Collaborative Ethnography and Public Anthropology

by Luke Eric Lassiter

Collaborative ethnography—the collaboration of researchers and subjects in the production of ethnographic texts—offers us a powerful way to engage the public with anthropology. As one of many academic/applied approaches, contemporary collaborative ethnography stems from a well-established historical tradition of collaboratively produced texts that are often overlooked. Feminist and postmodernist efforts to recenter ethnography along dialogical lines further contextualize this historically situated collaborative practice. The goals of collaborative ethnography (both historical and contemporary) are now powerfully converging with those of a public anthropology that pulls together academic and applied anthropology in an effort to serve humankind more directly and more immediately.

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While sustaining our fundamentals, probing the deep mysteries of the human species and the human soul, we must press outward, mobilizing our work and ourselves to make a difference beyond the discipline and the academy.

—JAMES L. PEACOCK

In his often cited essay "The Future of Anthropology," James L. Peacock (1997:9) set forth three possibilities for anthropology in the coming century: "extinction," "hanging on as [a] living dead," or a "flourishing redirection of our field into a prominent position in society." Focusing on this latter scenario, he argued that we must direct our efforts toward a renewed emphasis on anthropology's relevance to wider publics.

Peacock's essay marked a revitalization of earlier disciplinary conversations about how to "make a difference beyond the discipline and the academy." As anthropologists had in the 1960s and 1970s, we once again debated how to bridge theory and practice and craft a more activist and engaged anthropology. Indeed, Peacock's three scenarios for anthropology's future echoed the three strategies proposed by Dell Hymes in *Reinventing Anthropology* (1969:39–48) almost three decades earlier: to retrench (i.e., to reduce anthropology to the study of prehistory, the "primitive"), to let go (i.e., to be absorbed by other disciplines), or to relax (i.e., to reconsider anthropology's organization and to reconfigure its trajectories). "The issue is not between general anthropology and fragmentation," wrote Hymes (p. 47), "but between a bureaucratic general anthropology, whose latent function is the protection of academic comfort and privilege, and a personal general anthropology, whose function is the advancement of knowledge and the welfare of mankind."

Many anthropologists, past and present, have answered the challenge to redirect and reinvent anthropology along such lines as those articulated by Hymes, Peacock, and others (see, e.g., Sanday 1976). Some, however, have met these arguments with ambivalence. In particular, many applied anthropologists have wondered if such invention and reinvention is even necessary given the continuing vigor of its applied dimension. Merrill Singer (2000), for example, contends that the latest academic effort to invent a public anthropology is more a reiteration of hierarchical divisions between academic and applied anthropologists than a more broadly conceived proactive anthropology. "The avenue for approaching these goals," writes Singer (p. 7), "is through strengthening, valuing and more fully integrating applied/practicing anthropology, rather than inventing new labels that usurp the role of public work long played by an already existing sector of our discipline."

Singer is right. A perusal of past and recent issues of *Human Organization* or *Practicing Anthropology* will quickly put to rest any doubt that anthropologists are actively engaged in the public domain both as practitioners and as theoreticians. But Peacock, Hymes, and the many others who have written about redirecting and reinventing anthropology are also right. Paradoxically,

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the redirection of anthropology is still important for the very reasons put forth by Singer: anthropologists—particularly academic anthropologists—continue to struggle with reconciling anthropology's applied, public, and activist roots with the discipline's elite positioning in the academy. "Such a castelike assumption," writes Hymes (2002:xxiii), "ill befits a field that claims to oppose inequality. We teach against prejudice on the basis of race, language, and culture. Despite our praise of fieldwork, have we preserved an unspoken prejudice in favor of ourselves as literati?"

To be sure, the crux of the problem is primarily academic (Basch et al. 1999:3–20). After all, we train both future academic anthropologists and future applied anthropologists in the halls of academe (cf. Basch et al. 1999). Yet the larger problem remains the integration of theory and practice, research and training, the joining of academic and applied anthropologists, uninhibited by hegemony, in a common project, and the engagement of anthropologists with wider publics within and outside of academia (cf. Hill 2000). As Peggy Sanday (1998) suggests, merging anthropology with public currents "is more than a focus for research; it is a paradigm for learning, teaching, research, action, and practice within the field of anthropology."

Robert Borofsky (2002) suggests that this larger project "affirms our responsibility, as scholars and citizens, to meaningfully contribute to communities beyond the academy—both local and global—that make the study of anthropology possible." Anthropologists such as Philip Bourgois (1995), Paul Farmer (1999), Laura Nader (2001), and Nancy Scheper-Hughes (2000), as well as a host of others (see, e.g., MacClancy 2002), have provided compelling cases for what this public anthropology should look like. From human rights to violence, from the trafficking of body parts to the illegal drug trade, from problem solving to policy making, from the global to the local and back again, the issues informing this evolving project to merge anthropology with public currents have proven diverse and multifaceted.

An important component of this (re)emergent public anthropology is a heightened (re)focus on collaboration with the publics with which we work (cf. Moses 2004). Collaboration has of course long been an important part of the applied and public work of anthropologists (see, e.g., Stull and Schensul 1987), and activist and applied research strategies such as participatory action research have long recognized a responsibility to publics outside the academy (see, e.g., Kemmis and McTaggart 2000). All the same, however, collaborative research with research subjects is only recently entering onto anthropology's center stage as a necessary condition of both applied and academic work. We no longer just choose to engage in collaborative research with our subjects; collaboration is increasingly conditioning not only our advocacy but our so-called pure research as well. In the wake of the recent Tierney affair, for example, the American Anthropological Association's (2002) El Dorado Task Force singled out collaboration as follows:

The El Dorado Task Force insists that the anthropology of indigenous peoples and related communities must move toward "collaborative" models, in which anthropological research is not merely combined with advocacy, but inherently advocative in that research is, from its outset, aimed at material, symbolic, and political benefits for the research population, as its members have helped to define these. . . . Collaborative research involves the side-by-side work of all parties in a mutually beneficial research program. All parties are equal partners in the enterprise, participating in the development of the research design and in other major aspects of the program as well, working together toward a common goal. Collaborative research involves more than "giving back" in the form of advocacy and attention to social needs. Only in the collaborative model is there a full give and take, where at every step of the research knowledge and expertise is shared. In collaborative research, the local community will define its needs, and will seek experts both within and without to develop research programs and action plans. In the process of undertaking research on such community-defined needs, outside researchers may very well encounter knowledge that is of interest to anthropological theory. However, attention to such interests, or publication about them, must itself be developed within the collaborative framework, and may have to be set aside if they are not of equal concern to all the collaborators. In collaborative research, local experts work side by side with outside researchers, with a fully dialogic exchange of knowledge (that would not, of course, preclude conventional forms of training).

While some anthropologists were quick to dismiss the task force's recommendations (see, e.g., Gross and Plattner 2002), its call to pull advocacy and research into the same stream nonetheless marked a widening agreement among anthropologists that collaborative research is a valuable approach to human understanding.

This essay focuses on one component of the larger effort in collaborative research—collaborative ethnography, defined here as the collaboration of researchers and subjects in the production of ethnographic texts, both fieldwork and writing.[1] In previous essays I have sought to illustrate that while ethnographic fieldwork is, by definition, collaborative, collaborative ethnography extends fieldwork collaboration more systematically into the writing of the actual ethnography (see Lassiter 1998, 1999, 2000, 2001, 2002, 2004a, b; Lassiter et al. 2002, 2004). In this essay, however, I wish to establish a simple, more epistemological point: that collaborative ethnography, as one of many academic/applied approaches, offers us a powerful way to engage the public

with anthropology one field project, one ethnographic text at a time. In what follows I suggest, first, that contemporary collaborative ethnographic efforts stem from a well-established historical tradition of collaboratively produced texts that, as founded in part on activist trajectories, are often overlooked in our current discussions of collaborative research; second, that feminist and postmodernist efforts to recenter ethnography along dialogical lines further contextualize this historically situated collaborative practice; and, third, that the goals of collaborative ethnography (both historical and contemporary) are now powerfully converging with those of a public anthropology that pulls together academic and applied anthropology in a common effort to serve humankind more directly and more immediately.

Precedents for a Collaborative Ethnography

The co-production of ethnographic texts has a long history in anthropology. Historians of anthropology have elaborated a number of important collaborations between ethnographers and their interlocutors in the field's developmental years—collaborations that built upon and extended the collaborative requisite of fieldwork into the collaborative writing of ethnographic texts. The well-known collaborations between Franz Boas and George Hunt immediately come to mind (see, e.g., Boas and Hunt 1895; cf. Berman 1996). So do the collaborations between the French anthropologist/missionary Maurice Leenhardt and the natives of New Caledonia (see Clifford 1982), Robert Redfield and Alfonso Villa Rojas (see Redfield and Villa Rojas 1934), Sol Tax and Santiago Yach (see Tax 1979), H. Russell Bernard and Jesús Salinas Pedraza (see Bernard and Pedraza 1989), and a host of other collaborative projects carried out throughout the twentieth century (see Sanjek 1993). What I have in mind here, however, is to elaborate a stream of collaboratively inspired works that preceded and followed these better-known projects and have gone mostly unnoticed by contemporary ethnographers: those of the earliest Americanist tradition, in which American anthropologists and their Native American collaborators together coresearched and, in some cases, coconceived and cowrote their texts. While I agree with George E. Marcus and Michael M. J. Fischer (1986:viii) that American anthropology's still resonating experimental moment, which centers dialogue and collaboration in both ethnographic fieldwork and writing, "reflects a historical development in which anthropology in the United States seems to be synthesizing the three national traditions" of British, French, and American anthropology, I also agree with Regna Darnell (2001) that among the strongest precedents for collaborative practice emerged within the Americanist tradition.

The development of American anthropology was intimately tied to the study of American Indians (see, e.g., Mead and Bunzel 1960). Americanist ethnography consequently developed in close collaboration with American Indian people (cf. Bruner 1986). Indeed, one cannot consider the development of collaboration as a central component of Americanist ethnography without acknowledging how American Indian collaborators helped shape—at times as active participants—the earliest ethnographic descriptions of Native America (Liberty 1978a). It is noteworthy, then, that what is often considered as the first "true ethnography" of American Indians (Tooker 1978:19)—Lewis Henry Morgan's *League of the Ho-dé-no-sau-nee, or Iroquois* (1851)—makes explicit reference to the collaboration that engendered its writing. Its dedication reads: "To Hä-sa-no-an'-da (Ely S. Parker), A Seneca Indian, This Work, The Materials of Which Are the Fruit of our Joint Researches, Is Inscribed: In Acknowledgment of the Obligations, and in Testimony of the Friendship of the Author." Morgan (1851: xi) echoes this dedication in the book's preface, writing that Parker's "intelligence, and accurate knowledge of the institutions of his forefathers, have made his friendly services a peculiar privilege."

As Morgan so clearly acknowledged, the *League* would have taken a very different form without Ely Parker's active participation. A lawyer by training, Morgan originally became interested in the Iroquois because of his involvement in the Grand Order of the Iroquois, a secret fraternal order organized by him and friends in Aurora, New York, and patterned after Iroquois cultural and political institutions. In an effort to found the order on rationalism and authenticity (in contrast to earlier men's organizations, such as the American Tammany societies, which were based more on fictional representations of Indians), Morgan turned to scientific investigation of Native American peoples. Collaboration with Indians was crucial for authenticating this new scientific investigation and, in turn, the Order (see Deloria 1998: 71–94). When Morgan met Ely Parker in a bookstore in the early 1840s, he immediately took the opportunity to involve Parker in his scientific work, and Parker enthusiastically agreed (Tooker 1978).

Parker initially facilitated Morgan's access to Iroquois leaders, serving as an interpreter, but over time he came to provide firsthand knowledge and help organize interviews on the Tonawanda Reservation (see Fenton 1962). As Elisabeth Tooker (1978:23) writes:

> All the evidence indicates this was a collaboration. . . . that Parker was not only Morgan's interpreter but also provided him with information as he knew it and, when he did not know it, inquired of knowledgeable people at Tonawanda, a task made relatively easy for him by his personal and family connections. . . . The collaboration proved advantageous to both; Morgan not only called on Parker for information and other aid, asking him to attend meetings of the Order, but also Parker called on Morgan for help, such as asking him to come to Washington in the spring of 1846 to testify on Iroquois political organization.

While Parker eventually went on to join the Union Army, serve as General Ulysses S. Grant's military sec-

retary, and become Grant's Commissioner of Indian Affairs (Tooker 1978), his collaboration with Morgan served as a significant impetus for Morgan's subsequent writings on American Indians in general (see, e.g., Morgan 1871) and on the Iroquois in particular (see, e.g., Morgan 1858), in which he continued to "encourage a kinder feeling towards the Indian, founded upon a truer knowledge of his civil and domestic institutions, and of his capabilities for future elevation" (Morgan 1851:ix).

Morgan went on to focus on broader theories of kinship and evolution which, of course, had an enormous impact on the development of American anthropology (cf. Tooker 1992), but his first ethnography should not be underestimated. Not only was it characterized as "the best general book" on the Iroquois long after its first publication (see Fenton 1962:v) but it helped shape the way Americanist ethnographers—in direct contact and collaboration with Indians—approached the salvaging of Indian cultures as a scientific undertaking (cf. Hallowell 2002 [1960]:38–43). Major John Wesley Powell, the founder of the Bureau of American Ethnology, later wrote that Morgan's *League* was "the first scientific account of an Indian tribe ever given to the world" (1880:115), and his appreciation of it was more than just a passing thing. Morgan deeply influenced Powell's thinking; indeed, his writings (esp. Morgan 1877) helped to establish the Bureau of American Ethnology (BAE) within an evolutionary framework (see Baker 1998:38–45; Hinsley 1981: 113–43). His collaborative approach with Parker in *League*, however, influenced the way Americanist ethnographers went about describing (and salvaging) Native America. With the bureau's establishment, American ethnography as a scientific genre was systematized, and so was collaboration with Native American informants. Consequently, the direct involvement of these native collaborators—many of whom also became BAE ethnologists—powerfully authorized the work undertaken by the bureau in many of the same ways that authenticated the *League* and Morgan's Grand Order of the Iroquois.

But the story is more complicated than this (see Darnell 1974, 1998; Hinsley 1981; Deloria 1998:90–94). Although Morgan's and eventually the bureau's brand of salvage ethnography placed American Indians firmly in the past (by describing what were perceived to be unchanging beliefs and practices that American civilization would eventually subsume), involving Native American peoples in the construction of ethnography also meant, contradictorily, often engaging with Indian political struggles in the present. As Philip J. Deloria (1998:84) writes about Morgan's collaboration with Ely and other Parker family members:

> The relationships that developed between New Confederacy [a.k.a. Grand Order of the Iroquois] members and the Parkers and other Seneca people took the group far from the distant abstractions of fictionalized Indianness and into the free-for-all of Indian-American political conflict. Ely Parker had traveled to Albany to continue a long struggle being waged by the Tonawanda Seneca, who, under the terms of

an imposed treaty, were scheduled to abandon their reservation by 1846. The New Confederacy's subsequent involvement with the Senecas foreshadowed what has since become something of an anthropological tradition: political activism on behalf of the native peoples who serve as the objects of study.

Such activist tendencies, spawned by direct collaboration with native interlocutors, did indeed foreshadow an anthropological tradition, one that extended right into the Bureau of American Ethnology.

While Powell originally established the bureau to inform and influence Indian policy and arguably it never really did so, in practice the activism of its individual ethnologists often contradicted what came to be its official apolitical party line (Hinsley 1981). James Mooney, for instance, "caused Powell constant headaches" (Hinsley 1976:23). In his *Ghost Dance Religion* (1896) he helped to fuel growing public outrage over the Wounded Knee Massacre of 1890, going so far as to suggest, to the chagrin of his superiors, that the religious beliefs and practices for which Indians had been murdered were in the same league as Christian beliefs and practices (Hinsley 1976:23–25). Mooney did not stop there, however. Throughout his career as a BAE ethnologist he defended the rights of Indian people, often at great cost to his own career (cf. Gleach 2002). When he helped the Kiowas, Comanches, and Kiowa Apaches officially organize their peyote religion as the Native American Church, for example, he was barred from working on the Kiowa-Comanche-Apache reservation ever again (see Moses 1984: 206–21). This "political activism on behalf of the native peoples who serve as the objects of study" (Deloria 1998: 84) was a direct product of Mooney's ethnographic work on the Kiowas, *Calendar History of the Kiowa Indians* (1898).

One can hardly believe that Mooney would have gone to such lengths, putting his own career in jeopardy, without a deep personal commitment developed while systematically encountering, living among, and engaging with Indian people. The same could be said for many other BAE ethnologists, such as Frank Hamilton Cushing, J. Owen Dorsey, Alice Fletcher, Francis La Flesche, and James R. Walker (Lindberg 2002). Long before Bronislaw Malinowski insisted that anthropologists move "off the verandah" and into the everyday lives of the natives (see Stocking 1983), many BAE ethnologists had moved into Native communities and were participating in people's everyday lives, doing fieldwork in collaboration with Indian informants, and, in some cases, following in the tradition of Morgan, acting on behalf of their "subjects." Although political activism was off the beaten path of mainstream BAE practice (cf. Darnell 1998), its presence calls attention to a deeper and more complex ethnographic collaboration between ethnographers and native informants that, though vital, was often veiled in many early BAE texts.

The texts produced by the Bureau of American Ethnology between 1879 (when it was founded as a branch of the Smithsonian Institution) and 1964 (when it was

terminated) (Judd 1967) represent perhaps the largest single corpus of literature ever produced on Native North Americans (see Smithsonian Institution 1971). For the most part, these works employed the authoritative, normative style that was the writing tradition of the day, and their aim was the objective documentation of Native American beliefs and practices.

Though limited in some ways, the work is immensely expansive and impressive—overwhelming, actually—and unmatched in its depth and coverage. The unwavering commitment of BAE ethnologists to their craft (and, in many cases, to their Indian subjects) is immediately apparent. So, too, is the role of Indian collaborators in constructing these texts: the close work of BAE ethnologists and American Indians is evidenced by many ethnologists' references to native collaborators. It is often unclear, however, to what extent these Native American informants provided direct assistance or, indeed, contributed their own writings.

Some ethnologists, however, delivered more clearly collaborative ethnographies. Chief among them was Franz Boas, of course, who worked with Hunt and other collaborators in several other non-BAE texts as well. Also prominent were the efforts of the BAE ethnologist Alice Cunningham Fletcher, who, like Boas, explicitly acknowledged the role of her assistants (see, e.g., Fletcher 1904). Fletcher is perhaps best known for her collaborative efforts with Francis La Flesche, with whom she wrote *The Omaha Tribe* (Fletcher and La Flesche 1911). Both Fletcher and La Flesche were BAE ethnologists when their manuscript appeared, but their relationship had originally begun with La Flesche serving as Fletcher's field assistant and interpreter. As their work together intensified, so did their relationship: La Flesche began referring to Fletcher as "mother," and by the early 1890s she had adopted him as her legal son (see Liberty 1976, 1978b; cf. Lurie 1966, Mark 1988). The professional collaboration that would eventually produce *The Omaha Tribe* began when, as Ridington and Hastings (1997: 17–18) write,

> it became obvious, first to him and then to her, that [La Flesche] was a partner rather than simply a son, an interpreter, or an informant. The matter came to a head with her plans to publish a substantial paper entitled "A Study of Omaha Indian Music." Francis, himself an accomplished Omaha singer and the source of much of her information, managed to convince his adopted mother that his part in the work should be recognized in print. . . . By the time of their most comprehensive publication, *The Omaha Tribe*, in 1911, Francis had achieved the status of coauthor.

Significantly, La Flesche's negotiation of his role in the project was as much a matter of the native interlocutor's demanding agency as about the anthropologist's giving over control. La Flesche's insistence on being acknowledged was in fact to foreshadow native consultants' insistence that anthropologists and others include their names, voices, and contributions in texts about them, a demand that gathered power throughout the twentieth century.

Although La Flesche and Fletcher's coauthored manuscript was an exceptional case (Liberty 1976), it marked the growing involvement of Native American ethnologists in the Bureau of American Ethnology and other museum-based institutions. To be sure, several American Indian ethnologists had been collaborating with the bureau and other institutions for many years prior to the appearance of Fletcher and La Flesche's book and the subsequent appearance of La Flesche's own reports (see, e.g., La Flesche 1921). John N. B. Hewitt, for example, a "mixed-blood" Tuscarora Indian who worked with the BAE ethnologist Erminnie Smith, took over Smith's work after her death in 1886 (Darnell 1998:70–71). Like La Flesche, Hewitt contributed several of his own reports (see, e.g., Hewitt 1903, 1928).

To put it simply, the collaborations between Native American ethnologists and other ethnologists, in particular, and with institutions like the Bureau of American Ethnology, in general, are significant to appreciating the role of collaboration in the early development of Americanist ethnography, but they do not tell the whole story. Indeed, focusing solely on ethnologist-assistant relationships or white-Indian coauthored texts underestimates the actual role of collaboration in these early institutions. As Darnell (1998:80–85) points out, collaboration in the bureau was a complicated, multifaceted affair. Many other people—such as missionaries, former fur traders, and diplomats—also had intimate knowledge of Indian languages and cultures, and they also collaborated with the bureau to produce its reports, bulletins, and other manuscripts. One need only recall the well-known collaborations between Franz Boas and James Teit, a Scotsman who had an extensive knowledge of several Northwest tribes (see, e.g., Teit 1930). Native American ethnologists like Hewitt and La Flesche, it turns out, were just some of the many kinds of semiprofessionals who had close associations with American Indian peoples, knew native languages, and contributed their unique skills and knowledge to the bureau's goal of collecting Native American beliefs and practices before they presumably disappeared forever.

This is not to diminish the role of Native Americans in the bureau or other museum-based institutions—only to suggest that, while clearly seeking to elaborate more fully a "native point of view" through the use of knowledgeable collaborators, the bureau was not interested in using these collaborations for critiquing Western society and culture (although many individual ethnologists, like Mooney, certainly did), much less negotiating ethnography's ultimate goals. This would come later as anthropologists became much more intimately and critically aware of the colonially derived separation between those doing the representing (the Self) and those providing the firsthand data for these representations (the Other)—a separation that became all the more pronounced as anthropology became a professional discipline more firmly situated in the academy (cf. Fabian

1983). Whereas this critique became prominent along with a more explicitly expressed critical anthropology in the 1970s and 1980s, it had its beginnings much sooner: with the emergence of American Indian life histories under the influence of Paul Radin.

Beginning with his 1913 *Journal of American Folklore* essay "Personal Reminiscences of a Winnebago Indian," continuing with the publication in 1920 of "Autobiography of a Winnebago Indian," and culminating with the appearance of *Crashing Thunder* in 1926 (1913, 1920, 1926), Radin's earliest experiments with Winnebago biography marked "the beginning of truly rigorous work in the field of biography by professional anthropologists" (Langness 1965:7). Indeed, to this day Radin's work with what came to be generally known as "life history" is still widely regarded as among the most significant efforts to merge individual experience with ethnographic descriptions of culture (Darnell 2001:137–70).

Radin's fieldwork among the Winnebago was carried out intermittently between 1908 and 1913 (see Du Bois 1960), and in 1911 and 1912 he did ethnography under the auspices of the Bureau of American Ethnology. In the bureau's twenty-seventh annual report (published in 1923) he supplemented his exhaustive description of the Winnebago tribe with numerous first-person narratives (see Radin 1923). Two of the collaborators who provided their first-person accounts were Radin's "principal informants" (Radin 1926:xxi), Jasper Blowsnake and his younger brother Sam Blowsnake, both of whom he relied on considerably to construct his subsequent Winnebago (auto)biographies (Krupat 1983). Radin used Jasper Blowsnake's autobiography in "Personal Reminiscences of a Winnebago Indian," in which, being a student of Boas, he followed the standard Boasian procedure for representing native texts: Jasper Blowsnake's description of his life, written in his native language, was presented along with the English translation. In "Autobiography of a Winnebago Indian," based on Sam Blowsnake's autobiography, Radin deviated from his previous approach: he did not include a native text in Winnebago (but did include 351 notes in this short, 91-page account). In *Crashing Thunder*, Radin went even farther, expanding "Autobiography of a Winnebago Indian" to make the text more artfully literary and readable (Krupat 1983).

While Radin's approach to life history was straightforward—to describe a "life in relation to the social group in which he [the subject] had grown up" (1920:2)—his appreciation for and representation of life history as text was not as simple. He no doubt recognized the problems and limitations of the conventional approach to native texts (Vidich 1966)—that language and story were not in themselves "facts" but a "textualization" of facts which, of course, could yield multiple and divergent interpretations (Krupat 1983:xi–xv). In "Personal Reminiscences of a Winnebago Indian," for example, he (1913:294) briefly warned of the problems inherent in constructing and translating a life history, and in his *Method and Theory of Ethnology* (1933:11–12) he elaborated on these problems:

In science we stand beside or, if you will, above the facts. We are not a part of them. But we are a part of the cultural facts we are describing in a very real way. The moment we stand beside or above them, we do them injury; we transvaluate and make them facts of another order. In short, they are reduced to facts of the physical world. The disadvantages attendant upon being an integral part of the phenomenon we are describing must seem a fatal defect to the scientific mind. Unquestionably it is. But it is inherent in cultural phenomena and nothing can very well be done about it. This defect is not being corrected by treating them as physical facts. Objectivity, in the sense in which it exists in the natural and physical sciences, is impossible for culture history, except, perhaps, in the domain of material culture. For culture, the ideal of permanency and durability toward which a description of the physical world inevitably strives is unattainable. The more culture historians and ethnologists attempt it, the more suspect their descriptions become. There are too many *imponderabilia*, and these are too intimately connected with its very life blood.

This position was critical to Radin's approach to representing individual experience through biography (cf. Diamond 1981). Although anthropologists such as Boas and Malinowski had relied heavily on individual collaborators to elaborate the "facts" of culture, Radin argued that these individual collaborators and their experiences were largely absent from ethnographic accounts because of ethnologists' overzealous attempts to quantify and typify culture. Individual experience was too messy for them, argued Radin, (1933:42), too subjective, and as a consequence "the method of describing a culture without any reference to the individual except in so far as he is an expression of rigidly defined cultural forms manifestly produces a distorted picture."

Just as Radin's *Crashing Thunder* had marked a significant turning point in the use of life history, his argument for more firmly situating individual experience at the center of ethnographic inquiry marked an extremely significant turning point in Americanist ethnography itself. What it required was a more sustained focus on collaboration with native interlocutors, nonanthropologists with differing worldviews and perspectives who had their own unique experiences to present in an ethnography that was to be clearly separated from the "personality" of the ethnologist (see Radin 1933: 87–129). Arguably, the Americanist focus on presenting native texts in their original form did just this. Broadly defined, many of these texts consisted, for example, of the myths, stories, and legends relayed by native informants; more narrowly defined, many of them were written by native assistants in their native language and translated, transcribed, and/or edited by the ethnologist. Franz Boas, of course, became the most widely recognized proponent of this latter approach, with the Boas-Hunt collaborations representing its quintessential illustration. As Briggs and Bauman (1999) point out, in collaborations

such as this one the subjects of inquiry were largely chosen by the ethnologist. Although Radin (1933:114) admitted complicity, his focus on the life experience of his collaborators helped to usher in an innovative way of conceptualizing the structure of ethnography as based more on the *informant's* choices of story, narrative device, style, and flow (see Darnell 2001:137–70).

Within American Indian studies, Radin's focus on individual experience *in* culture set the stage for subsequent life histories that shifted away from the psychology of the individual—as articulated by Edward Sapir (1934)—and toward the relativistic representation of experience. Radin's approach also set the stage for more intensive long-term collaborations between ethnographers and native consultants that are perhaps unmatched in any other subfield of ethnographic inquiry (see Darnell 2001:105–70).

While the collaborative model for constructing life histories had a profound effect on the production of (auto)biography, it also fostered a more general collaborative approach to Native American ethnography. As Darnell (2001:208) writes, "The dialogic potentials of life history discourse are considerable, although the genres of ethnographic production that develop them have moved, in practice, beyond life history in the narrow sense. Contemporary Americanists reflect teachings from multiple Native specialists, emphasizing sharing and transmitting of knowledge rather than narrative authority jealously guarded by the anthropologist." American Indian studies are therefore replete with collaboratively conceived and dialogically informed ethnographic projects (not always coauthored) such as the use of Yuchi focus groups to construct community-based texts (Jackson 2003), the bringing together of museum resources to document a local chapter of the Native American Church at the request of Osage peyotists (Swan 2002), the use of a community-based editorial board to construct a locally centered text on the Bay Area American Indian community (Lobo et al. 2002), the use of collaborative methodologies and textual strategies by an anthropologist and his Kiowa relatives (Palmer 2003), and even the collaboration of a university press with the Salish Kootenai tribal government to produce a tribal oral history (Gary Dunham, personal communication, January 3, 2002).

Increasingly, of course, *all* ethnographers are finding themselves addressing issues of collaboration. Indeed, ethnographers in and outside of the Americanist tradition (e.g., the British and French) have also long dealt with these issues (see Sanjek 1993). Yet something uniquely *American* is at work in the history of collaboration in the Americanist tradition. Americans as a whole, of course, have long struggled with reconciling the differences between the ideal of equality, on the one hand, and the very real consequences of living in an inequitable society stratified along the lines of race, class, and gender, on the other (see Smedley 1993). Similarly, Americanist ethnography has more or less since its inception faced this paradox, especially as its subjects, assistants, informants, collaborators, and consultants have sought equal time and representation in the larger ethnographic project as undertaken, primarily, by middle-to-upper-class white Euro-American anthropologists (cf. Said 1979).

As American anthropologists in general turned away from American subjects and toward the British and French schools of anthropology for methodological and theoretical inspiration, such direct involvement of native collaborators became easier to sidestep. Moreover, the divisions between researchers and their subjects became all the more pronounced as anthropology became a professional academic discipline in its own right, developing and then emphasizing credentials that clearly separated the academic professional from the so-called amateur anthropologist (which included, of course, the non-university-trained American Indian). As the discipline solidified and professionalized, the writing of "objective" ethnography fell to scientifically trained and university-sited academics who tended to base their intellectual authority on the single-authored text. Indeed, collaborations between the likes of La Flesche and Fletcher would prove much more difficult to achieve in an academic setting, where to this day the single-authored text is valued over the multiple-authored text, interdisciplinary work among professionals over collaborative work between professionals and nonprofessionals, and academic credentials over experiential ones. With the academic professionalization of anthropology firmly in place, collaboration with ethnographic consultants was seemingly put on hold, only to resurface in fields such as feminist and postmodernist anthropology.

Feminist Anthropology

At least since the 1970s, women's studies scholars have contended that feminism linked with conventional social science research methods can yield more humane and dialogic accounts that more fully—and more collaboratively—represent the diversity of experience (see, e.g., Bowles and Duelli Klein 1983; cf. Westcott 1979). The feminist scholar Renate Duelli Klein (1983:94–95), for example, argued that

> whenever possible, feminist methodology should allow for such intersubjectivity; this will permit the researcher constantly to compare her work with her own experience as a woman and a scientist and to share it with the researched, who then will add their opinions to the research, which in turn might again change it.
>
> A methodology that allows for women studying women in an interactive process—without the artificial object/subject split between researcher and researched (which is by definition inherent in any approach to knowledge that praises its "neutrality" and "objectivity") will end the exploitation of women as research objects.

Many feminists agreed. "Our work," wrote Barbara Du Bois (1983:110), "needs to generate words, concepts, that

refer to, that spring from, that are firmly and richly grounded in the actual experiencing of women. And this demands *methods* of inquiry that open up our seeing and our thinking, our conceptual frameworks, to new perceptions that actually derive from women's experience."

Some feminist ethnographers have argued, however, that a feminist methodology might be more problematic than advantageous to the agendas of a larger, critical feminist theory. In "Can There Be a Feminist Ethnography?" Judith Stacey (1988:22) argued that although "the ethnographic method . . . appears ideally suited to feminist research [in that it] . . . draws on those resources of empathy, connection, and concern that many feminists consider to be women's special strengths," she ultimately questioned "whether the appearance of greater respect for and equality with research subjects in the ethnographic approach masks a deeper, more dangerous form of exploitation." She pointed to the contradictions between critical feminism, on the one hand, and collaborative ethnographic approaches, on the other—especially when the researcher's feminism conflicted with the agendas of her subjects. She reported, for example, that one of her informants, a fundamentalist Christian, had asked her to not reveal the secret lesbian relationship about which she had spoken to her. This intimate knowledge not only highlighted the potential for exploitation (if, for instance, Stacey had chosen to write about this "ethnographic fact" anyway) but also drew attention to the differences between Stacey's goals as a critical feminist and that of her interlocutor, who presumably accepted the larger society's disparaging view of homosexuals. "Principles of respect for research subjects and for a collaborative, egalitarian research relationship," wrote Stacey (1988:24), "would suggest compliance, but this forces me to collude with the homophobic silencing of lesbian experience, as well as to consciously distort what I consider a crucial component of the ethnographic 'truth' in my study. Whatever we decide, my ethnography will betray a feminist principle." These moral dilemmas notwithstanding, in the end Stacey was generally hopeful about the attainment of a feminist ethnography. Following James Clifford's assertion that "ethnographic truths are . . . inherently partial" (Clifford 1986:7), she concluded (p. 26) that "while there cannot be a fully feminist ethnography, there can be (indeed there are) ethnographies that are partially feminist, accounts of culture enhanced by the application of feminist perspectives. . . . I believe the potential benefits of 'partially' feminist ethnography seem worth the serious moral costs involved."

Ensuing feminist, reciprocal ethnographies—like those written by Elaine Lawless (1993), in which the researcher's feminism and the experience of the researched are negotiated and presented within the pages of the same text (even when they differ)—would in part resolve the disparities noted by Stacey and consequently inch a "partially feminist ethnography" a bit closer to a "fully feminist ethnography." But the potentials for a feminist ethnography revisited a larger problem in the discipline: contemporary feminist approaches that shared ethnography's goals with "subjects" placed a feminist ethnography in an inferior position relative to emergent "more professional" ethnographic experiments (cf. Strathern 1987). Simply put, it wasn't "objective" enough. Although an emergent postmodernist anthropology was also experimenting with ethnographic forms, struggling with issues of power and authority, and challenging notions of objectivity (as in Clifford and Marcus 1986), the advances in feminist ethnography along these lines were largely dismissed and ignored by its—mostly male—proponents (Behar 1995). Lila Abu-Lughod has suggested, in her own "Can There Be a Feminist Ethnography?" (1990a), that feminist ethnographers stood to lose too much in an emerging critical anthropology dominated by a "hyper-professionalism that is more exclusive than that of ordinary anthropology" (p. 19) and one that continued to reify a now obscured presumption of objective distance maintained by the traditional "rhetoric of social science" (p. 18). If a feminist ethnography challenged conventional ethnography by emphasizing everyday experience and everyday language (which engendered a presumably more "simplified" and "less rigorous" analysis via its identification and collaboration with "unprofessional" collaborators), then a more "professional," "theoretical," and "rigorous" ethnography challenged conventional ethnography by foregrounding a rarefied, jargonistic discourse (which presumed to engender a more "complex" analysis undertaken without the constraints of reciprocal responses from consultants). Even though, in actuality, the rigor of feminist ethnography revolves around the very complex negotiation of visions between ethnographers and interlocutors, collaborative and reciprocal approaches were once again, within the larger field (social science in general, anthropology in particular), caught not only within the still resonating divisions between professional and unprofessional work but also within the still very powerful if now obscured divisions between "objective" and "subjective," between "theoretical" and "descriptive," and between "masculine" and "feminine." As a consequence, Abu-Lughod (1990a:19) argued, contemporary feminist anthropologists may not have "pushed as hard as they might on epistemological issues nor experimented much with form . . . perhaps because," within an anthropological milieu in which the cross-cultural findings of a feminist anthropology (that is, of gender) were still relatively new, "they preferred to establish their credibility, gain acceptance, and further their intellectual and political aims."

Whether there can be a truly feminist ethnography or not, Abu-Lughod and other feminist scholars in and outside of anthropology (see, e.g., Bell 1993, Reinharz 1992, Stack 1993, Visweswaran 1988, Wolf 1992) suggest that a feminist ethnography can nevertheless offer anthropology a powerful reconceptualization of the goals of ethnography itself. In short, feminist ethnography is now broadly defined as an experimental ethnography that questions the positionality and authority of the ethnographic process (from fieldwork to text), foregrounds and simultaneously seeks to dissolve the power relationship between ethnographer and "subject," and, perhaps most

important, contextualizes ethnographic writing within a broader consciousness of the historical trajectories of feminist texts (rather than in terms of the response to the "classic" modernist male-centered ethnographic texts from which postmodernism arguably springs) (Visweswaran 1992, 1997). Feminist ethnography embraces a more conscious politics of representation, but in contrast to many dialogic approaches it also seeks to "expose the unequal distribution of power that has subordinated women in most if not all cultures and [to] discover ways of dismantling hierarchies of domination" (Wolf 1992: 119).

Feminist ethnography also offers anthropology an ethnography written by ethnographers who, as women whose knowledge is situated vis-à-vis their male counterparts (cf. Haraway 1988), are *already* Other (see Mascia-Lees, Sharpe, and Cohen 1989). A feminist ethnography, which unapologetically upholds "a nonpositivist perspective, rebuilding the social sciences and producing new concepts concerning women" (Reinharz 1992:46), is a research process whereby "others" study "others" and, as in studies by native anthropologists of their own communities (see Abu-Lughod 1990*b*; cf. Limón 1990), openly struggle in both fieldwork and ethnographic texts with issues of sameness (where both researcher and researched are women who share similar experiences with systems of domination) and difference (where class and race, for example, play a prominent role in interpretations of the complexities of gender) (cf. Moore 1988). "By working with the assumptions of difference in sameness," writes Abu-Lughod (1990*a*:25–26, 27), "of a self that participates in multiple identifications, and an other that is also partially the self, we might be moving beyond the impasse of the fixed self/other or subject/object divide that so disturbs the new ethnographers. . . . [Thus] the creation of a self through opposition to an other is blocked [in feminist ethnography], and therefore both the multiplicity of the self, and the multiple, overlapping, and interacting qualities of other cannot be ignored."[2]

2. Several women ethnographers had sought to do just this before. Perhaps the best-known example is Marjorie Shostak's *Nisa: The Life and Words of a !Kung Woman* (1981), in which Shostak frames her ethnography in terms of this interaction between sameness and difference (pp. 5–6, 7): "My initial field trip took place at a time when traditional values concerning marriage and sexuality were being questioned in my own culture. The Women's Movement had just begun to gain momentum, urging re-examination of the roles Western women had traditionally assumed. I hoped the field trip might help me to clarify some of the issues the Movement had raised. !Kung women might be able to offer some answers; after all, they provided most of their families' food, yet cared for their children and were lifelong wives as well. . . . I presented myself to them pretty much as I saw myself at the time: a girl-woman, recently married, struggling with the issues of love, marriage, sexuality, work, and identity—basically, with what womanhood meant to me. I asked the !Kung women what being a woman meant to them and what events had been important in their lives." But the experience of Shostak and that of !Kung women also diverged in very significant ways. For example, "their culture, unlike ours, was not being continuously disrupted by social and political factions telling them first that women were one way, then another." In the end, her ethnography was meant to illustrate the diversity of women's experience (through an intimate portrayal of Nisa's life),

Simply put, "feminist ethnography is writing carried out by a woman author who is always aware that she is a woman writing" (Behar 2003:40).

Conceptualized in this way, feminist ethnography has for the most part been associated with women ethnographers and the reciprocal and collaborative relationships with women interlocutors that have engendered its approach. Indeed, as feminist ethnography developed in response to patriarchal research and writing methods that either ignored women or dismissed feminist theory and methods altogether as irrelevant to larger discussions about ethnography, a feminist approach has more often than not implied that only "ethnography in the hands of feminists . . . renders it feminist" (Reinharz 1992:48). But, given its "gendered" marginalization (Abu-Lughod 1990*a, b*) and given that many feminist ethnographers question whether feminist theory and anthropology can establish more common ground (Gordon 1993, Strathern 1987), feminist ethnography actually has more similarities than differences with the dialogic and collaborative ethnographic experiments of the past several decades (and, indeed, with Americanist life-history accounts) (Caplan 1998, Visweswaran 1992). In particular, feminist ethnography's central focus on voice, power, and representation is converging with the central focus of ethnography in postmodernist anthropology (cf. di Leonardo 1991).

Postmodernist Anthropology

A more general critique of anthropology's claims to an ability to handle the complexities of a postcolonial and postindustrial world "authoritatively" and "objectively" converged in the 1980s with the emergence of a postmodernist anthropology. While the modern development of anthropology in the first three-quarters of the twentieth century had advanced the Western-centered project of the Enlightenment, emphasizing science and reason, authority and objectivity, positivism and realism, postmodernist anthropology resituated the goals of anthropology within a more complicated multicultural world (outside the divide between the West and the Rest), instead emphasizing power and voice, subjectivity and dialogue, complexity and critique (cf. Clifford 1986, 1988; Marcus 1992, 1999; Tyler 1987). In ethnography, specifically, the emergence of postmodernism marked a confluence of previous ethnographic approaches—such as that embraced by cognitive, symbolic, and humanistic anthropology—that had for some time variously struggled and experimented with the limitations of the ethnographic craft in representing the lived complexities of culture and experience from the "native point of view" (Marcus and Fischer 1986).

and, to a lesser extent, to present experiential alternatives to women's statuses and roles in the Western world (see Marcus and Fischer 1986:58–59 and Pratt 1986:42–46 for a more critical discussion). More recent examples that adopt this approach (perhaps more fully than Shostak) include Abu-Lughod (1993), Behar (1993), and Brown (1991).

Marcus and Fischer (1986:17–44) argue that interpretive anthropology, in particular, provided the context for addressing this so-called crisis of representation. With the recognition of a more complex "field" (in which "untouched" cultures no longer existed, anthropologist and their interlocutors were more and more politically, socially, economically, and intellectually interconnected in a global political economy, and new and shifting field sites demanded new research strategies), interpretive ethnographers revitalized experimentation with ethnographic forms that might bring "anthropology forcefully into line with its twentieth-century promises of authentically representing cultural differences" and respond "to world and intellectual conditions quite different from those in which [ethnography] became a particular kind of genre" (pp. 42–43).

While there were and continue to be many types of ethnographic experiments (variously "conveying other cultural experience" and/or taking into account "world historical political economy" [Marcus and Fischer 1986: 45–110]), many interpretive anthropologists have focused on dialogue as a key metaphor (rather than the textual metaphor established by Clifford Geertz [1973]) for reconceptualizing the ethnographic process. "Dialogue has become the imagery for expressing the way anthropologists (and by extension, their readers) must engage in an active communicative process with another culture," wrote Marcus and Fischer (1986:30) about this shift in focus. "It is a two-way and two-dimensional exchange, interpretive processes being necessary both for communication internally within a cultural system and externally between systems of meaning."

While many interpretive anthropologists engaged the dialogic metaphor more or less symbolically, some ethnographers took the metaphor more literally, looking to the dialogic processes of fieldwork itself to construct ethnographies that were more representative of the collaborative production of knowledge between anthropologist(s) and informant(s)—that is, "to present multiple voices within a text, and to encourage readings from diverse perspectives" (Marcus and Fischer 1986:68). Kevin Dwyer's *Moroccan Dialogues* (1987) is perhaps the best-known example. Dwyer's approach is similar to narrative ethnography in its focus on shared experience (see Tedlock 1991), but he narrows the field of vision even more, focusing on and problematizing the dialogic emergence of culture throughout. His purpose in doing so is to challenge the authority of the single-voiced monograph and, perhaps more important, to show how the complexities of Others are often lost in the textual world of paragraphs and sentences. "The anthropologist who encounters people from other societies is not merely observing them or attempting to record their behavior," wrote Dwyer (1987: xviii); "both he and the people he confronts, and the societal interests that each represents, are engaging each other *creatively*, producing the *new* phenomenon of Self and Other becoming interdependent, of Self and Other sometimes challenging, sometimes accommodating one another." Recognizing, of course, that presenting Moroccan dialogues in text and in English is itself an act of

distanced interpretation, a fiction, Dwyer challenged the reader to question the content of the ethnographic text and, more important, its goals and purposes (p. xix):

> If a faithful record, a full communication, of the experience is impossible, this is no excuse to reduce the effort to preserve in the text, and to convey to others, what one believes to be crucial in that experience. The effectiveness of this book should be judged, then, not in the light of a necessarily mistaken criterion of fidelity to experience, but in terms of the significance of taking certain aspects, rather than others, as essential, and the book's success in displaying them: here, the structured inequality and interdependence of Self and Other, the inevitable link between the individual's action and his or her own society's interests, and the vulnerability and integrity of the Self and the Other.

Dwyer's version of dialogic ethnography called for close scrutiny of the nature of cross-cultural understanding and appreciation of the very real challenges faced by ethnographers when they seek to forge experience as text. Simply put, Dwyer concentrated on process.

Other classic dialogic works that variously took up these kinds of issues include Vincent Crapanzano's *Tuhami* (1980), Jean Briggs's *Never in Anger* (1970), and Jeanne Favret-Saada's *Deadly Worlds* (1980) (cf. Marcus and Fischer 1986:69–71). While many of these ethnographies focused on the collaborative production of knowledge and directly challenged the goals of ethnography by resituating its power and authority in the dialogic process, writing dialogic ethnography did not necessarily mean engagement in collaborative practice with interlocutors to produce collaboratively conceived texts (cf. Tyler 1987). Many interpretive anthropologists embraced the metaphor of dialogue in their fieldwork and writing, but only a few ethnographers took the metaphor to this next logical step. Of course, several ethnographers had continued in the collaborative tradition of Hunt and Boas or Fletcher and La Flesche, coauthoring ethnographic texts with key informants/consultants (see, e.g., Bahr et al. 1974, Majnep and Bulmer 1977), but others were going a critical step farther by seeking to include reactions from their consultants in their ethnographic texts.[3] Examples include—in addition to those of the Americanist and feminist tradition already mentioned— Douglas E. Foley and company's *From Peones to Politicos* (1988), an ethnography of ethnic relations between Anglos and Mexicanos in a South Texas town, which includes native responses to the text; John C. Messenger's *Inis Beag Revisited* (1983), an ethnography focusing on a shipwreck off the coast of an island west of Ireland,

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3. I consider this step critical because, as Radin (1927, 1933) pointed out, engaging in coauthored projects does not necessarily mean engagement with diverging worldviews, especially when coauthors move to write conventional, authoritative, academically positioned texts. By including consultant commentary these ethnographers problematized audience in a different way by directly challenging (at the very least implicitly) the authority of the ethnographer to speak solely for the Other (see Clifford 1983).

a folk song that Messenger composed about the shipwreck, and the islanders' mixed reactions to both the song and his controversial ethnographic texts; and James L. Peacock's *Purifying the Faith* (1978), an account—part realist description, part symbolic analysis, part narrative ethnography—of the history, beliefs, and practices of a movement to reform Islam in Indonesia that includes commentary from one of Peacock's collaborators (presented as a preface) (cf. Lassiter 2001).

Although ethnographies that considered responses from the "natives" (even negative ones, as is the case with Messenger's work) were exceptions to the rule and involved different views of collaboration, they foreshadowed a focus on a trope of collaboration that would emerge full-blown in critical ethnography. This ethnography was marked by a number of important texts, including James Clifford and George E. Marcus's *Writing Culture* (1986), George E. Marcus and Michael M. J. Fischer's *Anthropology as Cultural Critique* (1986), James Clifford's *The Predicament of Culture* (1988), and Renato Rosaldo's *Culture and Truth* (1989)—all of which argued for a more ethical, humanistic, interpretive, intersubjective, dialogic, and experimental ethnography. Although many social scientists have taken these writers to task, the influence of their texts on the practice of ethnography today is unmistakable: few ethnographers embark on their projects without bearing these issues in mind (cf. Marcus 1994). Ethnography today involves a critical and reflexive process whereby ethnographers and their interlocutors regularly assess not only how their collaborative work engenders the dialogic emergence of culture (and the verity of their shared understandings) but also the goals and the audiences of the ethnographic products these collaborative relationships produce. Indeed, ethnography "no longer operates under the ideal of discovering new worlds like explorers of the fifteenth century. Rather we step into a stream of already existing representations produced by journalists, prior anthropologists, historians, creative writers, and of course the subjects of study themselves" (Fischer and Marcus 1999:xx). With the gap between ethnographer and consultant ever narrowing, collaboration between ethnographers and interlocutors—both of whom exist within and partake of a larger economy of representations in varied and complicated ways—takes on a whole new meaning.

Consider, for example, Paul Rabinow's reflections on the collaborations that produced the writing of *Making PCR* (1996)—an ethnography of the polymerase chain reaction (PCR) as it developed in the biotechnology company Cetus Corporation. In his essay "American Moderns: On Sciences and Scientists" (1999), Rabinow traces his collaboration with Tom White, a former vice president of Cetus. White engaged Rabinow in the project, giving him open access to scientists at all levels in the institution. He wanted an anthropologist to elaborate the complexities of the industry at a time when popular misunderstandings about biotechnology abounded, but more than this "White hoped that the collaboration could make him more productive. He never blurred the distinction between the technical and the therapeutic,

never asked me to play a facilitator or therapeutic role. He remained attentive to possible operationalizable aspects arising from my analysis. One thing he wanted to know was how to create 'an environment for future discoveries' " (p. 328). While White's goals helped to produce the foundation for collaboration, Rabinow's goals diverged from them in that he wanted to explore the relationships between the culture of science and the culture of the humanities (which includes the sociological study of science). In short, Rabinow's and White's goals may not have been identical, but Rabinow's ethnography did indeed help to advance White's agenda to "make something different happen that he couldn't entirely control" (Rabinow 1999:332)—a collaborative venture that he hoped would produce the same kinds of innovative results (in this case a "text") for which Cetus was already well-known.

George E. Marcus (1995, 1997, 1998, 1999, 2001) has argued that such experiments—conscious of both the larger interconnected streams of representations and the changing contexts of fieldwork today—may finally be pushing anthropology toward realizing the potentials of the 1980s critique of anthropology. While anthropologists had always sought to establish rapport with their informants as a prerequisite for collecting their "ethnographic data" within the "traditional mise-en-scène of fieldwork"—that is "the intensively-focused-upon single site of ethnographic observation and participation" (Marcus 1995:96)—and had, consequently, sought to build their shared understandings collaboratively (Marcus 1997), the specific attention given to dialogue and collaboration in the 1980s critique had great potential to unveil and make explicit the challenges of collaboration often glossed over by the trope of rapport. As Marcus (2001:521) writes,

> The relational context envisioned by the 1980s critique of anthropology for the explorations of levels and kinds of reflexivity in fieldwork was the idea of collaboration and the de facto but unrecognized coauthorship of ethnography. This reenvisioning of the traditional mise-en-scène of fieldwork as being collaborative was potentially the most provocative and transformative reinterpretation of conventional ethnographic authority to which the use of the concept of rapport was wedded. . . . Rapport signaled instrumentally building a relationship with a participant or informant with the predesigned purposes of the anthropologist's inquiry in mind and without the possibility that those very purposes could be changed by the evolution of the fieldwork relationship itself, governed by building rapport. In contrast, collaboration entails joint production, but with overlapping mutual as well as differing purposes, negotiation, contestation, and uncertain outcomes.

In the same way as the dialogic metaphor came to replace the textual metaphor in interpretive anthropology, the collaborative metaphor came to replace the dialogic metaphor in critical anthropology. Given this, though, the trope of collaboration that emerged in the 1980s critique

"failed to displace the older tropes that even now continue to define the regulative ideals of fieldwork in the professional culture of anthropologists," continues Marcus (2001:521). "The idea of rapport was too established, too enmeshed within positivist rhetorical style, and thus too legitimating to be replaced. And so, its use has persisted even after the 1980s critique."

Essentially serving as another word for "rapport," then, "collaboration" indeed became cliché in the 1980s and 1990s (and remains so today), while actual experiments in collaboration like those mentioned above were forgotten (2001:522). Marcus argues, however, that the contemporary challenges of fieldwork like that described by Rabinow (1996, 1999) present a "new set of emerging norms and expectations for fieldwork for which collaboration is a key trope and transformative practice for the whole ethnographic enterprise." In an ever-evolving, shifting, and multisited field in which dichotomies such as West/East and local/global have lost their methodological utility, ethnographers are now, perhaps more than ever, having to reflect on the challenges that collaboration presents to both ethnographic fieldwork and representation (see Marcus 1998, 1999). In sum, critical ethnography can potentially move collaboration from the taken-for-granted background of ethnography to its foreground.

With this in mind, Marcus (1997) argues that collaboration explicitly uncovers the differing purposes, goals, and agendas in ethnography and makes the relationships inherent in fieldwork even more central to the writing of critical ethnography. But collaboration also advances the goal of a critical ethnography to articulate the activism and citizenship of the anthropologist as a more complete participant in the larger anthropological project of social justice and equity—which, although in many ways uniquely American (see Marcus 2001:520), now struggles to be engaged as a public, as well as an ethical, act. "Having to shift personal positions in relation to one's subjects," writes Marcus (1999:17–18), "and other active discourses in fields that overlap with one's own generates a sense of doing more than just traditional ethnography, and it provides a sense of being an activist in even the most 'apolitical' fieldworker." Indeed, as Marcus (1999: 27) continues,

> there are very clearly other constituencies for ethnographic work that break the frame of the isolated scholarly enterprise: again, circumstantial activism and the citizen anthropologist become an integral part of ethnography. Work slips in and out of para-public settings; it is answerable to one's subjects in more substantial ways than in the past; it becomes thoroughly immersed in other kinds of writing machines in the space of its operations. Knowledge can be produced in this way also, but what sort of knowledge and for whom? Being open to this radical transformation of the research process is what is at stake in acting on a crisis of representation.

In pulling ethnography, collaboration, citizenship, and activism into one stream, Marcus suggests, "being open to this radical transformation" has enormous potential to relocate ethnography within public currents that engage ethnographers and consultants in representational projects that realize a more explicit collaborative practice.

Envisioning critical ethnography as a "reflective process of choosing between conceptual alternatives and making value-laden judgments of meaning and method to challenge research, policy, and other forms of human activity" (Thomas 1993:4) closely coincides with the time-honored focus on collaboration within applied anthropology (see, e.g., Austin, 2003, LeCompte et al. 1999, Stull and Schensul 1987) and feminist anthropology, which made this connection over a decade ago. "Feminist research is more closely aligned with applied anthropology," wrote Frances E. Mascia-Lees, Patricia Sharpe, and Colleen Ballerino Cohen (1989:23–24). While their purpose was to distinguish between feminist research and an emergent experimental ethnography, today the differences between feminist ethnography and the critical ethnography that emerged from the still resonating experimental moment are less clear. Taken together, the differences between its goals and those of an applied anthropology are also less clear, but this should not be surprising. The goals of anthropology seem to be shifting as the discipline's practitioners, academic and applied, establish themselves in streams of practice more relevant, more public, and more accessible to a diversity of constituencies (cf. Basch et al. 1999, Hill and Baba 2000). Collaborative ethnography, in my view, is situated squarely at the center of this newly emergent and publicly engaged trajectory.

Intersections: Contemporary Strategies for Collaborative Ethnographic Practice

From such complex roots one would expect complex and multifaceted approaches to collaboration, and these approaches are indeed diverse. While, as Marcus points out, the notion of collaboration has long been clichéd in ethnographic practice, ethnographers have begun to outline more specific collaborative strategies for embracing the publics with which they work. In general, these strategies fall into six (not mutually exclusive) categories: (1) principal consultants as readers and editors, (2) focus groups, (3) editorial boards, (4) collaborative ethnographer/consultant teams, (5) community forums, and (6) coproduced and cowritten texts.

Many ethnographers have used principal consultants as readers and collaborative editors for a very long time—presenting their ethnographic texts, as Richard Horwitz (1996:137) describes it, "to the informant for review, inviting corrections . . . [and] edit[ing] the final draft together"—but few have actually detailed the more complex methodological processes involved in this type of collaborative ethnography, especially the negotiation of differences in interpretation (see Lassiter 2000, 2001).

Horwitz reports that his own "editing sessions have ranged from the most congenial to the most acrimonious encounters of my adult life."

Using concrete examples from his own research, Horwitz points out that collaborative reading and editing with key ethnographic consultants is a two-way process in which differences in visions, agendas, and expectations emerge that are not always easily resolved. Many collaborative ethnographers (see, e.g., Evers and Toelken 2001, Hinson 1999, Lawless 1992) have argued that collaborative reading and editing with consultants should be understood as a conversation situated within a very particular relationship and undertaken in a very particular time and place—a dialogue about rather than a final statement on any particular ethnographic topic (see Lassiter 2004b).

A second collaborative strategy is the use of focus groups (see e.g., Bernard 1995:224–29). For example, when I was writing *The Power of Kiowa Song* (Lassiter 1998), in addition to having individual Kiowa consultants read the entire text I met with small groups of Kiowa people to review individual chapters that included issues in which they were interested. Many of my consultants of course lacked the time, the energy, or the desire to invest in my project on the same level as the principal consultants, and focus groups allowed them to be involved in responding to and commenting on the text.

Similar to the use of focus groups is the use of formal editorial boards appointed by the community. This strategy is common in American Indian studies, for instance, where tribal councils (or appointed committees from the tribal council) may serve as editorial boards of sorts. The use of these boards may seem only bureaucratic, their only purpose being to rubber-stamp the final text (see Mihesuah 1993), but in some cases it has provided the opportunity for the kind of collaborative reading and editing that moves ethnographic texts in the direction of collaborative ethnography. For example, for the book *Urban Voices* (Lobo et al. 2002), an editorial committee materialized from a series of conversations about collecting the oral histories of the Bay Area American Indian community. This editorial committee, made up of the anthropologist Susan Lobo and members of the local Indian community, directed a larger project to collect and record the community's oral history as text. Evolving over several years, the committee involved hundreds in the textual and editorial process. The product was truly "a book *of* the community," the editorial board writes (Lobo et al. 2002:xix), "a reflection and documentation of the history of some of the people and significant places, events and activities that make up and shape the community."

The use of ethnographer-consultant teams is, of course, best for collaborative ethnographic projects that involve large numbers of both. For example, in a recent collaborative study of the African American community of Muncie, Indiana—the site of the famous "Middletown" studies (see Lynd and Lynd 1929)—entitled "The Other Side of Middletown" (Lassiter et al. 2004), Hurley

Goodall, Elizabeth Campbell, Michelle Natasya Johnson, and I organized teams of community advisers and student ethnographers to work on individual chapters together. As a result of ongoing conversation, the students and their advisers chose the topics of study and defined the chapters' trajectories. As the students finished chapter drafts, they took these back to their community advisers for comment and discussion. We embarked on this project with the understanding that the students' community advisers were not "representative" of the community. All of us (professors, students, and consultants) were clear that each chapter team was only engaging in a discussion *about* Muncie's African American community, a discussion framed by the contours of their particular subject areas, their particular relationships, and their particular interests in the project. Each chapter therefore had clear boundaries (like any conversation) but also clear potentials for in-depth dialogue about what it meant to live in and identify with Muncie's African American community (see Lassiter 2004a).

The students also discussed the evolving text in several larger community forums in which members of the broader Muncie African American community publicly commented on the developing student-adviser chapters. Such an approach, generally speaking, has been used for many years by applied anthropologists involved in community-based participatory action research (see e.g., Flocks and Monaghan 2003). Of course, community feedback is anything but homogeneous (cf. Lackey 2003). When, for example, the National Museum of the American Indian (NMAI) began work on a new Kiowa exhibit for its upcoming "Our Peoples" Exhibition in Washington, D.C., I assisted in organizing several community forums in the Kiowa community to identify a community-based plan for the exhibit. The NMAI was building similar collaborative museum-community relationships all over the country and asking each participating community to determine how its story would be told. As might be expected, Kiowa people differed strongly as to which stories should be told and how, and community-based discussion continued for several months as NMAI staff made return trips to gauge, through community forums, this developing conversation and to present the evolving exhibit design to the Kiowa community at large. While consensus was anything but smooth, these community forums kept the exhibit plan in the open, encouraging participation in its concept and design (Kiowa people wrote some of the exhibit panels, for example). These forums also encouraged Kiowa people to raise questions about how the NMAI would represent Kiowas to the world. Considering the number of Native communities in which NMAI staff proceeded in the same way and the scale of the eventual exhibit, this may have been among the largest collaborative-based projects in the history of museums.

The final strategy for collaboration is probably the most direct in addition to being the first employed: the creation of cowritten texts. Collaboratively written texts can take a variety of forms. Ethnographers and their interlocutors bring diverse skills and experience to any

given ethnographic project. While all collaborative ethnography is arguably *coauthored*, not all collaborative ethnography can be *cowritten* (Hinson 1999). Many cowritten texts follow the pattern of Severt Young Bear and R. D. Theisz's *Standing in the Light: A Lakota Way of Seeing* (1994), which engages the consultant as narrator and the ethnographer as compiler and translator: Theisz recorded Young Bear's narratives and organized the material on paper, maintaining Young Bear's style and delivery as best he could, and the two edited the text together as it developed. I have proceeded similarly in some of my own collaborative texts (see, e.g., Horse and Lassiter 1997), as have many other ethnographers (see, e.g., Blackman 1992, Cruikshank et al. 1990, McBeth 1996). In other coauthored collaborative texts, consultants have had an even more direct role in the writing of the text, contributing their own writings. In "The Other Side of Middletown," some consultants responded to the students' chapter drafts by presenting texts of their own, which the students then integrated into their chapters (see, e.g., Lassiter et al. 2004:186–87). Les Field describes a slightly different process in his writing of *The Grimace of Macho Ratón: Artisans, Identity, and Nation in Late-Twentieth-Century Western Nicaragua* (1999), for which his collaborators provided essays about their experience as artisans, which Field then integrated into his ethnography. But he diverges somewhat from other ethnographers' approach to coauthorship: while he does not involve his consultants in reading and editing the final manuscript, he nonetheless cautions the reader to recognize how his own "experiment in coauthorship is nothing if not fraught with contradictions and dangers" (Field 1999:20). He elaborates (pp. 20–21):

> I have not individually listed these Nicaraguans as coauthors of the book, because that would misrepresent how the book was written. I organized, edited, conceptualized, and wrote the vast majority of this book, and I claim its overall authorship. On the other hand, I have tried to navigate a blurry middle ground between treating the essays written by my friends as rich ethnographic material, with which I can support my own points, and handling them as I would a text written by another academic.

Field points out that in comparison with the situation with other collaborative projects (see, e.g., Jaffe 1996), in his project the power differential between ethnographer and consultant is extremely lopsided (p. 21). He is an American intellectual, with the power to present " 'the last word' about Nicaraguan cultural history through this book, which limits the collaborative glow with which I want to endow it."

One can only admire Field for being so honest about the nature of his collaboration, but he raises an important point: when ethnographers engage in collaborative text production with their consultants, the power that they can wield over the process must not be underestimated. Therefore, rather than merely giving lip service to collaboration, ethnographers are increasingly describ-

ing the exact nature of their collaborative approach to coauthorship when appropriate (cf. Briggs and Bauman 1999:520–22). While some collaborative projects can proceed through relatively equitable relationships, a good many cannot. Indeed, collaborative coauthorship, like all strategies in collaborative reading and editing, is not an end that can always be fully achieved.

Most collaborative ethnographers are today variously employing a combination of these strategies (see Brettell 1996). Every collaborative project is, of course, unique. Each calls for specific strategies appropriate to its individualized relationships and particular contexts. Cowriting of texts with consultants is not always possible, but to my mind collaborative reading and editing (especially that which pushes toward cointerpretation) is what ultimately makes an ethnography collaborative. When taken seriously and applied systematically rather than bureaucratically, any one or a combination of these strategies leads us from the mere representation of dialogue to its actual engagement, from one-dimensional to multidimensional collaboration, and from a clichéd collaborative ethnography to a more deliberate and explicit collaborative ethnography that more immediately engages the publics with which we work.

Conclusion

Engaging the publics with which we work in our ethnographic research and writing necessarily casts ethnography as a public act. It also, as Marcus (1999) points out, casts it as an act of citizenship and activism that has long figured prominently in various ethnographic approaches (Americanist, feminist, and postmodernist among them). The integration of collaboration into the ethnographic research process engendered broader commitments to the people with whom we work when Lewis Henry Morgan engaged Ely Parker's Iroquois community in both research and political activism, when James Mooney chose to act on behalf of Kiowa peyotists as a consequence of his Kiowa research, and when Paul Radin insisted on the Blowsnakes' right to tell their story *their* way. The same is true of more recent feminist and postmodernist conversations about the role of dialogue and collaboration in contemporary ethnography—when feminist scholars like Judith Stacey or Lila Abu-Lughod struggle to realize a feminist ethnography as one that more fully embraces other visions of gender identity, even when those visions differ from the ethnographer's, and when postmodernist ethnographers such as Paul Rabinow embrace collaborative research projects that realize their consultants' visions for developing innovative understandings of themselves, their organizations, or their communities. But it is only recently that collaborative ethnography—which encourages collaboration in both research and writing—has begun to move more systematically from the field's margins to its center.

Collaborative ethnographic practice is now converging with an engaged, public anthropology, and an important component of this emergent public anthropology is writ-

ing for publics beyond the boundaries of anthropological discourse. This may be among our biggest challenges if we want to speak more powerfully to public issues and concerns (cf. Jaarsma 2002). A collaborative ethnographic practice encourages us to address the publics with which we work. This collaborative, public act is, of course, often, though not always, locally based, but it is not therefore immaterial to a larger public anthropology discussion. At a time when anthropologists have in their sights a redefinition of anthropological activism within much more multifaceted, multisited, and shifting field contexts (Marcus 1995), we should not forgo the opportunity that most of us have for building a public anthropology from the ground up and from the center out. Collaborative ethnography is a grassroots public anthropology that must go hand and hand with the larger project outlined by Borofsky (1999), Peacock (1997), Sanday (1998), and others. Without this grassroots collaborative action, this larger public anthropology is bound to fail. Indeed, the time is ripe for us to develop the potential for writing texts that speak even more directly to our consultants' concerns—concerns that are no doubt global in their interconnectedness to a wider political economy but, like those of an activist or applied anthropology (Wulff and Fiske 1987) and those of participatory action research (Kemmis and McTaggart 2000), community-based. Collaborative ethnographic practice has the potential to pull academic and applied anthropology, feminist and postmodernist approaches, and Americanist and other anthropological traditions into the same stream, fashioning an engaged anthropology that, as Peacock (1997:14) suggests, "prob[es] the deep mysteries of the human species and the human soul" and encourages us to "press outward, mobilizing our work and ourselves to make a difference beyond the discipline and the academy."

Comments

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Too often in anthropology we profess to learn from the mistakes of our past—our profession's colonial legacy, our hierarchically situated interpretations of human evolution and experience, and so forth—but inadequately acknowledge the contributions of our predecessors. Foundational anthropologists such as Lewis Henry Morgan, James Mooney, and even Franz Boas in some cases become the "whipping boys" of the discipline, whose theoretical trajectory is regarded as a lesson in how not to conduct ethnography. Lassiter reminds us that our history *is* our strength—that our roots *are* utilitarian and publicly relevant.

If Morgan's theory of unilineal cultural evolution rings ethnocentric, it also acknowledges the mental equality of *all* human groups. In that respect, one might argue that the seeds for a critique of the concept of race were sown long before Boas entered the limelight and at a time when such thinking met potentially volatile challenges from the emergent pseudo-science of eugenics. In fact, such challenges did face many early BAE ethnologists, including James Mooney, whose efforts to engage the people with whom he worked Lassiter duly notes. What Lassiter implies but does not emphasize is that Mooney recognized the complex human side of his collaborators and saw them as far more than subjects. Indeed, while his advocacy for the rights of Western tribes is well known, his earlier work with Eastern tribes—especially in Virginia, where he urged certain indigenous groups to establish formal tribal councils at a time when the state's policy toward Indians was fueled by eugenic arguments denying their legal existence—set the precedent for his decisively activist approach to ethnology.

Franz Boas understood the complexity of human life and experience from his early fieldwork, which ultimately led to his benchmark indictment of concepts such as "race" and "eugenics." He clearly instilled this understanding in many of his students, some of whom spent part of their professional lives as second-generation BAE anthropologists. Frank Speck, for instance, had deep ties with the indigenous communities with which he worked and is still fondly remembered by some. In fact, he took Mooney's advocacy of Virginia Indian rights to such extremes that his first book on the Powhatan Indians was banned in the state for several years.

Lassiter's focus on Americanist anthropology is important precisely for the reasons he states—namely, that with the shift toward British and French theoretical and methodological models in the mid-twentieth century it lost prestige and visibility. It would be a fallacy to equate this loss of prestige with diminishing legitimacy and intrinsic professional value. Failing to see a historic continuum in the development and convergence of collaborative ethnography and public anthropology would also be a mistake. Many of us take for granted the contributions of feminist anthropologists in widening our methodological understandings of dialogic ethnography. I was reminded of this recently when a local historian from another part of my state asserted that feminist approaches to oral history were too "open-ended" to provide facts. Needless to say, his remark put me on the defensive. As a male ethnographer, my appreciation for such pioneers as Barbara DuBois was not only revitalized but reached an existential level that made the power dynamics involved in the forging of collaborative approaches to ethnography painfully apparent.

Power, in fact, is the central issue in developing, articulating, and sustaining approaches to collaborative ethnography. Likewise, if it seems ironic that Lassiter chooses to explicate the contributions of postmodernist anthropologists in the emergence of public anthropology, one must appreciate the *utility* of theory—something that is all too often taken for granted. If poststructuralist/postmodernist theory fails to endorse "the Enlightenment Project," as some have claimed, it does provide a strong com-

plement to feminist theory in explicating the social construction of power structures and challenging constructions of objectivity. To that end, postmodernist anthropologists have taken the Foucauldian notion of theory as a "tool kit" (whose components may be applied in any number of combinations to fit the task at hand) to a utilitarian level.

While Lassiter's discussion is understandably restricted to innovations and developments within anthropology, the holistic and interdisciplinary nature of our profession warrants a discussion of contributions outside of the discipline. Where collaborative and public approaches to anthropology are concerned, some of us owe a debt to scholars such as Brazilian educator Paolo Freire, whose work inspired many anthropologists, sociologists, and other social scientists to develop models for participatory research with grassroots communities—models intended to address real-life problems by prompting people in such communities to see the value of their own knowledge.

In the end, the evolution and fate of a public anthropology sustained by collaborative ethnographic research depend on forces within the profession itself. Once we have reached agreement on whether we want to pursue knowledge for its own sake or for the betterment of humankind, that fate will be determined. Examining our discipline's history, I believe that the answer is already clear.

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Lassiter is both one of the foremost practitioners of collaborative ethnographic research and writing and one of the most articulate and well-versed promoters of these methodologies/epistemologies. With respect to my own work and general sympathies, he is of course preaching to the choir. My critical observations of this piece have to do with how he goes about reaching his conclusions. What sort of story is he telling here?

One characteristic of this text and of the oeuvre to which it belongs is that its author seems to cite and like just about everybody. There is little here about the work of those who are critical of the collaborative project. The article's broad historical scope and inclusive citations describe a significant genealogy of scholars who have explored and developed collaborative methods and goals in the course of the past 130 years. This approach is more common and more expected, I think, among North Americanist scholars than among Latin Americanist scholars, for example, and therefore Lassiter's narrative is a familiar and comforting one in the North Americanist scholarly tradition.

While approach contrasts with the all-too-common "gee whiz, look how new and improved we are" attitude of the 1980s postmodernists, he includes those very same po-mos in his chronology of collaborative work in a troubling way. He notes the disagreements between the po-mos and the feminists who are also part of his narrative chronology, but because he wants to make use of the insights from both sorts of scholars he plays down the significant areas of conflict between different postmodernists and different feminists. It is not, after all, simply that Clifford and Marcus (1986) sniped at feminist anthropology in their introduction or that only one woman was included in *Writing Culture*. I am not disputing the value of Lassiter's efforts to include ideas from both feminists and po-mos in a theoretical, epistemological, and methodological tool box for collaborative research and writing, but I think he needs to make clear that this *is* a tool box, *his* tool box, rather than naturalizing a coherent narrative of progress in collaborative ethnography and writing in which feminism and postmodernism share happy space under a big theoretical tent.

Because the story of the development of collaborative work that Lassiter tells is, as far as I am concerned, too smooth and seamless when he gets to the 1980s, I wondered what else he has glossed over in describing the work of scholars of previous decades. I don't know very much about how Radin's work was received by the Boasians—Kroeber, for example—but this article has made me curious about that. Lassiter's account of BAE ethnography strikes me as enlightening but rosy. I have recently told a very different story about the truth-making powers of the BAE in its determination of legitimate Indian identities (Field and Muwekma Tribe 2003). Perhaps my version is excessively negative, but both Lassiter and I note that a maverick ethnographer like Mooney posed real problems for the BAE mission. I stress the conflicts and disjunctures while Lassiter to a certain extent minimizes them.

Perhaps the biggest underlying problem is that he never grapples with the opposition to collaborative projects and its significance. This is, in so many words, a matter of power; it matters very much that Gross and Plattner (2002), for example, are opposed to collaborative work. If collaborative research and writing are to advance in the sense of developing better and more effective methods and strategies and convincing more anthropologists of their merit, it is important to know what forces are stacked against such projects and how to engage those forces and maybe even learn something from them.

Lassiter's story is a good one because it is inclusive and invites contemporary anthropologists to contemplate a long history of collaborative work in our discipline. I am not sure, however, that it provides adequate narrative or methodological cover for the future of this kind of work.

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Rightly or wrongly, anthropology—certainly the academic kind—is often associated with the safety of the academic ivory tower: distanced, disengaged, disassoci-

ated. Public anthropology, in contrast, should place us in the middle of society, engaged with the needs of the people it studies and conscious of its responsibilities in research. Following Lassiter's argument, collaborative ethnography provides us with the means to locate this desirable middle ground. Tracing the roots of this approach back to the very beginnings of anthropology in the United States, he leaves us with the obvious question why it has not penetrated the discipline, obviating the need for development of "a public anthropology that pulls together academic and applied anthropology in an effort to serve humankind more directly and more immediately."

Lassiter focuses on "the collaboration of researchers and subjects in the production of ethnographic texts, both fieldwork and writing," tracing the subject historically but also taking excursions into feminist and postmodernist anthropology. Yet, if early ethnographers like Boas collaborated closely with their key informants, why did this not develop into accepted ethnographic practice? Why do both feminist and postmodernist anthropology challenge "mainstream" anthropology as lacking it? To comprehend this, I think it necessary to take a closer look at the research process as a whole. The writing of ethnographic texts is not just the exchange of information between researcher and subject; there is always a third party involved, namely, the audience. (I am simplifying here, for the audience is a compound category consisting of the readers of the texts produced, the sponsors of research, employing institutions, and others.) Researchers have to keep their audience in mind when writing their texts. The audience requires information; the subject is the designated source of information, and the researchers gather and structure the information in ways accessible to their audiences.

When the research process is perceived in terms of the exchange of goods and services, there is a flow of information from the subject to the researcher and on to the audience. The counterflow consists of payments and occasional services but seldom of information relating to the research itself. There is therefore no equal exchange of information between the parties involved. The researcher looks for information in a structured way; the subject "possesses" this information but is in most cases unaware of the structure that the researcher is seeking to identify. The more structured the questions and the more aware the subject is of what the researcher is looking for, the more quickly results are reached and the higher their quality. Language, education, and mutual experience—and the latter can be both increasing familiarity with a key informant or shared experience of what is being studied (as in feminist research)—are all preconditions for rapport between researcher and subject. In most cases, however, the distance between researcher and subject in anthropological research is extreme simply because of the open and unstructured nature of the process, whether the research is done in the highlands of New Guinea or in central New York.

Does this affect Lassiter's argument? It does in the sense that there is nothing self-evident about collabo-

rative ethnography. The relationship between Boas and Hunt and the others that Lassiter mentions are examples of successful collaboration, but this is collaboration involving long-term field relations. Any relation between researcher and key informant(s) that spans a number of years and multiple periods of fieldwork has the possibility of becoming multistranded, with the key informant fulfilling a multitude of different roles ranging from adopted family member to coauthor. However, does collaboration on a personal plane also explain collaborative ethnography in feminist and postmodernist anthropology? Here we should, I think, refrain from seeing collaboration as the result of fieldwork (as is the case with Hunt and others) and fieldwork in the form of collaboration as one and the same phenomenon. The one is an occasional reality, the other more or less the holy grail of anthropology. Both feminism and postmodernism challenge mainstream anthropology by developing very focused relationships with their subjects and audiences. This can be understood as collaborative ethnography in the sense that the triangular relation between subject, researcher, and audience becomes extremely close, but it is not collaboration in the personal sense referred to above. If anything, collaboration is used as a metaphor here.

I doubt whether all this brings academic and applied anthropology closer together in a public anthropology. The need to serve humankind more directly and more immediately is not simply related to the way we do fieldwork and write ethnography but also to the way we deal with subject and audience alike. Any researcher can realize this on a personal level, but this is a far cry from realizing it at the level of the discipline.

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Lassiter elucidates a shrewd, creative, and necessary linkage between collaborative ethnography and public anthropology. The link helps solve several problems. The first problem is the one that critical, postmodernist, postcolonial thinkers have emphasized under various labels (Orientalism, imperialism, etc.)—the hegemony of privileged ethnographers over those whose lives they represent. The second problem is the one that others emphasize—the need for anthropology to address issues of concern to the wider society, the public. Merging the two issues is often difficult. I recall a panel designed to inform journalists about the situation in a certain place during a recent war there in which an anthropologist chose to treat the intricacies of his personal involvement and ethnographic methodology. When one goes "public" one is pressed to bypass issues on the participant side of participant observation and address the issues directly, perhaps erring on the side of imposing concepts, plans, and action on "others." This problem is acknowledged famously by the World Bank, for example, which is re-

thinking its approach to some extent, and it was perhaps a problem with an Institute for Human Issues that some of us attempted to create under the sponsorship of the American Anthropological Association as a follow-up to the speech that Lassiter cites.

Collaborative ethnography, then, is a welcome if partial solution to the linked problems of too much introversion and too much extraversion. The collaboration should draw one out of academic solipsism and into the social arena, at least as defined by the consultant, while staving off the bulldozer—the outside force, the reformer who does what is good for you whether you want it or not. Lassiter's solution is, of course, partial because even if two heads (ethnographer plus consultant) are better than one, the issues that press for debate and resolution transcend the perspective of any of us. What Lassiter provides, however, is a brilliant idea, a link between ethnography's strongest resource—relationship between ethnographer and consultant (and the field situation generally)—and societal issues. As he says, "We should not forgo the opportunity that most of us have for building a public anthropology from the ground up and from the center out."

Applauding and affirming Lassiter's welcome and well-articulated proposal, I would only add a further point obvious to those of us who have attempted to traverse from ethnography to public anthropology in the broad sense of endeavors that address societal issues. Anthropology alone is not enough and sometimes seems irrelevant. Other disciplines and a spectrum of social forces and social actors come into play, with the result that ethnography of any kind, consultative or other, may or may not surface easily. Goals and needs and the resulting pressures of time and resources may preclude our favored approach. However, Lassiter's argument gives us strong reason to include consultative ethnography in public-issue arenas as a way of including the voices of stakeholders as participants in a democratic process.

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Lassiter's "Collaborative Ethnography and Public Anthropology" warmed my heart. In a time when we are experiencing ongoing pressure to reinvent ourselves, it is excellent to be reminded that we actually have roots and that our history connects with the urgent issues we encounter today. I note that many of Lassiter's examples concern relations between anthropologists and Indigenous people in settler societies. My experience in Australia is also situated in this way, and I will bring a few of these perspectives into the discussion.

It would be very unusual today for anthropological research with Indigenous people in Australia to be carried out without an agreement that specified many of the issues Lassiter discusses. "Community" agreements raise further issues around the politics of "community,"

and the negotiations can go on and on. Such is the nature of collaboration. My most collaborative book (Rose 2002) took years not only in collaboration but also in finding a publisher who understood the project and was willing to fund a fairly costly production.

More significant, anthropological writing often brings people who have been relatively anonymous into the public eye. In Australia almost every aspect of Indigenous people's lives (health, education, culture, custom, land rights, employment, substance use, etc.) is subject to state scrutiny and public debate, usually with reference to statistics rather than to specific people or groups. The process of making people's lives accessible to a wider public is necessarily interactive with the people involved because they will bear the brunt of public response. This means that representation also is shaped by people's perceptions of that wider public and its goodwill, or lack of goodwill, toward Indigenous people.

Having negotiated these turbulent issues for a number of years now, what strikes me, as it does Lassiter, is the open-ended potential of collaborative work. In our postcolonial and/or decolonizing societies, dialogue is a source of creative action. Collaborative, dialogical research is a search for common ground, knowledge sharing, and moral action. For many, dialogue includes advocacy, persuasion, and imagination; multiple knowledges are negotiated and enhanced through engaging our multiple perspectives in a crumbling global order. Collaborative research acknowledges not only our coeval situation, as Fabian has argued so persuasively, but also that we and our research colleagues are caught up in the same fragile ecosystems and the same globalizing power relations. Practiced at local levels and disseminated far more widely, dialogical collaboration enables us—anthropologists, colleagues, and members of the wider public—to work together in seeking to shape futures that will enable life to flourish for us and for our coming generations.

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I am commenting on this fascinating paper as a British social anthropologist working in departments of Language in Education at King's College in London and the University of Pennsylvania. Whilst the paper focuses on the U.S. traditions of both collaborative ethnography and public engagement, it has interesting resonances with British anthropology, as Lassiter acknowledges. A space might open here for discussion amongst U.S. and British anthropologists concerning both the specific practical projects they have engaged in over the years and the complex questions he raises regarding the tropes of collaboration and dialogue and their potential for "transformation." In the U.K. I would cite two traditions that provide rich sites for such exploration: the Mass-Obser-

vation Project and current debates regarding anthropology and education.

The work of the Mass-Observation Project has long raised issues of collaboration, especially with respect to "ordinary" people's writing. Begun in 1937 and revived in 1981 after a break of two decades, the project engages a group of ordinary people from all over the U.K. to observe and reflect in writing on everyday life. The participants are known as "correspondents," and three to four times a year they are sent a "directive" asking them to focus on a particular set of topics (e.g., monarchy, wars, writing, health practices). Scholars and others then use the archive produced to research these topics in both contemporary society and the past. Currently, the archive contains over 1 million pieces of paper, and more than 3,000 people have served as correspondents since 1981.

The Mass-Observation Project is part history project, part anthropology, part (auto)biography, and part social commentary, but it is not history, anthropology, life history, or social commentary done only by those typically authorized to do those things (e.g., scholars, journalists). Something like a community or worker writing project, it is a forum for those typically excluded from the writing of history, but in contrast to such a project it relies on academics (and others in established institutions such as the BBC) to pull together the diverse written contributions and make them public. Recent uses of this material (see Sheridan et al. 2000) include attention to the writing itself as evidence of ordinary people's engaging in everyday literacy practices in ways often denied — or even decried —by educators and politicians concerned about "illiteracy" and "falling standards." In this sense, the collaborative nature of the writing is itself evidence for the "stream" of "citizenship and activism" to which Lassiter refers.

The reference to educators' often negative view of out-of-school literacy practices raises another dimension of this stream that is particularly salient at present in both the United States and Britain—the role of anthropology in education. There is a long history of connection with this dimension of the "public" sphere in the United States, linked, I would suggest, with the ways in which the ethnography-of-communication tradition pushes researchers to consider the relationship between communicative practices inside formal institutions and those outside them (Street n.d.). In the U.K. the connections have been less well developed, perhaps because the focus on language has been different, and there is scope here for considering what collaborations and forms of activism might be appropriate. The Royal Anthropological Institute in Britain has recently revived its Education Committee in order to consider exactly this question, working for instance towards formal examined curriculum in anthropology for schools at the same time as embedding anthropological insights and perspectives in general courses such as "citizenship." These initiatives suggest the need for an anthropology of such public anthropology (in line with Bourdieu's famous "sociology of sociology," which represents an important strand of the reflexive turn to which Lassiter refers [cf. Foley 2002]. The claim for collaborative ethnography as "a transformative practice" in that it forces attention outwards and is "inherently public" needs to be followed up with ethnographies of such a "public": there are, of course, numerous such "publics" and numerous sources of such data. Whilst Lassiter provides fascinating accounts of early texts, in a sense his "take" is that of a political anthropology collaboration rather an ethnographic account of the texts and practices that make up such collaboration. His incisive piece could make a good starting point for accounts of public engagements between anthropologists and collaborators of the kind evident in the Mass-Observation Project and in the anthropology of education.

Reply

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First and foremost, I thank the reviewers for their careful reading of my essay. They raise some important issues about the practice and implications of collaborative ethnography. Indeed, I consider it a great privilege to engage in a conversation such as this, and I will attempt to address here what I consider some of the more salient questions and concerns raised.

As Field so insightfully points out, my approach in this essay is primarily to construct a "toolbox" of sorts, to recognize that which came before, and, admittedly, to emphasize connection rather than disjuncture. I choose to take this approach because I believe that seemingly disparate camps of thought—Americanist, feminist, and postmodernist among them—have a great deal in common when it comes to their increasing focus on voice, power, and representation. Have so-called postmodernists, for example, been dismissive, even sexist—for example, in writing that feminism "has not produced either unconventional forms of writing or a developed reflection on ethnographic textuality" (Clifford and Marcus 1986:21)? Undoubtedly so. Have some feminists seemingly played the same card—for example, in arguing that "feminist ethnography is writing carried out by a woman author who is always aware that she is a woman writing" (Behar 2003:40)? Perhaps. To be sure, the differences between feminism and postmodernism are very real—among them the differing literatures to which each looks for inspiration or the position taken by many feminist ethnographers that they are "others studying others." But for anyone searching for the theoretical roots of a collaborative ethnography, these differences pale next to the similarity of their recurrent calls for reciprocation, coauthorship, citizenship, and action. After reading hundreds, if not thousands, of pages from both camps, it seems to me that much paper has been wasted arguing for difference when there is so much common ground

between feminism and postmodernism and, indeed, between them and other camps such as Americanist anthropology. I am, of course, not the first to make such a connection: the British anthropologist Pat Caplan (1988: 16) wrote almost two decades ago that "second-wave feminism and postmodernism are contemporaneous, and share many of the same sources, yet they are seen as independent developments." This is an important insight (rather than a gloss, as charged by Field), one which *should* enhance our common efforts to destabilize the conventional hegemony of ethnographic practice and representation.

And this, of course, is what collaborative ethnography is all about. As Cook argues, collaborative ethnography is ultimately about *power* and, I would add, *control*—about who has the right to represent whom and for what purposes and whose discourse will be privileged in the ethnographic text. Americanists, feminists, and postmodernists (as well as applied and public anthropologists) have long dealt with this issue in various ways. But, as Jaarsma points out, the question remains why collaborative ethnography has not developed into accepted ethnographic practice and why doing collaborative ethnography has taken a back seat to our more theoretical arguments about collaboration. I have speculated on this issue before (see, e.g., Lassiter 2001) and have provided a much more in-depth discussion along these lines in the opening and closing chapters of the *Chicago Guide to Collaborative Ethnography* (Lassiter 2005); suffice it to say here that I believe that, even today, most anthropologists are unwilling to give over power and control of their work to others, something that collaborative ethnography often demands. As I suggest in this essay, this was true as anthropology moved from museums (where collaboration was much more common) to the academy. In many ways, unwillingness to give up power and control continued through the feminist and postmodernist critiques—which, being largely based in the academy, have always valued and emphasized theory building over the applied practice required of collaborative ethnography. Simply put, doing collaborative ethnography—*really* doing it, with consultants directing the text's content—brings little prestige, power, and authority for academics who depend on prestige, power, and authority for the growing of their careers. While many ethnographers are quick to give collaboration lip service, few actually engage in it. It is much safer to theorize it and, in practice, to engage it only as metaphor. A recent incident in my own department is instructive.

One of my graduate students recently embarked on a collaborative project with members of the local African American community. Because our graduate school allows the inclusion of local experts on graduate thesis committees when the need arises, my student—in a deliberate move to transcend the metaphor of collaboration—decided to include one of her principal consultants on her committee: he had worked with her for over a year and a half (much closer than any other faculty member, save me, perhaps), knew more about local African American history than anyone in the department, and,

as a published writer, was recognized and widely cited among scholars of "Middletown" as *the* local expert on its African American history and culture. The student's inclusion of this individual on her committee seemed reasonable, logical, and smart. Moreover, as a self-identified black feminist, she saw it as important to include an African American on a committee that was otherwise all white. I would not have imagined that my senior colleagues would take offense—especially because, as do most anthropologists, they regularly offered lip service to collaboration. But they balked: including the student's consultant on the committee, they charged, would be a "conflict of interest," apparently because he would have too much control over the direction of the thesis (as if faculty committee members did not exert control over the direction of the thesis). That some of them went so far as to propose a new policy that would prevent future students from including consultants on their thesis committees suggests to me that *actually* doing collaborative ethnography continues to pose a very real threat to the power and control which so many anthropologists and other academics still hoard.

Perhaps I am focusing too sharply on the "political anthropology of collaboration," as Street points out. But my overriding purpose in this essay is not only linking Americanist, feminist, and postmodernist anthropology within a common "toolbox," if you will, but also situating collaborative ethnography within a discussion that calls for a more explicitly engaged, public anthropology. Jaarsma reasonably wonders if I am being too optimistic about collaborative ethnography's capacity to transform ethnographic practice and writing—and indeed, the discipline of anthropology—along these lines. It is, to be sure, only a partial solution, as Peacock suggests, for advancing a public anthropology: it often works well when ethnographers work with indigenous communities, as noted by Deborah Rose. Like Rose's, most of my own collaborations have been carried out in indigenous communities, where my consultants—who live within streams of previous representations—now take the power relations inherent in ethnographic representations *about them* very seriously. To be sure, when representation is a central issue in ethnographic practice—and it is increasingly so—collaborative ethnography works especially well. But I would argue that it can go much farther than this, extending into other local communities (such as in the "Other Side of Middletown" project) and beyond. Street's discussion of the Mass-Observation Project is an example, and, as in public history and public folklore, such collaborative projects share not only power and control but also a vision of citizenship and action that, as I suggest in this essay, is at the heart of both collaborative ethnography and public anthropology.

A final issue is the *critique* of collaborative ethnography, on which, admittedly, I focus little attention. For the past several years much of my work on collaborative ethnography has attended to building—if I may be so bold—a loose paradigm of sorts that establishes a more explicit and deliberate collaborative ethnography, one which takes into account more fully its history, theory,

and practice. This project is necessarily incomplete, and I see this essay as only a step in the construction of this tripartite model. Few anthropologists have actually taken the assumptions and implications of collaborative ethnography to task in a serious scholarly manner. Very few of the contemporary collaborative ethnographies with which I am familiar, for example, have been reviewed in mainstream journals such as *American Anthropologist* or CURRENT ANTHROPOLOGY. Such reviews, of course, provide important spaces for discussion and critique. So do essays such as this. All in all, though, we have yet to move beyond arguing about what makes one school of thought different from the other to a deeper consideration—and critique—of the implications and consequences of a more explicit and deliberate collaborative ethnography. My hope is that this conversation can at least get us started.

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