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Beyond the Lonely Anthropologist: Collaboration in Research and Writing

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FOR PEOPLE WHOSE BUSINESS it is to be sociable, we anthropologists often have an oddly isolationist view of ourselves. Working with people is of course the very stuff of fieldwork; yet recent discussions of the nature of our field experiences typically focus only on a single type of person with whom we routinely interact: those we continue uneasily to term “informants” (see Sanjek 1993). Happily, following an earlier underrecognition of scholarship in this area, there is a growing literature on the nature of our relationships with informants, friends, and others we first encounter while conducting fieldwork.¹ Several recent papers also problematize in productive ways the distinction of “native/nonnative” that we sometimes take for granted.² But such is not the case for other forms of collaborative work in which anthropologists so frequently engage in research and writing: our enduring relationships with spouses or other domestic partners, as well as our collaborations with colleagues, whether or not we and/or they are “native,” and whether or not they may also happen to be spouses or otherwise intimate partners.

Strangely enough, despite the recent glut of fieldwork memoirs, the fact that we often work in tandem with such others has been all but ignored. Even field memoirs in which a spouse is present rarely have much to say about the effect of the spouse on the researcher’s fieldwork.³ We ourselves often contribute to this tendency toward spousal or coauthorial invisibility in the technical decisions we make in our writings: most coauthors either merge their voices by using the first-person plural, or they avoid a voice altogether and write in the impersonal style of academic prose.⁴ Both tactics have the same effect: they leave unexplored and unproblematized the nature of dual authorship.⁵

Why the consistent silence? In the brief remarks that follow, I will suggest several possible explanations for the continuing neglect, although undoubtedly there are other reasons not yet understood.

First, it has taken anthropologists long enough to come to terms with the very issue of fieldwork as a contested site filled with continuing moral dilemmas. Yet increasingly, we *have* begun to recognize the field endeavor as a site occupied by a group of interacting, positioned actors—the anthropologist and various “others”—whose attempts at a conversation are inevitably shaped by mutual images, suspicions, assumptions, and histories. Perhaps not surprisingly, we have been even more reluctant to take the critical next step: to problematize the different sets of social and intellectual relations that, in one way or another, we often bring with us to the field, or in which we become intricately enmeshed while in the field, as members of a professional and/or personal team.

Second, in the case of collaborators who are also spouses or otherwise intimate partners, there may be a culturally shaped shyness at work: a reluctance to limn the complex and problematic terrain where the personal and professional converge. No less than other Westerners and/or urbanites, academics work in a setting that generally urges a firm boundary between public and private.⁶ Accordingly, a distinctly analytical gaze on the oxymoronic zone of academic intimacy may challenge a basic premise that we often feel compelled to uphold in the way we conduct our necessarily divided lives. If this has been so for heterosexual couples, it is even more the case for gay and lesbian couples, whose very identities have been all but ignored or denied in our discipline until very recently.⁷

Third, despite the large number of explicitly coauthored works in anthropology, we may have been unwilling to consider the implications of such collaborations (whether or not they involve fellow anthropologists) because, as Elizabeth Kennedy remarks in this issue, we subscribe, perhaps somewhat unconsciously, to the widespread Western tendency to see the *author*, in particular, as a singular creation standing alone in her or his artistic achievement. While there has been much critique of this position in recent years,⁸ we social scientists may still cling, however unknowingly, to this venerable tradition.

In turn, this may go some way toward accounting for our collective disinclination to come to terms in any serious fashion with the written collaborations in which many of us engage. Thus, as Kennedy (this issue) and Stephen Murray (1993) each point out, when we read a coauthored work, it is rarely clear which scholar did what. Did one author write each section separately, each then editing the other, or did they collaborate sentence by sentence? Alternatively, did one author supply the basic ideas and the other the technical backup? In the course of writing, did the coauthors have analytical disagreements, and if there had been a different outcome of such discussions, might alternative interpretations have prevailed? No matter how these seemingly personal questions are answered, how did the prior relationship of the coauthors—whether colleagues, intimate partners, or both—shape the collaboration and the final text? Did sociological and political considerations affect the outcome? For example, do the perspectives of a coauthor who happens to be a woman, a member of an ethnic minority, a student, or otherwise defined—whether explicitly, or by cultural assignation—as a junior partner, tend to get nudged out in the final analytical pose publicly adopted by the pair? These questions, I suggest, are central if we want to assess the nature, merit, and impact of coauthored writings—and of writings that may have hidden, uncredited coauthors; yet such questions have gone largely unasked.⁹

A related issue revolves around the solitary fieldworker, which is equally dominant in our views of ourselves. As Stephen Gudeman and Alberto Rivera point out (1993), this persistent image is allied to an epistemological conviction that the fieldworker ought to make herself as invisible as possible in order to get at the “real truth” of the culture “out there.” But clearly, the more people in the anthropological team, the less this increasingly problematic fiction is able to be maintained in any viable manner.

Still, we might take some solace in the fact that as a discipline, we are not alone in our reluctance to engage such issues. Discussing Western literature, the critic Jack Stillinger (1991) has recently explored the extent of hidden collaborations and other sorts of authorial influences that have gone underanalyzed by his critic colleagues. Yet, he contends, such mutual literary involvements have shaped the work of some widely read and respected Western authors. For example, Stillinger claims that some of Samuel Coleridge’s major literary pieces are the result of “creative plagiarism” (96 ff.). Equally provocatively, considering a major work of John Stuart Mill, Stillinger asks, “Who wrote J. S. Mill’s *Autobiography*?” He answers his own question by contending that this “autobiography” is in fact the work of seven authors (50 ff.).

Stillinger’s detective work will undoubtedly serve to prod his colleagues into reconsidering the reverence for single authors-cum-saints that invests Western literary critic-hagiographers no less than it does Westerners at

large. Indeed, more recently, critic Françoise Meltzer (1994) has interrogated the theoretical underpinnings of the Western practice of literary criticism itself by calling into question the very assumption of artistic originality that lies at the heart of the discipline. In our own field, an equivalent assumption may help account for our unwillingness to inspect critically the nature of works that we already know to be collaborations.¹⁰

A further factor, as Kennedy points out, undoubtedly involves gender. Although anthropology has an impressively high proportion of women in the discipline, there are nevertheless strong signs of a macho ethos that pervades the intellectual orientations of many anthropologists, both male and female. Long ago, feminist anthropologists began to identify such strains as lying at the heart of much classic social theory.¹¹ The Marlboro Man-like impulse to celebrate individual achievement rather than collective collaboration may be one component of this tendency. Recently, the philosopher Sara Ruddick (1990) has argued that “maternal thinking”—which, in her view, includes an orientation toward group cooperation rather than individual competition—is socialized in women but not men in Western societies. If she is correct, then the “masculine” orientation of the theoretical foundations of our discipline may likewise be at least partly responsible for the polite ignoring of our professional and personal collaborations. The fact that many of these professional collaborations involve a male-female pair may further disincline us to explore their nature, as either sexual or political considerations—or, even worse, a combination of both—may explain some of their contours in ways that could make us distinctly uncomfortable.

And yet, we are all aware that anthropologists often work in pairs—with a spouse or other domestic partner—and often, as well, in teams of researchers. One need only think of the !Kung team out of Harvard, or, earlier, the group of brilliant British social anthropologists associated with the Rhodes-Livingstone Institute to realize the longstanding tradition of professional teamwork in anthropology. And of course for domestic partners, we can look back to the creative teams of Mead and Fortune, and then Mead and Bateson, to recognize that there is much in the way of early models for the many heterosexual anthropology couples who now work creatively together in the field (Ariëns and Strijp 1989:11–17). Murray (1993) draws attention to several more recent, illustrious anthropologist couples; doubtless dozens of others could easily be added to the list.

Although anthropologists have rarely taken account of their spouses in print other than the inevitably brief mention in the acknowledgments (see Ariëns and Strijp 1989:7), the reverse is not as true: a handful of spouses—so far, all wives—have begun on their own to declare themselves. Rosemary Firth published an early, illuminating article on the experience of being the wife of eminent

anthropologist Raymond Firth (1972). During the same period, Elizabeth Warnock Fernea published two highly successful books that looked at the female half of Muslim life, to which her husband, Robert Fernea, was denied access in Iraq (1965) and then Morocco (1975) (see also Fernea and Fernea 1989). More recently, Marianne Alverson has written an engaging memoir (1987) of daily life in a Botswana village while accompanying her husband, Hoyt Alverson, along with their two children; artist Julie Myerson (1990) has described her experiences accompanying her anthropologist husband, Gary Urton, during his fieldwork in a Peruvian village; Edith Turner (1987) has chronicled her own very rich fieldwork among the Ndembu while accompanying her late husband, Victor Turner, along with their young children (see Bruner 1991); and Marion Benedict has written a semifictionalized account of her work in the Seychelles with anthropologist-husband Burton Benedict (Benedict and Benedict 1982; B. Benedict 1993; M. Benedict 1993). Yet even these engaging personal accounts tend to gloss over the nature of the authors' relationships with their husbands and how these may have shaped each of their fieldwork experiences. In the attempt to provide a balancing of the ethnographic record, these accounts of fieldwork collaboration for the most part offer half of a whole, and so once again the nature of collaboration itself as a mode of research goes unexplored.¹²

So far I have used the term "collaboration" unflexibly. Oddly enough, as with our equally problematic term "informant," the word has extremely pejorative connotations derived from the "intelligence" community and other dubious political activities. Are our own professional and personal collaborations tainted by a sort of consorting with the enemy? Certainly not! our first impulse must be to declare. Yet it is likely that collaborative projects often contain hidden sources of discomfort, accommodation, and compromise that may keep them at least distantly allied to the problematic political terrain just alluded to. Inevitably, some collaborative projects are more professionally successful and personally fulfilling than others—and of course the participants in a single project may go through a gamut of such reactions at different stages. The point here is that collaboration may exhibit infinite variations, each posing its own range of problems.

Finally, what difference does collaboration make in research, writing, or both? And what difference does the particular *kind* of collaboration make—whether it is purely professional, purely personal, or some combination of both? The essay by Kennedy poses these critical questions in the context of a particular set of intriguingly interdisciplinary collaborative projects. Yet in suggesting her own specific answers, Kennedy's reflections will undoubtedly serve to provoke further questions.

Accordingly, I hope that Kennedy's piece will open up a long and continuing conversation among our colleagues. To that end, I invite anthropologists to contribute to this conversation. Famous and closet collaborators, married and nonmarried collaborators, straight, gay, and lesbian collaborators, anthropological and interdisciplinary collaborators, native and nonnative collaborators, would-be collaborators and failed collaborators—all have something to say about the endeavor of fieldwork conducted in some collaborative setting and the difference that collaboration makes in the very practice of ethnography. The more we read of such collaborative tales, I suspect, the more we will come collectively to realize how our discipline overall is characterized to a great extent by a pervasive structure of cooperation in one form or another. Indeed, as Gudeman and Rivera state, "Doing ethnography *is* joint work, teamwork" (1993, emphasis added).

At the same time, disciplinary reflexivity concerning such normative practices is useful not for the sake of solipsism, but, ultimately, for serving as another tool with which we understand what it is to be human—both for ourselves in our relations to others, and for our interpretations of those others' lives. Interrogating our own research and writing practices—including our various collaborations—and the often unconscious assumptions that underlie them, is one means of taking responsibility for the assertions we make about others. In short, thinking through, challenging, inspecting, and assessing our various disciplinary collaborations is a collective project, I suggest, whose time has come. And acknowledging that mandate may well enable us to retheorize the discipline itself and the philosophical foundations on which our Lone Ranger image of ourselves has been built.

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Notes

Acknowledgments. As always, I am grateful to my sometime coauthor but always friend and husband, Philip Graham, for his perceptive comments during the many lively discussions we have held together both during fieldwork and through the writing of our own collaborative field memoir (Gottlieb and Graham 1994). For agreeing to engage with me on an earlier joint work of a far different sort (Buckley and Gottlieb, eds. 1988), I am also grateful to my colleague Thomas Buckley, whose very supportive work style inspired me to continue walking down the collaborative path.

An earlier version of this paper was presented at the 92nd annual meeting of the American Anthropological Association in Washington, D.C., in November 1993, in a session on collaboration. We were fortunate to engage as discussants John Comaroff and Andrew Strathern, each of whom has participated in a range

of extraordinarily fruitful collaborations of his own. Their lucid and suggestive comments were inspirations to us all.

1. An incomplete list would include: Bêteille and Madan 1975, Bowen [Bohannan] 1954, Casagrande 1960, Deluz et al. 1978, Dumont 1978, Freilich 1977, Golde 1970, Henry and Saberwal 1969, Jongmans and Gutkind 1967, Papanek 1964, Powdermaker 1967, Read 1965, Rynkiewich and Spradley 1976, Spindler 1970, and Wax 1971. Examples of more recent field memoirs and reflections include Cesara 1982, Gibbal 1994, Gottlieb and Graham 1994, Okely and Callaway 1992, Whitehead and Conaway 1986, and Williams 1994.

2. Some recent writings on this are pieces by D'Amico-Samuels (1991), Hong (1994), Kim (1993), Motzafi-Haller (1993), Murray (1993), Narayan (1993), and Ohnuki-Tierney (1984).

3. Field memoirs in which spouses are present but not, as it were, accounted for include those by Hayano (1990), Messenger (1989), Mitchell (1987), Shostak (1981), Stoller and Olkes (1987), and Ward (1989). On the other hand, the effect of one's child(ren) on a parent's fieldwork has recently begun to be explored (Cassell 1987 and Fernandez and Sutton n.d.).

4. For some reflections on the frustrations that my partner Philip Graham and I initially experienced with this writing strategy, and the somewhat unorthodox solution we eventually chose, see Gottlieb and Graham 1993.

5. My remarks here on written collaborations assume a pair of coauthors. Of course some coauthored works involve three or more authors; the questions I pose are even more relevant in such cases, as Kennedy's essay (this issue) demonstrates.

6. For some penetrating anthropological analyses of this phenomenon, which is by now notorious, see, for example, Comaroff 1987 and Rosaldo 1980; for another perspective, see Sennett 1977.

7. Some very recent writings on this subject have just begun to explore this topic, such as Ariëns and Strijp 1989:18–19, n.1, Murray 1993, and Elizabeth Kennedy (this issue).

8. The two paradigmatic pieces are Barthes 1977 and Foucault 1977[1969].

9. But see some brief but pungent remarks by Ariëns and Strijp concerning spouse-coauthors (1989:7).

10. I also suspect that there may be more than a few singly authored works of anthropology that, on close inspection of the acknowledgments, ought to be billed as coauthored—as is suggested, for example, by Bruner (1991), Murray (1993) and Tedlock (in press). This, however, is another project.

11. For overviews of our discipline that call attention to this hidden bias, see, for example, Caplan 1988a, 1988b; Milton 1979; Schrijvers 1979; and Shapiro 1983.

12. For one short account by a female anthropologist who does discuss her husband's role in the field and the effect it had on her own fieldwork, see Friedl 1986. More recent pieces that address various issues involving heterosexual marriages between two anthropologists include Oboler 1986 and the four brief articles that appear in Ariëns and Strijp 1989. To my knowledge, my own partner is the only non-anthropologist husband of a female anthropologist to publish on the joint fieldwork experience (Gottlieb and Graham 1994)—in his case, from the perspective of a fiction writer.

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In Pursuit of Connection: Reflections on Collaborative Work

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WHEN I REFLECT on my three experiences with collaborative research, I am struck by how each raised a different set of rewards and problems. I carried out my first research accompanied by my ex-husband, Perry Kennedy, in Colombia, South America from 1964 to 1966; it resulted in my individually authored dissertation and three jointly produced films. The second was the collective writing of the book *Feminist Scholarship: Kindling in the Groves of Academe* from 1977 to 1985 with four other scholars: Ellen Carol DuBois, Gail Paradise Kelly, Carolyn W. Korsmeyer, and Lillian S. Robinson. The third was the joint research and writing of *Boots of Leather, Slippers of Gold: the History of a Lesbian Community* with Madeline Davis, a librarian, singer, and songwriter, from 1978 to 1993. These represent three different kinds of teamwork: The first grew out of the emotional and social bonds of the husband-wife relationship; the second entailed crossing disciplinary boundaries with colleagues; and the third brought together the academy and the lesbian community.

Thinking about my 30 years of teamwork, my first idea for a title of this article was "My Life as a Collabora-

tor." The ambiguous meaning of "collaborator" made it unsuitable, while highlighting the suspicion that the society in general and the academy in particular—at least in the humanities and social sciences—have had about cooperative work. It is unquestionably easier to do cooperative research and writing in the 1990s than it was in the 1960s. Three major intellectual developments are responsible for this change in the late 20th century—feminist scholarship, anticolonialist scholarship, and interpretive anthropology—all of which present challenges to the traditional "objective" report authored by the heroic anthropologist, the scientist of culture who works alone.

While narrative ethnography with its emphasis on dialogue and reflexivity has freed me to experiment with different forms for research and writing, feminist and anticolonialist writings have given me the self-consciousness about social hierarchy that is needed for successful team research. In addition, feminism—the political as well as the scholarly—supported me to initiate and pursue cooperative work and encouraged respect for it in the academy. Until now most interpretive anthropology has tended to ignore the contribution of feminism,¹ but I have found the feminist critique of gender hierarchy extremely useful when working with others in a society where difference usually means hierarchy.