his white cicerone” (presumably Saville) and later returning, where after a few exchanges of “compliments in pidgin-English” and “some tobacco changing hands” he “tried then to proceed to business” (1922/1966, pp. 4–5—emphasis added). Young (2004) confirms that “some work” was done during this “first week” on the island (p. 332).
26. In fact, J. L. Myers describes Rivers’ contributions to the 1912 edition of *Notes and Queries* as “a revelation to all but an inner circle of colleagues” and “setting a standard of workmanship in the field” (1923, p. 15). Would Malinowski, who went on to be the recognized setter of the next new standard, have been among that inner circle of colleagues? Stocking, for one, definitively names Malinowski as the last member of the “Cambridge School” to get into the field (1983b, p. 82). If by 1912 Malinowski was not a member of Rivers’ inner circle, he would have beyond any doubt been only one degree removed.
 27. See for instance Deloria (1969/1988), Willis (1972), Asad (1973), Owusu (1978), Magubane & Faris (1985), R. Rosaldo (1989), Smith (1999/2012), A. A. Young (2008).
 28. This is only a smattering of what was included. For the complete list and a discussion of its significance, see M. W. Young (2004, pp. 264–267).
 29. In a fascinating discussion, James Urry (1972) outlines how *Notes and Queries on Anthropology* was specifically marketed to colonialists to help mitigate the consequences of cross-cultural disagreements and misunderstandings. He concludes that, at the dawn of the twentieth century, “political and economic motives for the collection of ethnographic materials were becoming as important as the scientific” (p. 49).
 30. Lowie supplies an exclamation point to the story by recounting how, the following year, a New York City election official stood “completely nonplussed” after being told that Lowie’s occupation was ethnology; “[h]e evidently lacked the educational advantages of the Crow reservation,” Lowie concluded (1959, p. 60).
 31. Several North American researchers, most notably Frank Hamilton Cushing (Pandey, 1972; Green, 1979) and Boas (Cole, 1983), had previously achieved this level of integration.
 32. See Karam (2007, p. 18–19); some of the details of this account were also confirmed through personal email correspondence (August 20, 2012).
 33. See D. Jones (1970), Nakhleh (1979), Hau’ofa (1982), Ohnuki-Tierney (1984), and Narayan (1993).
 34. Curiously in the list that M. W. Young (2004) presents there is no mention of a phonograph recorder. Wax cylinders did not work well in the tropics and, as Young notes, only six cylinders (of six dozen shipped) of sound recordings survived.
 35. Thanks to Lakshmi Jayaram and Ali Colleen Neff for pointing out these specific practices to me.
 36. Also compare same-culture studies conducted by Redfield (1930) against Lewis (1951), Dollard (1937) against Powdermaker (1939), as well as Mead (1935) against Fortune (1939).
 37. Nash & Wintrob (1972, p. 531) credit the “assertions of independence by native people” in a general sense—outside of native anthropology in particular—with unsettling the self-assuredness of the Western colonial view of non-Western people.
 38. I caution that all modes of recording—including video camera—have certain biases of perspective and limitations. Nevertheless, for many audiences—and particularly Western audiences conditioned to privilege vision over other sensory input (M. Jackson, 1989, p. 6)—seeing is believing.
 39. This is by no means a one-way debate. Although many ethnographers would be more than happy to not have to deal with IRBs, some feel that by not requiring IRB approval, ethnographers would be further marginalized as unscientific and/or not *real* research (Lincoln, 2005).
 40. Young (2004) elaborates on Malinowski’s preoccupation with the “salacious details” of Trobriand sex life including what was likely a rather unnerving correspondence with Annie Brunton regarding the “sensual temptations” of Kiriwinian young women (pp. 402–405).
 41. In fact, one could quite straightforwardly make the case for the “Sage of Great Barrington” (as DuBois came to be known) as the inventor of modern ethnography.
 42. Howard Becker is critical of this designation, arguing that “‘Chicago’ was never the unified chapel... [or] unified school of thought” that many believe it to have been (1999, p. 10).
 43. This can also be seen in the ethnographies conducted by white sociologists of African American communities during the integrationist period of the 1960s (A. A. Young, 2008).
 44. On the basis of their distortions and lack of scientific rigor (Mills, 1942; Pfauts & Duncan, 1950; Madge, 1962; Colson, 1976; Frank, 1977), oversights (Thernstrom, 1964; Lassiter et al., 2004), and their presentation of ideal types of community members as opposed to portraying genuine personalities (Goldschmidt, 1950; Ingersoll, 1997). If such critical reception followed the publication Hollingshead’s studies of *Elmtown Youth* (1949), it seems to have been less publicized, most likely owing to the fact that, unlike Middletown and Yankee City, Elmtown’s true identity remained hidden.
 45. More recently some urban ethnographers have focused their attention of elite institutions—i.e., “studying up”; examples of this research include Latour (1987), Cassell (1991), Karam (2007), Foshier (2009), and Ho (2009).
 46. Even if, customarily, the researcher-as-person would then disappear into “scientific omniscience” (Coleman & Collins, 2006, p. 1).
 47. See Clifford (1994), Friedland & Boden (1994), Downey & Dumit (1995), and Marcus (1996). For some very good recent examples of transnational ethnographies, see Pribilsky (2007) and Zheng (2010).
 48. Malinowski (1922/1966) specifically said that “[o]ne of the first conditions of acceptable ethnographic work certainly is that it should deal with the totality of all social, cultural, and psychological aspects of a community, for they are so interwoven that not one can be understood without taking into consideration all the others” (p. xvi). This idea of anthropology as a holistic science continues to be reiterated in the introductory chapters of most discipline textbooks.
 49. Such transparency might seem rather pedestrian by today’s standards, but, in its historical context, insisting on these types of divulgences was a noteworthy gesture.

50. Representative examples of this work include Rabinow (1977), Myerhoff (1978), Crapanzano (1980), M. Rosaldo (1980), Taussig (1980), and Friedrich (1987).
51. For examples of such work from the feminist tradition, see Rosaldo & Lamphere (1974), Reiter (1975), and Daniels (1983); from the indigenous or native ethnography tradition, see Jones (1970), Owusu (1978), and Nakhleh (1979).
52. Inspired by Narayan's (1993) insights, I distinguish between native and indigenous ethnographers on the basis of the former being an ascribed identity and the latter being a political stance.
53. In his Introduction to *Writing Culture*, Clifford (1986) lists Geertz, Victor Turner, Mary Douglas, Lévi-Strauss, Leach, Mead (1928/1961), Ruth Benedict, as well as Malinowski as forerunners of this ethnographic tradition. I would resolutely add Zora Neale Hurston (1935/1990; 1942/1991).
54. His publications were noticeable and memorable for their poetics. The titles of his monographs alone make the case, including the dignified splendor of *Argonauts of the Western Pacific*; the crude promotional-ism of *The Sexual life of Savages and Sex and Repression in Savage Society*, which Stephen Hugh-Jones and James Laidlaw (2000) describe as "fairly low gimmicks" (p. 17); and the mystical intrigue of *Coral Gardens and Their Magic*.
55. Fine (1996) cites institutional placement as one of the key factors in enabling reputation building and sustainment. Beyond his position at the London School of Economics and his paramount role in establishing it as the leading center for anthropology in Europe, through his outstanding lectures and excellent mentorship (Kluckhohn, 1943) Malinowski cultivated a generation of scholars—among his academic progeny were some of the biggest names in twentieth century anthropology—who would continue to sing his praises for years to come.
56. For a good discussion of this, see Firth's (1989) "Second Introduction 1988" to the republication of Malinowski's *Diary*.
57. And very much like Malinowski, Cushing was not beyond strategically constructing his own legend (see Green, 1979, p. 25 n. 5; Koianos & Weisman, 2005).
58. Other prominent candidates for "original participant observer" include Malinowski's American anthropological counterpart, Franz Boas (see Rohner, 1969; Cole, 1983); Alice Cunningham Fletcher, who first traveled to Nebraska in 1881 in the interest of studying the life of Omaha women and ended up "traveling with the Omahas for weeks at a time, learning their customs and listening to their fears" about being taken advantage of by the American government (Mark, 1980, p. 67); Nikolai Mikouho-Maclay, the Russian fieldworker who in 1871 found himself "virtually alone among previously uncontacted and totally 'untouched' groups" on the northern coast of New Guinea (Stocking, 1992, p. 222); and Lewis Henry Morgan, whose *League of the Ho-de-no-sau-nee* (1851) has been referred to as "the first 'true ethnography'" (Lassiter, 2005, p. 30).

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