FROM PARTICIPANT OBSERVATION TO THE
OBSERVATION OF PARTICIPATION: THE
EMERGENCE OF NARRATIVE ETHNOGRAPHY

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Beginning in the 1970s there has been a shift in cultural anthropological methodology
from participant observation toward the observation of participation. During participant
observation ethnographers attempt to be both emotionally engaged participants and coolly
dispassionate observers of the lives of others. In the observation of participation, ethnog-
raphers both experience and observe their own and others’ coparticipation within the eth-
ographic encounter. The shift from the one methodology to the other entails a representational
transformation in which, instead of a choice between writing an ethnographic memoir
centering on the Self or a standard monograph centering on the Other, both the Self and
Other are presented together within a single narrative ethnography, focused on the character
and process of the ethnographic dialogue.

The mythic history of anthropology is populated by four archetypes: the
amateur observer, the armchair anthropologist, the professional ethnographer,
and the “gone native” fieldworker. Eighteenth- and early nineteenth-century
amateur accounts—written by explorers, travelers, medical doctors, colonial
officers, missionaries, and the idle rich—provided the materials for the armchair
ruminations of late nineteenth-century anthropologists. It was not until after
the First World War that academically trained ethnographers, in any numbers,
began undertaking intensive fieldwork and constructing ethnographic infor-
mation.² It was in this shattered, nihilistic world—which gave birth to the Jazz
Age with its bobbed hair and bathtub gin—that intellectual value was placed
on traveling to distant places in order to study and reconstitute a humane order
out of devastation and disorder.

In the French tradition, the emphasis was on team research, using a doc-
umentary approach.³ The British and the American traditions emphasized in-
dividual research, using an experiential approach that was labelled with the
oxymoron “participant observation.”⁴ There is no doubt but that this peculiar
methodological stance causes stress, for as Benjamin Paul (1953:441) has
noted, “Participation implies emotional involvement; observation requires
detachment. It is a strain to try to sympathize with others and at the same time
strive for scientific objectivity.” This simultaneously empathetic, yet distancing,
methodology, which is widely believed by ethnographers to produce data that
somehow reflect the native’s own point of view, in time became the principal
mode of production for anthropological knowledge.⁵

Ever since Malinowski (1922[1961]:25) suggested that an ethnographer’s
goal should be “to grasp the native’s point of view, his relation to life, to realize
his vision of his world," there has been an expectation that participant ob-
ervation would lead to "human understanding" through a fieldworker's learning
to think, see, feel, and sometimes even behave as a native. Since we can only
enter into another person's world through communication, we depend upon
ethnographic dialogue to create a world of shared intersubjectivity and to reach
an understanding of the differences between two worlds. In order to accom-
plish this form of human understanding, it is necessary to undertake an engaged
period of fieldwork. It is this experience that has become the professional
ethnographer's necessary initiation—variously referred to as a puberty rite, ritual
ordeal, or rite de passage.

Ethnographers who have learned not only the language but also appropriate
behavior (including nonverbal communication codes) have been transformed,
sometimes quite radically, by their fieldwork experience. In his ethnography,
Poker Faces, David Hayano (1982:149) reveals that he became so immersed
in the subculture of California poker players that "within several years I had
virtually become one of the people I wanted to study!" Liza Crihfield Dalby
not only took on the social role of a geisha (a charming erotic entertainer)
during her fieldwork in Japan, but she also claims to have become one in both
body and spirit. Her assertion that she learned to think and behave as a geisha
suggests the "gone native" archetype of the anthropological imagination. How-
ever, since she also published an ethnographic memoir about her fieldwork
experience, Geisha (1983), she might more accurately be described as having
"gone native" culturally, but not socially, or, better yet, as having become
bicultural. In other words, while Dalby became a geisha in Japan to an im-
pressive degree, she did not totally abandon her ethnographic role and status
back in the United States.

The number of fieldworkers who have given up anthropology altogether in
order to join the flow of life elsewhere is very small. Perhaps the most famous
case of a "gone native" fieldworker is that of German scholar Curt Unkel, who
went to Brazil early in this century to study the Indians of the Amazon region
and never returned. Although he was adopted into a tribal group and took the
Guarani name Nimuendajú, he maintained a house in the city of Belém. He
also wrote a series of ethnographic manuscripts and asked the American an-
thropologist Robert Lowie for help both in publishing his work and in obtaining
a research grant to continue his field studies. Another candidate for the "gone
native" award is Frank Hamilton Cushing. During his four years of fieldwork
at Zuni Pueblo, he was initiated as a war priest, but he nonetheless continued
to do fieldwork and published a number of ethnographic works—including an
early narrative account of the fieldwork experience (Cushing 1882–83). Eventu-
ally, he married a white woman and left Zuni to settle on the East Coast. A
third candidate is Verrier Elwin, an Englishman who went to India, married
into a tribe, became an Indian citizen (even a close confidant of Mahatma Gandhi
and Jawaharlal Nehru), and was recognized as a pioneer Indian anthropologist.
He published a series of extraordinarily detailed ethnographies much admired
in both India and England.

For these individuals, fieldwork was not a rite of passage, or route to an
academic union card, but rather the lived-reality of the field experience was the center of their intellectual and emotional missions as human beings. As Elwin expressed it, “For me anthropology did not mean ‘field-work’: it meant my whole life. My method was to settle down among the people, live with them, share their life as far as an outsider could and generally do several books together. . . . This meant that I did not depend merely on asking questions, but knowledge of the people gradually sank in until it was part of me” (Elwin 1964:142).

What seems to lie behind the belief that “going native” poses a serious danger to the fieldworker is the logical construction of the relationship between objectivity and subjectivity, between scientist and native, between Self and Other, as an unbridgeable opposition. The implication is that a subject’s way of knowing is incompatible with the scientist’s way of knowing and that the domain of objectivity is the sole property of the outsider. Several fieldworkers have rejected this sharp analytical distinction between Self and Other. Bennetta Jules-Rosette, who joined an African church in the process of studying it, wrote that “through continued observation, I began to develop a repertoire of knowledge and expectations, or a common culture, that was shared with participants and created in interaction with them” (Jules-Rosette 1975:21). In his introduction to Jules-Rosette’s book, Victor Turner observed that “to each level of sociality corresponds its own knowledge, and if one wishes to grasp a group’s deepest knowledge one must commune with its members, speak its Essential We-talk” (in Jules-Rosette 1975:8). In phenomenological terminology, this communicative interaction, or “we-talk,” belongs neither to the realm of objectivity nor to that of subjectivity, but rather to “human intersubjectivity.” It is this realm that distinguishes the human sciences from the natural sciences as a field of investigation.  

There was a time when assuming a participatory stance, such as taking on an apprenticeship role, was criticized due to the belief that it might somehow interfere with the objectivity of the description (Coy 1989:108). The enculturation that accompanies any intensive fieldwork experience might also be critiqued for the same reason, but this has not always been the case. Solon Kimball portrayed his own involvement in the Irish world as so intense that, without any formal instruction in spectral sightings, he saw a well-known local apparition. Describing his experience, he suggested that “the time may have arrived when we are ready to undertake systematic observations of the processes of induction and involvement in another culture” (Kimball 1972:192). The time has indeed come. There are now several detailed ethnographic reports of intensive enculturation, including successful formal and informal apprenticeships (Chernoff 1980; Cooper 1980; B. Tedlock 1982; Johnson 1984; Coy 1989).

Nevertheless, the public revelation of participatory details of the fieldwork experience is still considered embarrassingly unprofessional by some ethnographers. It is as though fieldwork were supposed to give us two totally independent things: reportable significant knowledge and unreportable mysticism and high adventure. If we were to be so foolish as to make the mistake of
combining these elements, it would somehow seriously discredit our entire endeavor. Paul Rabinow (1977:10) nicely summarized this contradiction: “As graduate students we are told that ‘anthropology equals experience’; you are not an anthropologist until you have the experience of doing it. But when one returns from the field the opposite immediately applies: anthropology is not the experiences which made you an initiate, but only the objective data you have brought back.”

In the past, the most common way out of this double bind was either to publish the fieldwork experience as a novel or else to suppress the actual events that took place during the research, together with all reference to specific individuals, including the ethnographer and the ethnographic subjects. When the novelistic path was taken, some ethnographers were careful to distance themselves from the work by using a pseudonym. Margaret Field published her memoir of doing ethnography among the Gâ of West Africa, Stormy Dawn (1947), under the name Mark Freshfield. Laura Bohannan novelized her experiences as a neophyte ethnographer in West Africa under the name Elenore Smith Bowen. Her book Return to Laughter (1954) was a highly acclaimed, commercially successful publishing venture, with more than 350,000 copies printed as of this writing. Philip Drucker published his Mexican ethnographic novel, Tropical Frontier (1969), under the name Paul Record. This use of pseudonyms enabled these ethnographers to publish their field experiences and keep this activity totally separate, even secret, from their profession. This distancing move indicates, I believe, that individuals felt that publishing a personal fieldwork account would somehow damage their reputations or credibility as professional ethnographers.11 Later in life perhaps, as Laura Bohannan did, they could reveal their pseudonyms to the profession and take credit for their literary output.

The second and more common way of dealing with field experiences was simply not to mention them, but instead to abstract the meaningful data from the objects of study and to remove all traces of the observer. The result of this strategy is that, as Stephen Tyler (1987:92) recently noted, “ethnography is a genre that discredits or discourages narrative, subjectivity, confessional, personal anecdote, or accounts of the ethnographers’ or anyone else’s experience.” This suppression of firsthand experience in ethnographic monographs has been described by George Devereux, in From Anxiety to Method in the Behavioral Sciences (1967:97), as a professional defense mechanism producing “scientific (?) ‘results’ which smell of the morgue and are almost irrelevant in terms of living reality.”12

Because ethnography is both a product and a process, our lives as ethnographers are embedded within field experience in such a way that all of our interactions involve choices, and thus, “there is a moral dimension—made explicit or not—in all anthropological writing” (Herdt 1988:185). What we see or fail to see, reporting a particular misunderstanding or embarrassment, or ignoring it, all involve choices. We also make a choice when we edit ourselves out of our final written ethnographic product. This is so no matter how narrow
the focus or scientifi city of our research design. That a personal account of fieldwork is always an option, regardless of our chosen topic or research methodology, is demonstrated in Dennis Werner's (1984) beautifully crafted Amazonian narrative ethnography. In this work he combines a lively personal account of his life in the field, a rich portrait of Mekranoti culture, and a precise description of the various quantitative approaches he used during his research. Roger Sanjek (1990:254) classified Werner's text as among the "most ethnographically rich of the personal accounts . . . written by one who admits, 'While I was still in graduate school, a fellow student once complained that every time I opened my mouth numbers came out.'" Gilbert Herdt (1988:186) points out that in ethnographies that are totally removed from a discussion of fieldwork, "the author's field tactics and experience can remain invisible," leaving only "impersonalized—no—depersonalized accounts."

An early exception to the impersonal style in anthropological writing was Spider Woman: A Story of Navajo Weavers and Chan ters, published by Gladys Reichard in 1934. In this engaging rendering of her fieldwork experience, Reichard narrates—in the first person, active voice, present tense—four separate field trips to Arizona. Her re-creation of her experiences and feelings is full of precise descriptions of the details of everyday life, including her own unspoken thoughts and reactions to events she shared with her Navajo family. In a painful vignette, we see her struggling to remain within the bounds of the Navajo world when her adoptive grandmother undergoes a gruelling eight-day traditional curing ceremony, though she has what Reichard believes to be pneumonia. "My sympathy has run the gamut from the weakest sort of pity to bitterness at not finding the doctor," she writes, "from grim fatalism at being compelled to fetch the Chanter to the most abject futility at watching Maria Antonia shampooing her hair in that wind. It now flares into feverish anger which dies down in despair as I see her rest once more disturbed, when she is forced by the tenets of the cure to sit up while the Chanter blows medicine—pine leaves floating on water—on her side where the pain torments her" (Reichard 1934:252–53). The overall plot line of this account follows that of a novel of education; thus, in the last chapter, entitled "Degree in Weaving," Reichard portrays herself as a student who ironically ends up teaching her own instructor the diamond twill technique.

The French ethnographer Michel Leiris published his African diary, L'Afrique fantôme, in the same year Spider Woman appeared. In addition to documenting the two-year Dakar-Djibouti expedition to the Dogon of Sanga and the Ethi opians of Gondar, together with the activities of various African subjects, Leiris revealed the strained relationships between the European members of the research team and the unethical museum-collecting procedures of the expedition. These revelations were to be the cause of his eventual permanent break with his colleague Marcel Griaule. He also disclosed the invasion of his own dream life by images arising directly from his fieldwork: "Suddenly, the smell of the herbs I've had scattered around my room enters my nostrils. Half dreaming. I have the sensation of a kind of swirling (as if reddening and turning
my head I were doing the gourri dance characteristic of trance) and I let out a scream. This time I'm really possessed" (Leiris 1934:358; English translation by Clifford 1986:44).13

In his introduction to The Nuer (1940), the British anthropologist E. E. Evans-Pritchard included a seven-page first-person confessional account of the terrible living conditions and informant difficulties he experienced during fieldwork in the Sudan. In sharp contrast, the remainder of the book, written in an omniscient third-person authoritative voice, describes highly abstract, nonempirical entities, such as lineage and age-set systems, and the idealized actions of common denominator people: the Nuer do this, the Nuer do that. During this same decade, Oliver La Farge, the American anthropologist and Pulitzer Prize-winning novelist, published a complex narrative ethnography, Santa Eulalia: The Religion of a Cuchumatán Indian Town (1947). In it, individuals are portrayed and actual events are described in detail. Liberally sprinkled with engaging first-person narrative vignettes, the text climaxes with a narrative of stolen idols and the placement of a death curse upon La Farge and his party. Although Evans-Pritchard and La Farge both place themselves within the ethnographic frame, their purposes and the resulting texts could not be more different. Evans-Pritchard's self-portrait serves to distance him from the Nuer, giving the appearance of objectivity; La Farge's self-portrait places him far enough within the Mayan world to reveal his subjectivity.

La Farge's humanistic stance toward the portrayal of the fieldwork encounter can also be found in two of Alice Marriott's books, The Valley Below (1949) and Greener Fields (1952). In the first of these ethnographic memoirs, which centers on a sojourn in northern New Mexico, Marriott (1949:239) explains her writing problem: "I started with the idea of an orderly description of a society that was blended of three elements: Indian, Spanish, and Anglo. I found that I couldn't describe the society without telling how we [her artist companion Martha and herself] came to be part of it. I couldn't analyze the people without describing them, and the description took the form of telling of the impact of their characters on ours." In her second memoir, Greener Fields, the chapters alternate between a highly accessible account of the history of anthropology and anecdotal narratives, ranging from the hilarious to the poignant, of her many years of fieldwork and close friendships with Plains and Southwestern Indians.

In spite of its appeal, first-person, experiential writing by ethnographers dealing with actual people and events was rare during the 1930s, 1940s, and early 1950s. Indeed, the Arctic explorer-ethnographer Jean Malaurie decided that the only way to change the situation was to promote this form of expression actively among his friends and colleagues (Balandier 1987:1). In 1955 he initiated a documentary literary series, Terre Humaine, with the Paris publisher Plon. The series was specifically dedicated to the publication of well-written, firsthand documentary testimony combining "scientific objectivity with a dialectic of personal relationships" (Malaurie 1987:10). Over five million copies of the fifty volumes published to date have been sold.
To start the literary series, Malaurie encouraged Claude Lévi-Strauss and Georges Balandier to write up their field experiences. *Tristes Tropiques* (Lévi-Strauss 1955, 1961) and *Afrique ambiguë* (Balandier 1957, 1966) resulted. Together with his own *Les derniers rois de Thulé* (Malaurie 1956, 1982), these were the first fieldwork accounts he published. Each of the three volumes was successful, reaching a wide, appreciative audience. By now, Malaurie’s book has been translated into sixteen languages. But the most renowned volume, and one that became an immediate best-seller in France, was Lévi-Strauss’s *Tristes Tropiques*, with its odd juxtaposition of a traveler’s tale, personal feelings, ethnographic observation, and abstract models. Although popular today, it was virtually ignored when it was first translated into English in 1961. Perhaps the English-speaking academic world was not yet comfortable with a first-person narrative account of fieldwork, or perhaps we were simply not ready for what Susan Sontag (1966) called “the anthropologist as hero.”

At almost the same time *Tristes Tropiques* came out, Georges Condominas (1957) published his Vietnamese field notebooks with Mercure de France, under the title of *Nous avons mangé la forêt de la Pierre-Génie Gôô*. His writing takes the form of a diary, listing and commenting at length on the events he witnessed in the village of Sar Luk from November 1948 to December 1949. In his introduction he comments, “I shall no doubt be reproached for alluding to my own presence at events I describe. But my purpose is not to paint an exotic canvas or to construct some sort of prehistoric ethnography. Rather, it is to render reality as it was lived while being observed” (Condominas 1977:xix). Instead of being reproached, the book was rapidly translated into Italian, German, and Russian. However, Condominas’s attempt to get the book translated and published in English is a bizarre tale of international copyright infringement. After years of unsuccessful publication attempts, he ran across a 1962 pirated English edition produced by the United States Department of Commerce, which, as he notes, “acted on political and military reasons rather than out of any deep scientific concern, and did so without consulting either me, the author, or the original French publisher” (Condominas 1977:xii). This pirated edition of a field diary that sympathetically portrayed the indigenous peoples of Vietnam was available neither to the anthropological profession nor to the general public during the period of the war in Vietnam.

From the 1960s to the present day, the relationship of fieldworkers to the people they study, to the political authorities and other powerful figures of the host community, and to themselves as observers, participants, and interpreters has been explored in depth in a series of edited volumes and in individual fieldwork accounts. When the Society for Applied Anthropology published Gerald Berreman’s *Behind Many Masks: Ethnography and Impression Management in a Himalayan Village* (1962), Robert Smith commented, in his foreword, that it was the “only attempt known to me to present within an analytical framework the subtleties of what the author calls ‘the human experience’ of field work” (in Berreman 1962:3). Using Erving Goffman’s (1959) interactionist approach to “impression management,” which involves a description of the
performances staged for the observer (including the behavior that goes into producing them, as well as the backstage situation which conceals them), Berreman analyzed the specific patterns of social interaction he found during his ethnographic fieldwork in a highly stratified North Indian village. Although only one other ethnographer (so far as I know) has utilized the social interactionist method so explicitly (Gregor 1977), Berreman’s monograph has been cited as an inspirational work by a number of ethnographers who have subsequently written personal accounts of their fieldwork interactions and experiences.

Earlier intimate accounts of fieldwork—such as Reichard’s Spider Woman (1934), La Farge’s *Santa Eulalia* (1947), and Marriott’s *Greener Fields* (1952)—had combined ethnographic information with accounts of the way this information was gathered. During the 1960s and 1970s, however, it became more common for such accounts to be kept separate from standard ethnographies. This segmentation, as in the earlier case of ethnographic novels, reveals a dualistic approach: public versus private, objective versus subjective realms of experience. Colin Turnbull, for example, published an accessible first-person account of his fieldwork among the Congo pygmies, *The Forest People* (1961), and then five years later released a coolly distanced, more authoritative monograph on the same topic, *Wayward Servants: The Two Worlds of the African Pygmies* (1965). John Beattie reversed this process, first publishing an “objective” ethnographic monograph based on his doctoral thesis, *Bunyoro: An African Kingdom* (1960). Five years later, at the suggestion of his editors, he wrote what he saw as a “subjective” first-person account, *Understanding an African Kingdom: Bunyoro* (1965). Although the book emphasizes overt methodologies—the use of assistants, informants, questionnaires, house-to-house surveys, note taking, photography, keeping a diary, and writing up the research—Beattie was nonetheless apologetic about being autobiographical or subjective, remarking in his preface on his “somewhat immodest undertaking.”

Following the publication of *Understanding an African Kingdom*, the Spindlers encouraged ethnographers who had already published monographs for their Holt, Rinehart and Winston Case Studies in Cultural Anthropology series to write methodologically oriented narrative accounts of their fieldwork. Eleven such essays, including one of their own, were incorporated in *Being an Anthropologist: Fieldwork in Eleven Cultures* (1970). This volume includes thumbnail biographical sketches of the authors, statements concerning why they had chosen anthropology as a career, and snapshots showing them in the field. These novel features add a more personal flavor to what are, for the most part, rather impersonal methodological statements. The photographs primarily serve to document the presence of the ethnographer at the ethnographic scene, but they also reveal that ethnographers enjoy representing themselves as fieldworkers. We see Alan Beals getting his hair cut native-style in Gopalpur, India; Robert Dentan burning the fur off a monkey in Malaysia; John Hostetler praying over a meal in a Hutterite apartment and his colleague, Gertrude Huntington, dressed as a Hutterite woman pushing a Hutterite baby in a pram;
John Hitchcock working up his notes in a headman’s goat shed in Nepal; and Louise Spindler sitting on a step with a Menomini woman who is taking a Rorschach test.

Unlike these authors, British anthropologist Nigel Barley released his ethnographic monograph and fieldwork account simultaneously. The monograph is a Lévi-Straussian structuralist study, Symbolic Structures: An Exploration of the Culture of the Dowayos (1983b), and the field account is a funny, warts-and-all, first-person narrative of his African field research, Adventures in a Mud Hut: An Innocent Anthropologist Abroad (1983a). The remarkable difference in tone, tenor, and material presented in these two books reveals Barley’s discomfort with representing himself in an ethnographic account. In Adventures in a Mud Hut, he comes off as a silly, sad, incompetent, and even slapstick character who marched, limped, and finally was carried through his initiatory field research, and the natives also come off as clowns. Foolish old men stare at the photographs of lions and leopards he uses in order to elicit information on local fauna, turning them in all directions and saying things like, “I do not know this man” (Barley 1983a:96). This sounds like an African variant on that old anthropological story about Polynesians who weren’t able to interpret photographs at all. In general, what purports to be a personal narrative ends up as a lampoon of the entire ethnographic enterprise.

Discomfort with the act of self-representation, within a serious ethnography, can also be detected in Paul Rabinow’s first-person memoir, Reflections on Fieldwork in Morocco (1977), where he refers to his previous monograph, Symbolic Domination (1975), as a “more traditionally anthropological treatment of the same data” (Rabinow 1977:7). Jean-Paul Dumont, like Rabinow, published his standard ethnographic monograph, Under the Rainbow (1976), before his first-person fieldwork account, The Headman and I (1978), but he displays a rather different attitude towards including himself in his ethnography. For him the work of self-representation is neither laughable nor any less “traditional” than the standard ethnographic representation of the Other. In fact, as Peter Rivière (1980) has pointed out, we learn rather more about the Panaré in Dumont’s first-person account than we do in his monograph. Part of the reason for this is that Dumont self-consciously centered The Headman and I around the serious question of who he was for the Panaré, rather than who the Panaré were for him, the latter being the implicit question most ethnographies explore. With this change in focus comes the subtle shift of genre from the ethnographic memoir toward narrative ethnography.

In the ethnographic memoir, an author takes us back to a corner of his or her life in the field that was unusually vivid, full of affect, or framed by unique events. By narrowing the lens, these authors provide a window into their personal lives in the field, a focus which would not be possible in a full-length autobiography. The author of a narrative ethnography also deals with experiences, but along with these come ethnographic data, epistemological reflections on fieldwork participation, and cultural analysis. The world, in a narrative ethnography, is re-presented as perceived by a situated narrator, who is also
present as a character in the story that reveals his own personality. This enables the reader to identify the consciousness which has selected and shaped the experiences within the text. In contrast to memoirs, narrative ethnographies focus not on the ethnographer herself, but rather on the character and process of the ethnographic dialogue or encounter.

Two key essays centering on the exploration of the ethnographic encounter were published by Stanley Diamond and Kurt Wolff in a volume entitled _Reflections on Community Studies_ edited by Vidich, Bensman, and Stein (1964). Diamond’s essay, “Nigerian Discovery: The Politics of Field Work,” subtly explores the complex political dimensions involved in crossing cultural boundaries. Kurt Wolff’s “Surrender and Community Study: The Study of Loma” describes how, during his field research in a northern New Mexico village, he opened himself to the risk of being hurt by becoming so totally involved and identified with the community that everything he saw or experienced became relevant to him. “It was years before I understood what had happened to me: I had fallen through the web of culture patterns and asserted conceptual meshes into the chaos of love; I was looking everywhere, famished, with a ruthless glance” (Wolff 1964:235).

It is precisely the vulnerability revealed by Wolff that Kevin Dwyer (1982:272–74) sees as the central anthropological project. No matter how much care an ethnographer devotes to his or her project, its success depends upon more than individual effort. It is tied to outside social forces including an anthropological community that accepts the project as meaningful and international relationships that make the fieldwork possible. In Dwyer’s view, the issues of the fieldwork endeavor are not so much objectivity, neutrality, and distance as they are risk, the possibility of failure, and the hope of success.

Speaking pedagogically, firsthand accounts are useful in preparing ethnographers for fieldwork. Their value in detailing the complex and ambiguous realities involved in the fieldwork experience was formally recognized by the University of Amsterdam in the early 1960s, when the Institute of Cultural Anthropology set up a series of formal lectures for anthropologists recently returned from the field. An edited volume of these lectures, _Anthropologists in the Field_, was compiled by Jongmans and Gutkind (1967), together with four previously published essays and an annotated bibliography on field methods. While some personal aspects of field experience appear in several of the contributions, the main thrust of the volume is the description and evaluation of various field techniques—social survey, quantification, restudy—rather than an in-depth exploration of the subjective elements of fieldwork.

Beginning in the 1970s, there was a shift in emphasis from participant observation to the observation of participation. Martin Yang (1972) wrote an important essay discussing the role of both his graduate education and what he called his “first-hand fieldwork” in the production of his highly acclaimed ethnography, _A Chinese Village_ (1945). His field research was done in the village in which he grew up and lived until he went away to college. As he put it, “My fieldwork was my own life and the lives of others in which I had an
active part" (Yang 1972:63). This type of ethnographic experience has been called both "ethno-sociology" and "auto-ethnography" (see Hayano 1979). Yang was by no means the first indigenous anthropologist to publish an ethnography about his own group—Jomo Kenyatta (1938), Fei Hsiao Tung (1939), and Chie Nakane (1970), among others, had already done so—but he was unique, at that time, in writing a self-reflexive essay about the experience of doing so.¹⁶

During the 1980s more first-person experiential fieldwork accounts, including auto-ethnographies, appeared than during the previous two decades.¹⁷ These volumes clearly reveal the continued movement from participant observation to the observation of participation. What was only a trickle in the 1930s grew into a stream of confessional accounts by the 1960s and became a swollen river of self-revelatory celebration by the 1980s. The exploration of the process of producing ethnographic information and publishing ethnographic accounts had turned toward the political, philosophical, and poetic implications of such work.¹⁸ A critical literature simultaneously sprang up. The process of self-examination led to the examination of other ethnographers' selves, and the eye shifted from the ethnos in ethnography to the graphia—the process of writing.¹⁹

A number of fieldworkers are currently at work on book-length narrative ethnographies, combining ethnographic information with a dialectic of personal involvement. Meanwhile, edited volumes focused on specific aspects of fieldwork continue to appear. Fieldworker identity is the focal point of Fieldwork: The Human Experience (Lawless, Sutlive, and Zamora 1983). The impact of an ethnographer's sex and gender identity on fieldwork and the effect of fieldwork on an ethnographer's view of gender self-identity are the central topics in Self, Sex, and Gender in Cross-Cultural Fieldwork (Whitehead and Conaway 1986). In Arab Women in the Field: Studying Your Own Society (Altorki and El-Solh 1988), the roles of gender and indigenous status in the fieldwork experiences of Arab women ethnographers are explored. Most recently, the mishaps, pratfalls, and lessons learned while doing fieldwork are the main topic in The Humbled Anthropologist: Tales from the Pacific (De Vita 1990).

What explains the shift in ethnography toward representing ourselves in the act of engaging with and writing about our selves in interaction with other selves? In part, the change reflects today's general intellectual climate of epistemological doubt. Another factor in the development of reflexivity and alternative styles of ethnographic representation has been the notable growth in the prestige of anthropology as a discipline and in the size of the audience for the anthropological perspective. Interest began to increase during the 1960s and accelerated throughout the 1980s. Not only have we captured the attention of the general public, thanks in part to Margaret Mead and public television programs such as "Nova" and "Smithsonian World," but we have also become the darling of both the humanities and the social sciences. As our current public guru, Clifford Geertz (1985), has recently pointed out, our prestige in fields such as history, philosophy, literary criticism, theory, law, political science, sociology, psychology, and economics has never been higher.

In an enlarged marketplace for anthropological ideas, ethnographers have
been pursued by publishers for interesting ideas about humankind and fascinating stories. Some publishers have even been willing to read dissertations and mentor the rewriting process. When Barbara Myerhoff turned her dissertation into the book *Peyote Hunt* (1974), her editor insisted that she insert herself and her observations into the manuscript. Rewrite the text top down was the suggestion; use an active personal voice. Quite pleased with the results, Myerhoff notes that “I found I had written a book I trusted more, that was clearer and more reliable” (Myerhoff and Ruby 1982:33).

There have also been notable changes in the population of individuals electing to become ethnographers—in terms of gender (more women), class (more from middle- and lower-class backgrounds), and ethnicity (more third- and fourth-world scholars). These transformations have spurred a new critical awareness and a radical democratization of knowledge resulting in the suggestion that the class, race, culture, and gender beliefs and behaviors of the inquirer be placed within the same historical moment, or critical plane, as the subjects of inquiry (Harding 1987:9; Rose 1990:10). Another key alteration in anthropology has been an emphasis on research as a fundamentally action-oriented endeavor. Sherry Ortner, in her review essay “Theory in Anthropology since the Sixties,” notes that there has been a growing interest in such interrelated terms as practice, praxis, action, interaction, activity, performance, and experience, together with agent, actor, person, self, individual, and/or the subject of the action (Ortner 1984:144).

Major transformations in anthropology have also come about because of the emergence of a highly articulate population of “native” ethnographers from the third and fourth worlds, including various bicultural inside/outsiders. While it is undoubtedly true that insiders may have easier access to certain types of information, especially in the area of daily routines (Jones 1970; Ohnuki-Tierney 1984), native anthropology can also be distinguished from indigenous anthropology in that native ethnographers are those who have their origins in non-European or non-Western cultures and who share a history of colonialism, or an economic relationship based upon subordination (McClaurin-Allen 1989:18–24). Just as being born female does not automatically result in “feminist” consciousness, being born an ethnic minority does not automatically result in “native” consciousness. Native ethnographers have critiqued the discipline—for example, the strange preoccupation with issues such as caste in South Asia (Daniel 1984; Appadurai 1986)—and have worked to bridge the gulf between Self and Other by revealing both parties as vulnerable experiencing subjects, working to coproduce knowledge. They have argued that the observer and the observed are not entirely separate categories. To them, theory is not a transparent, culture-free zone, not a duty-free intellectual marketplace hovering between cultures, lacking all connection to embodied lived experience. They believe that both knowledge and experience from outside fieldwork should be brought into our narratives and that we should demonstrate how ideas matter to us, bridging the gap between our narrow academic world and our
wide cultural experiences. These strategies should help us simultaneously deepen and invigorate our writing and our selves. 20

As Vietnamese author and cinematographer Trinh Minh-ha (1989:76) has written, “In writing close to the other of the other, I can only choose to maintain a self-reflexively critical relationship toward the material, a relationship that defines both the subject written and the writing subject, undoing the I while asking ‘what do I want to know you or me?’” Or, as Jean-Paul Dumont (1978:200) said at the close of his narrative ethnography, “Who was I for them?”

The movement from ethnographic memoir to narrative ethnography has gone unanalyzed within the rapidly growing meta-anthropological literature. This omission has occurred, I think, because so much emphasis has been placed on rhetorical strategies, while substantially less attention has been given to the widening of the audience for ethnography. If today’s ethnographers are writing not only for various academic audiences (area specialists, members of other disciplines, and students) but also for the educated public, including members of their host communities, then they are no longer in a position to write as if they themselves were the only active parties in cross-cultural exchanges. Not only did Edward Said’s (1978) discussion of Orientalism reveal this form of exotic portraiture to be unacceptably neocolonialist, but a local audience would also know it to be a blatant falsehood.

Just as writing for and about the feminist movement by feminist scholars has provided a dynamic public sphere within which ideas can be discussed in both a politically and personally engaging manner, so writing for and about the ethnographic community in which one has lived and worked at length should produce engaged writing centering on the ongoing dialectical political-personal relationship between Self and Other. The likelihood that an ethnographer might unselfconsciously take center stage when representing the fieldwork experience is, I think, in direct proportion to the spatial, temporal, and cultural distance of this individual from the host community at the time of such representation. The farther away, longer ago, and more culturally Other the field experience was, the more probable that the authorial figure will be dominant, while members of the host community will shrink into the background. The self-conscious shift away from writing a memoir of the field experience, in which the author is the only developed character, towards writing a narrative ethnography, in which the author purposely becomes a secondary character, alleviates this problem of foregrounding and backgrounding.

This sea change in ethnographic representation of both the Self and Other within a single text has been lumped together with other writing strategies and labeled as part of a mythic “experimental moment in the human sciences” (Marcus and Fischer 1986:165). However, as Renato Rosaldo (1989:231) has indicated, such an analysis embodies a facile application of Thomas Kuhn’s notion of experimentalism as occurring only during scientific paradigm shifts. Rather than describing the ongoing refiguration of social thought and practice
as part of an experimental moment (i.e., fad, or flash in the pan), it ought to be recognized for what it is, a change in ethnographic epistemology embodying key ethical and analytical issues that has already produced a major body of work. The creation and subsequent celebration of a fictive “experimental moment” has produced an easy target for unsympathetic critics, who gleefully dismiss the serious attempt to represent both Self and Other within a field account as nothing but an apolitical solipsistic exercise in exploring the Other merely to find the Self (Polier and Roseberry 1989). It is to be hoped that critiques of this sort, which reduce a long-term dialectical negotiation between the Self and Other to nothing but a dualistic caricature of such an encounter, will not delay the further development of a productive dialogue between persons of differing cultural backgrounds and political situations. Ethnographers may be able to overcome this problem if they follow the lead of feminist critical theory, which, when it denies the split between epistemology and politics, is simultaneously reflexive and political.

However the discipline may develop historically, there currently exists a new breed of ethnographer who is passionately interested in the coproduction of ethnographic knowledge, created and represented in the only way it can be, within an interactive Self/Other dialogue. These new ethnographers—many of whom are themselves subaltern because of their class, gender, or ethnicity—cannot be neatly tucked away or pigeonholed within any of the four historical archetypes I enumerated at the outset of this essay: the amateur observer, the armchair anthropologist, the professional ethnographer, or the “gone native” fieldworker. Rather they, or we, combine elements from all four of these categories. Thus, for example, we embrace the designation “amateur,” since it derives from the Latin amatus, the past participle of amare, “to love,” and we are passionately engaged with our endeavor. We accept the “armchair” designation because we have a serious concern with both reading and critiquing the work of other ethnographers in order to try to change past colonialist practices. We insist that we are “professional,” because of the seriousness of our field preparation and engagement, and also because of our attention to issues of representation in our own work. Finally, to the extent that fieldwork is not simply a union card but the center of our intellectual and emotional lives, we are, if not “going native,” at least becoming bicultural.

NOTES

1. I began to formulate the perspective presented here during my National Endowment for the Humanities Fellowship at the Institute for Advanced Study in Princeton, New Jersey (1986–1987). Sections of this essay were given orally before the anthropology faculties of the University of Michigan at Ann Arbor (1989), McMaster University in Hamilton, Ontario (1989), and the University of Wisconsin at Madison (1990). To the audience members, and most especially my generous hosts and primary interlocutors—Clifford Geertz, Sherry Ortner, Bruce Mannheim, Ruth Behar, Ellen Badone, and Kirin Narayan—I extend my sincere thanks for many perceptive questions, comments, and suggestions.
2. Even though Bronislaw Malinowski took on the archetypal role of “the Ethnographer” in the opening chapter of his *Argonauts of the Western Pacific* (1922) and claimed to have invented the method of fieldwork, there are other candidates for this honor, including Morgan, Cushing, Haddon, Seligman, Boas, Rivers, and Radcliffe-Brown. As Raymond Firth (1985) has pointed out, Malinowski’s innovation lay in elevating the fieldwork method into a theory. For a brief history of fieldwork, see Rosalie Wax (1971:21–41). George Stocking opened his “History of Anthropology” series with a collection of superb articles entitled *Observers Observed: Essays on Ethnographic Fieldwork* (1983b).

3. The French fieldwork tradition was initiated during the 1920s by Marcel Mauss and Maurice Delafosse, who, together with Lévy-Bruhl and River, founded the Institut d’Éthnologie, where many Africanist colonial officers studied ethnographic methods. Marcel Mauss, although he never undertook fieldwork, taught an annual course, “Ethnographie descriptive,” and an important fieldwork text, *Manuel d’ethnographie* (1947), was developed from his notes. Mauss recommended that the “professional ethnographer” adopt the “intensive method,” by which he did not mean long-term individual experiential research, but rather multifaceted documentary team research, resulting in hundreds of sound recordings, textual accounts, and major collections of art and artifacts (Clifford 1983).

4. Although I have grouped the British and American traditions together here, there are substantial differences between and within them. British anthropologists specialized in relatively isolated “primitive” peoples and focused on social anthropology, emphasizing social structures. American ethnographers studied less isolated peasant groups, were more inclusive, and focused on culture and psychology (Kirsch 1982:103).

5. For further discussion of the emergence of fieldwork, together with holism, relativism, and the comparative approach as key methodological values in the United States and Britain, see David Mandelbaum (1982:36) and George Stocking (1982:411–12, 1983a:74).

6. For more on the importance of communicative interaction through language, especially dialogue, see Johannes Fabian (1971) and Dennis Tedlock (1979, 1987).

7. That the participant-observation form of the fieldwork ritual is still considered necessary for membership in the ethnographic tribe is revealed in a recent review of James Clifford’s *The Predicament of Culture* (1988), in which Philip Bock (1990:109–10) invites Clifford “to undergo the ritual ordeal of fieldwork (not just as an observer in a Mashpee, Massachusetts courtroom)” in order to become “one of us.”

8. One reviewer of Dalby’s book took exception to the author’s claim of having become a geisha (Cornell 1986) and questioned her overall reliability as a witness. The objection was not to the fieldworker’s attempt to become a geisha, but rather to her assertion that she was successful. This effort at discrediting a fieldworker who claims to have undergone a successful intercultural apprenticeship demonstrates the persistence of the Western intellectual construction of Self and Other as two entirely incommensurable categories.

9. More information about Nimuendajú can be found in his obituary in the *American Anthropologist* (Baldus 1946) and Lowe’s (1959:119–26) discussion of his long-distance relationship with him. Assessments of Frank Hamilton Cushing’s fabled induction into Zuni culture can be found in Jesse Green’s (1979) introduction to his edited volume of Cushing essays and in Sylvia Gronewold’s (1972:33–50) essay “Did Frank Hamilton Cushing Go Native?” For more on Verrier Elwin’s life, see his autobiography (1964); a perceptive discussion and evaluation of his ethnographic work can be found in Misra (1973).

11. Buelow (1973) found that most anthropologists who have written fiction have used pseudonyms. Nancy Schmidt (1981:12) later noted that “fieldwork experiences were not an acceptable topic for anthropologists to write about, so Bohannan used a pseudonym for her novel.” She also reported that few anthropologists to date have openly acknowledged their literary pseudonyms. As she put it, “ethnographic fiction has been hidden, or perhaps more accurately swept under the rug, by anthropologists who felt that it was inappropriate for consideration as ethnography,” and “editors of anthropological journals have . . . refused to consider it for review” (Schmidt 1984:11, 12).

12. Devereux (1967:41–42) has argued that this type of anxiety-reducing device can easily be transformed into countertransference reactions, leading to “acting out” masquerading as science. He also noted that “the elimination of the individual from ethnological field reports was formerly a routine procedure. Linton once quoted to me the following remark of a colleague: ‘My monograph about the X-tribe is almost finished. All I have to do now is cut out the life’ (i.e., all references to real people and events)” (Devereux 1967:89).

13. Leiris’s work, as Clifford (1986) and Beaujour (1987) have pointed out, should be read as both an encouragement and a cautionary example for the project of ethnographic representation. Leiris faced “epistemological aporias in his effort to combine sympathetic observation of the Other, the unavoidability of literary self-inscription, and the imperative of cultural critique” (Beaujour 1987:479).


15. Perhaps the most striking example of this radical split is found in Malinowski’s
Experience:

Jones writing, with his various ethnographic monographs (1922, 1935) on one side and his controversial, posthumously published diary (1967) on the other.

16. In ordinary "reflectiveness," one is conscious of oneself as an Other, but in "reflexivity," one is conscious of being self-conscious of oneself as an Other. For a thorough discussion of the difference between these two concepts, see Babcock (1980). Béteille and Madan published a collection of reflexive field accounts, *Encounter and Experience: Personal Accounts of Fieldwork*, in 1975. The authors consisted of Indian anthropologists who had worked in India, Indians who had researched other cultures, and non-Indians who had done intensive fieldwork in India. Four years later, Srinivas, Shah, and Ramaswamy published *The Fieldworker and the Field* (1979), a set of eighteen new essays by Indian social anthropologists and sociologists.


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