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# Sows' Ears and Silver Linings

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## A Backward Look at Ethnography<sup>1</sup>

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by Sidney W. Mintz

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Globalization, changing views of science, and alterations in the culture concept have gradually modified the way fieldwork is perceived within anthropology. This paper briefly examines the contributions of four ethnographers in order to argue that ethnography based on fieldwork remains essential to our definition as a profession. The claim is made here that unless anthropologists continue to make fieldwork central, anthropological theory will be cut off from its grounding in data. Other observers, less skilled than anthropologists but more daring, will increasingly supplant ethnographers in gathering new information that they then interpret and in claiming the public readership that anthropologists were once able successfully to address.

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Fieldwork as done by cultural and social anthropologists is a common subject of reflection and reminiscence among them. In recent years, as anthropologists were growing more concerned with the history of their own discipline and of its practitioners (as demonstrated, for example, in the splendid achievements of George Stocking and his collaborators), fieldwork became an even more important subject of discussion.

But fieldwork merits additional attention now because the place of ethnography itself has changed measurably in recent decades. That we continue to carry out fieldwork, to write about it, and at times to teach it shows that it is still viewed by many as vital to our profession. The flood of books and articles about fieldwork confirms its persisting importance to most of us, and we continue our work in the field with living peoples. Yet we do so now under changed and rapidly changing conditions.

I think that three massive happenings helped to usher in a new era for anthropological fieldwork. The first has been the profound change in relationships among societies worldwide, such that people, capital, goods, and ideas (including ideas *about* people, capital, and goods and media for the transmission of such ideas) now move with greater frequency and velocity across political boundaries than ever before in history. Among many more important consequences, this change has profoundly affected traditional classifications or typologies of human societies. By blurring the boundaries once thought to separate these societies from each other and by calling into question all of the familiar criteria of social classification, this change has emptied such categories as "Western/non-Western," "developed/underdeveloped," "primitive/civilized," and like polarities of even their residual meanings.

The second change, which has come about for complicated reasons that I believe we do not yet wholly grasp, has been a fundamental questioning of the epistemological status of all of the disciplines wherein the observer is part of the field being observed—anthropology clearly being one such. This questioning, some of it by nonanthropologists but much of it within the discipline, has gone a long way toward nullifying the supposed authority of the observer, perhaps particularly of the professional observer, such as the anthropologist. In contrast, the reporter—as well as the TV anchorman, the talk-show host and talking head, the photographer, the artist, the critic, the poet, the novelist, and the *paparazzo*—have now acquired much greater relative authority.<sup>2</sup>

The third such process has been an ever-widening currency in the use of the term "culture," with an accompanying vulgarization of its meanings, perhaps similar to the absorption of Freudian concepts in demotic form into the language and thinking of lay persons half a century or so ago. During the history of the profession, an-

1. This article was the American Anthropological Association Distinguished Lecture for 1996. I thank Eytan Bercovitch, Richard Fox, Carla Freeman, William Merrill, Jacqueline Mintz, and Joel Robbins for valuable help and advice and the late Eric Wolf for encouragement. I only regret that I could not comply with all of their wishes.

2. This decline in the perceived authority of the individual ethnographer has apparently been accompanied by a growing belief that what is to be reported is now far less knowable and clear than it was once thought to be, such that *no* report on it can be accorded much credibility. I do not attempt to address this phenomenon here.

thropologists did not always agree upon the meaning of the term "culture." But today many anthropologists hardly bother to try to define it or to state the problems connected to its definition, even for their students or for each other. This near-abandonment of the concept has been paralleled by a wholesale (and, I think, somewhat opportunistic) adoption of some of anthropology's ideas and tools by nonanthropologists, who use them for their own purposes, new and old.

Anthropological fieldwork, or ethnography, had developed as an alternative to the construction of theories about humankind by imagining the nature or intentions of other humans without a firm basis in data collected from living peoples. Going to the field helped to replace a certain amount of speculation about what "primitive people" were like. It was much more advantageous than the collection of data through appeals to missionaries, government officials, traders, soldiers, and others who might find themselves among distant and remote peoples—though of course those data also proved enormously useful.

Though at first everyone may have been equally adept, over time anthropology developed a tool kit of techniques, of methods, for the doing of fieldwork. Lengthy experience, many failures, and some successes resulted in the accumulation of real skills. There is no doubt that there came a time when anthropologists could clearly do much better what they did than could others without those skills, that training, and the focus that having data collection as an objective provides.

Still, many anthropological scholars today think of fieldwork as being merely part of our methodology and for that reason are loath to give it decisive importance in defining the discipline of anthropology. Yet the fieldwork method is closely tied to the data themselves. "Genuine science," Alfred Kroeber has written, "is characterized first of all by its *method*, only secondarily by its subject matter, except that this must be in nature and must consist of phenomena" (Kroeber 1948:296, italics added). Without fieldwork, ethnographers would have had to depend on the fieldwork of others (and their published and manuscript results) or on speculation to have a basis upon which to formulate or "test" theoretical propositions. To be sure, fieldwork is "only fieldwork"; but it is for nearly every purpose the best means of access we have to the data we need. The interdependence among data, method, and theory are inescapable in the work of anthropologists, even though we must keep on acknowledging that we are mere humans, observing other humans.

In a well-known article on ethnography written three decades ago, Harold Conklin (1968:175) notes:

Because ethnographers interact personally and socially with informants, they find themselves carrying on a unique type of natural history, in which the observer becomes a part of (and an active participant in) the observed universe. The extent of this involvement and its importance for ethnographic recording depend on many situational considerations,

including the personalities of the ethnographer and his informants. In some types of field inquiry the ethnographer's practical success or failure may depend as much on those impressions he makes locally as on the cultural events being observed.

It had long been recognized by anthropologists that the data were no better than their collectors and that sometimes the data collection hinged on the personalities of ethnographer and "informant." But given the nature of our discipline, there is—perhaps unfortunately—no non-human (machine) route to the data.

It also bears noting that ethnography was a professional activity that first grew out of observations of the sort that human beings have doubtless been making about each other since the beginnings of society. While both the specific methodology and the principles, practical and ethical, of ethnography were developed within the discipline, there certainly were people who did much of what ethnographers do *before* there was ethnography. As I suggested earlier, the basic skills that ethnography requires had to be described, objectified, and given form even before anthropologists could claim them as their own. Indeed, for purposes of argument it could be claimed that the craft of ethnography is separable from anthropology as such, as if it were an independent undertaking. But while that is definitely not in my view what ethnography *should* be, in the light of current trends I think it is what ethnography may yet become. I mean to keep the distinction in mind, because the existence of ethnographic practitioners without ethnography—that is, of persons who do *approximately* what ethnographers do without anthropological training—has always been with us; it is doubtless with us now.

Before the results of fieldwork can be written up to become articles, monographs, or books, the fieldwork itself must be done, to produce data, usually at first in the form of field notes. Fieldwork became what anthropologists did even before Boas and Malinowski and even before it was called ethnography. In spite of the hyperbole, C. G. Seligman was making a fair point when he said that "field research in anthropology is what the blood of the martyrs is to the Church" (Stocking 1995: 115). "The field" was where we went to collect observations, artifacts, texts, and fossil materials.

It was Alfred Kroeber who suggested that anthropology was dedicated to the study of what were called "primitive" peoples not out of any scientific preference for such peoples but simply because none of the other social sciences was prepared to take them seriously (Kroeber 1953: 13). At no point, he argues, did anthropology renounce its interest in all peoples, "primitive" or not. For the most part anthropology was prideful, perhaps even a little petulant, about its choice of subject matter. Yet while glorying in its research on nonliterate societies, anthropology did not cede the study of other sorts of humanity to its sister disciplines.

Occasional studies of human beings other than "primitives" were done by anthropologists at an earlier time,

but only since around 1950 has there been a matter-of-fact acknowledgment that the study of so-called non-primitives might be just as valid anthropologically as the discipline's traditional focus. At the time, for instance, that M. G. Smith went to Carriacou, R. T. Smith and Chandra Jayawardena to then-British Guiana, and Julian Steward's students to Puerto Rico, soon after the end of World War II, such Caribbean research was still considered by many as not quite authentic anthropology. There was an implicit scale of values at work that favored the study of "primitive" peoples, and long after World War II research money, job opportunities, and even professional admiration were modulated by feelings that true anthropology was possible only with "real primitive people" (Mintz 1994). Anthropologists today do not know with what occult fierceness the desire to have "one's people" viewed as isolated and, hence, *legitimate* subjects of study was defended half a century ago.

With the passing of time, of course, more and more anthropologists have worked outside the so-called primitive world; they have also redefined anthropology to make that sort of once-cherished division less relevant.<sup>3</sup> But as that happened the place of fieldwork came to be seen somewhat differently and perhaps even as somehow less important to the development of anthropological theory. I would like to argue against the implications of that trend.

In order to say something here about the practice of ethnographic fieldwork, I want to draw upon the oeuvres and personal histories of four ethnographers. I have chosen them only as illustrative and according to my own judgment of the field. But I must observe that at least one of them did ethnography before there could be said to have been any ethnography, professionally speaking. Three of them did ethnography without any professional training in ethnography. One of them is remembered not as an ethnographer but as what used to be called an "informant"—though there is no doubt, in my mind at least, that he was an ethnographer as well. Since I am writing about ethnographic fieldwork, the seeming lack of professional qualification among some of the subjects is a relevant datum.

Of the four ethnographers I have in mind, only one, Audrey Isabel Richards, seems to me to fall squarely within any contemporary professional definition of an ethnographer. The other three are the ethnographer of the Ghost Dance religion, James Mooney; the ethnographer of the Ifugao people of the Philippines, Roy Franklin Barton; and the Puerto Rican sugarcane worker Anastacio Zayas Alvarado, for whose life history I was privileged to be the amanuensis. I would like to say a little about each of them.

3. That this development, positive in many ways, has at the same time seriously undercut evolutionary theory may prove unfortunate. I cannot consider it here.

## Four Ethnographers

JAMES MOONEY (1861–1921)

James Mooney is best known for his work on the Ghost Dance, but that work came toward the end of an illustrious career. Mooney was born in Indiana in 1861, of Irish immigrant parents. He became interested in American Indians while still a youth and had aspirations, never fulfilled, of one day studying the Indians of Brazil. He was interested in the yearnings of peoples for political independence and self-definition and concerned about Ireland's future; he was also interested in socialism.

Mooney was in his twenties when he came to know Major John Wesley Powell. Powell, who had just become the chief of the newly created Bureau of Ethnology, was favorably impressed by Mooney. He let him work for a year as a volunteer (if he was paid at all, it was very little, and irregularly) and then offered him a post in the Bureau. During the ensuing 36 years, Mooney's life became the study of American Indian peoples.

His *The Ghost Dance Religion and the Sioux Outbreak of 1890* (Mooney 1896) is one of the greatest studies of an American Indian religion ever written—perhaps the greatest such. It merits study still. One finds here a convincing account of the nature of faith and of the political and social circumstances that engendered in this instance its distinctive and moving manifestations.

To give but one example, while researching the Ghost Dance Mooney talked with Wovoka, the Walker Lake Paiute prophet. He was the only white observer interested in the dance who seems to have thought of doing so. Coming back from that visit, he stopped again to see the Cheyennes and the Arapahos, and he carried to them magpie feathers that Wovoka had given him. These were viewed as sacred by the Cheyennes and Arapahos because they had come from the messiah and because they symbolized the gift to be bestowed on faithful ghost dancers, who would be able to fly above the earth when its destruction came. Excited by Mooney's revisit, we are told, the Indians touched his clothing, for he himself had touched Wovoka. For those people, Mooney really was what his biographer calls him—the "Indian man" (Moses 1984).

Mooney's role in Bureau activities was quite distinctive, it seems. When hostile Indian acts occurred, Bureau personnel such as Mooney were expected to explain *why* they had happened. We are told that in this connection Mooney was exceptional; his explanations took into account what he had learned and thought about the Indians as the outcome of the long-term basic research he had carried out in their midst and always with their help. Joan Vincent has provided us with an astute account of Mooney's role not only as ethnographer of the Ghost Dance religion but also as interlocutor and advocate for the people with whom he worked. "To an increasingly politicized dialogue," Vincent writes, "James Mooney brought political realism, first by reporting the political views of the Native Americans themselves and then by producing a narrative and through it an explanation of

what was occurring" (Vincent 1990:53). Of course neither the narrative nor the explanation is (or ought to be) the final word, but I think that most serious students of the Ghost Dance would agree that so far no one has done it better.

Mooney's life as a Bureau ethnographer was stormy, largely because he thought that what he knew and the findings of his research were relevant to policy; he clearly and courageously expressed his views on the issues of the day. His obituary in the *American Anthropologist*, written by John Swanton (1922:210–11), contains a remarkable passage apposite to the point I am trying to make here:

Mr. Mooney's attitude toward the subjects of his study was not merely scientific. He took an intense personal interest in them, was always ready to listen to their troubles, to lay their difficulties before those who might be able to adjust them, and to spend time and money in aiding them to obtain any and all advantages which he believed to be their just due. When he had once reached a conclusion he maintained it with unfaltering courage and clung to it with a tenacity which not infrequently seemed to his friends to be carried to extremes, but of the honesty of his intentions there could be no doubt. This attitude was oftenest in evidence in defense of a subjugated race or an oppressed class, for which the circumstances of his ancestry were no doubt largely responsible. But beneath all was an intense emotional attitude which was a part of himself and was the secret both of his success as an ethnologist and his influence as a man. From this particular point of view he has had few equals among ethnologists and certainly no superiors.

In consequence of it he had a wide acquaintance among peoples other than the Indians and those of his own race and among classes other than that to which he naturally belonged.

These comments by a friend suggest how unusual Mooney must have been in the company of other ethnographers and how strange his beliefs and passions, perhaps particularly those that grew from his ethnic and class background, must have seemed to them.

In his biography of Mooney, L. G. Moses (1984:179–221) provides us with a detailed account of his persecution by some of his contemporaries. It happened most of all because Mooney defended the rights of the Indians to use peyote in their religious ceremonies. At considerable risk to his own career, he had appeared before a Congressional committee to say so. So impassioned and committed was Mooney's testimony that, as Moses makes painfully clear, the anti-peyote movement was soon transformed into an anti-Mooney movement. It also seems likely that many anthropological colleagues thoroughly disliked Mooney because he was one of the very few ethnologists in the United States who chose to defend Boas after Boas had criticized the government's employment of spies masquerading as anthropologists

during World War I.<sup>4</sup> Boas and Mooney had not in fact been friendly, but Mooney thought that Boas was right in this matter and that his critics were unethical as well as wrong.

His independent, maverick, and honest turn of mind made Mooney few friends. Yet it was especially because he came to be viewed as an intemperate ally of the Indians that his work was fatally interrupted and he was effectively prevented from continuing his fieldwork among them.<sup>5</sup> From 1918 on, Mooney was expressly denied permission to pursue his research among the Indians with whom he had worked and lived and whom he had grown to love. He spent those final years, seriously ill, his salary cut (for no reason but sheer vindictiveness), his career cruelly shortened. Those were bitter rewards for his political honesty and courage. Mooney died, a broken man, in 1921.

Mooney's treatment during his final years was reasonable cause for self-pity on his part, but he wasted little time feeling sorry for himself. In his letter to Dr. Frederick Smith, dated May 18, 1920 (cited in Moses 1984:217), he writes:

My most important investigation, which promises to be of the most value to the scientific and medical world, a research which I initiated and to which I have given a large part of thirty years, is blocked and killed, and I am debarred from the field . . . for declaring the scientific truth and defending the freedom of religion of our citizen Indians as guaranteed under charter and incorporation of the state of Oklahoma.

But Mooney's disappointments had to do with the work that he was prevented from completing even more than with the people who did this to him.

Talking with a reporter in 1893, while still a young man, Mooney had said, "Unless you live with people you cannot know them. It is the only way to learn their ideas and study their character" (Moses 1984:219). He was making an observation about fieldwork that reveals, it seems to me, what Swanton referred to as the "intense emotional attitude which was a part of himself and . . . the secret both of his success as an ethnologist and his influence as a man." Mooney's importance inheres in the quality of his fieldwork, in his identification with his Indian friends, and, I think, in the consistency he forged between his way of working and his way of life. Mooney thought that anthropology was about finding things out in the field, using that learning to build a theoretical position, and then telling other people about the findings in convincing ways in the hope of effecting

4. John Swanton seems to have been the only other public defender of Boas at the time.

5. William Merrill told this story eloquently in the paper he presented at the special session celebrating the Smithsonian's 150th year at the 1996 annual meetings of the American Anthropological Association.

understanding and perhaps beneficial change.<sup>6</sup> Where issues of conscience were involved, Mooney was not found wanting.

ROY FRANKLIN BARTON (1883–1947)

Roy Franklin Barton is not at all so well known as James Mooney, but he shared much, I think, with him. Like Mooney, Barton was an ethnographer's ethnographer; like Mooney, he was self-taught. And like Mooney, he wanted nothing more than the chance to do fieldwork. He was born near the Spoon River country in Illinois in 1883, just a couple of years before Mooney joined the Bureau. After his schooling in Illinois and, apparently, a few years of teaching, Barton was seized—suddenly, it seems—with the idea of teaching overseas and signed on for a civil service teaching job that took him to the Philippines. He soon asked to be transferred to the Ifugao region because within it there existed a still-functioning indigenous culture and he wanted to see it and experience it himself.

And yet after a decade in the Philippines, Barton returned home and undertook to study dentistry. In those days, too, there were too few jobs for anthropologists, and Barton knew it. He actually finished writing both *Ifugao Law* (1919) and *Ifugao Economics* (1922) while earning his degree in dentistry. The monographs were then published in the anthropological series of the University of California.

Chafing under an alimony judgment he considered unjust, Barton left the United States without a visa—he managed to wangle one in Europe—and in 1930 entered the Soviet Union. There he practiced dentistry at first but was soon able to affiliate himself with the Institute of Ethnology of the Academy of Sciences in Leningrad. He stayed ten years in the Soviet Union and remarried there.

In 1937 Barton returned to Ifugao for more fieldwork, and in 1938 he published his remarkable *Philippine Pagans* (or, as it is entitled in a later edition, *Autobiographies of Three Pagans*), which any course on life history could profitably include. In 1940 Barton left the Soviet Union for the Philippines once more. There he was taken prisoner by the occupying Japanese army and was interned for three years in the camps; only in 1945 was he able to return to the United States. Here he finally began teaching anthropology, though much weakened by starvation, beri beri, and old injuries. He still intended to return yet again to the Ifugao people, but while holding a research fellowship at Chicago he fell ill and died soon after, in 1947.

Of Barton, Alfred Kroeber would write that he had “produced some of the most gifted ethnography ever written in English” (Kroeber 1949:91). Fred Eggan also wrote admiringly of Barton, particularly about his con-

tributions to our understanding of bilateral systems and the nature of the kindred. W. Adamson Hoebel had high opinions of Barton's treatment of law and legal practice; he used much of Barton's materials in his own theorizing.

Anyone who reads *Ifugao Law* or *Philippine Pagans* will discover how Barton's immersion in the round of daily Ifugao activities endowed his ethnographic knowledge with both substance and life. In *Philippine Pagans* he introduces the reader to three native autobiographers, first providing a sketch of Ifugao culture. We get a sense of Barton's conception of fieldwork here from his various offhand remarks. “Throughout my eight and a half years in Ifugaoland,” he writes (1963[1938]:10),

my house was a boys' *agamang* [bachelors' house]. The youngsters came in at dusk without any by-your-leave, would help my houseboys with their chores if asked to, would scuffle with each other, romp and banter for a while, would quiet down a bit when tired, lie on the floor, scuffle again and play pranks with each other's bodies, tell stories and obscene jokes, and finally fall asleep, several under one blanket.

Sounding rather like an indulgent housemaster or parent, Barton then proceeds to give us, through the autobiographies of his friends, remarkable accounts that could only have been elicited in a context of genuine trust and friendship. These narratives are particularly telling for the cross-cultural light they throw upon concepts of romantic love. But they are also telling for the illumination they afford of Barton's skill as ethnographer. Of his *Ifugao Law*, Joan Vincent (1990:136) writes:

The greatest contribution to the anthropology of politics between 1898 and 1919 came from the pen of one who was neither an academic nor a professional anthropologist. It was the work of a schoolteacher whose sole ambition at one time had been to be a “data-collector,” an ethnographer working in the field.

The distinction I sought earlier to suggest between ethnographer and professional anthropologist is made more visible here. Though Kroeber, Eggan, Hoebel, and Vincent praise his achievements highly, Barton was “neither an academic nor a professional anthropologist.” His work proves that anthropology cannot control the skills of ethnography, though of course it can exercise them if it chooses. But it is also important that Barton, like Mooney, evinced the kind of readily apprehendable sympathy that earned him purchase on the honest opinions of his friends and hosts in the field, and in a manner that is simply not available to most of us.

Even if we cannot become Bartons, we can learn from him. He loved doing fieldwork, and he worked at doing good fieldwork. He liked the people he worked with, liked being there, liked learning about their culture, and plainly gloried in becoming their friend. There is not the slightest indication that he had any reason for doing this

6. I do not mean to imply that people go to the field without a theoretical outlook or suggest that they should. The relation between facts and theory is an issue that falls outside my argument here.

beyond wanting to be a first-rate ethnographer. Kroeber says of Barton: "He was always memorable: courageous, candid, forthright, self-reliant, alert but kindly, hewing his own way. *Above all, he forgot himself over the objective that rose in his view; his soul never rusted*" (1949: 41, italics added).

Barton's deep commitment to fieldwork made his remarkable contributions to theory possible. Yet what mattered to him were the human exchanges upon which the opportunities to do good fieldwork depended. He was being himself; and being himself, Kroeber tells us, "he forgot himself over the objective that rose in his view." The personality of the ethnographer and the intrinsic quality of the fieldwork seem alloyed in Barton's success. But it was upon the data he collected that the theoretical worth of his monographs rested. He needed the fieldwork opportunities to collect the data. His love for the work and his personality helped to make the data available. Upon the data he collected he was able to build his theoretical positions.

#### AUDREY ISABEL RICHARDS (1898–1984)

Audrey Richards is remembered by some because there was a time when it appeared that she might marry her teacher, Bronislaw Malinowski. But her fellow students, those who became her coworkers and her students, and we who read her work have far better reasons for remembering her. Richards began her fieldwork studying the food-related behavior of the Bemba; her work there is mostly embodied in two books, though the field data she collected far exceeded the material she could publish. The second of those books, *Land, Labour, and Diet in Northern Rhodesia* (1961), is quite simply the best monograph on the anthropology of food ever written. Just how good it is becomes even clearer if, after reading it, one then reads her dissertation, which became the book *Hunger and Work in a Savage Tribe* (1948).<sup>7</sup> Once liberated from the topical, theoretical, and stylistic constraints that her teacher, Malinowski, must have represented, her own great good sense endowed the data with their everyday meaning. In doing so, I think that Richards returned to her work its potentiality for contributing to relevant theory and removed it from the rather less relevant theoretical heights to which Malinowski's views appear to have consigned it.

Despite the superb scholarship that *Land, Labour, and Diet* represented, its publication did not lead Richards to continue to work on food and nutrition. At the time, specialization in food and nutrition was viewed—accurately, it seems—as the kiss of death for professional anthropologists, particularly if they were women. Richards never really returned to the subject, though her contributions to it were immense. Her findings, partic-

7. This odd title, which falls so unpleasantly on modern ears, should not distract readers unduly. It seems certain both that the term "savage" did not always and only signify contempt and that even Richards's choice of a title may have been influenced by other opinions.

ularly in regard to overworked and underfed Bemba women, were—it seems now—quite carefully ignored because of the government biases of the day and perhaps also Malinowski's own hierarchy of research priorities. In an enlightening comment, Gladstone (1986) argues that Richards was much ahead of her time in her analyses of gender inequality from the vantage point of labor time and food intake. Since Richards's work, much more attention has been paid to the problem of *intrafamilial* variation in consumption, expressed as gender- and age-based consumption differentials.<sup>8</sup>

Though she mostly gave up nutritional studies, Richards went on to distinguish herself by research on kinship, political organization, and of course ritual, as well as on a number of applied issues. Her fieldwork was marked by great care and an unusual capacity to concentrate on the task at hand. On visiting Richards in the field, for example, her sister was upset to notice that the Bemba babies with whom she was working were covered with flies and began to remove them. But Richards responded by saying, "Don't do that, count the babies; and count the flies" (1967:45, quoted by Caplan 1986:66). Richards wanted the results of her fieldwork to be accurate; she also wanted them to be of use. "I believe that there is also something of value in having research done," she once wrote, "by field workers who are assuming for the moment that customs have a purpose, that the society works, and that the African is right, not wrong" (1967:46, quoted by Caplan 1986:66). Gladstone has written feelingly of Richards's fieldwork attitude, of her desire not to overintellectualize the reasons people do things. She says (1986:349):

It was against her scholarly instinct to attempt the brilliant coup by which all that was taking place on a ritual occasion suddenly came together, to be made clear in a learned paper that was brought home when the fieldwork was over—fine fruit of ethnographic clarity, but not the fair reporting she believed the quintessential basis of all good fieldwork, demographic, ethnographic, nutritional, epidemiological.

Richards did not hold for long any regular teaching position in the university system. She did have, however, many students while she was an administrator at Cambridge, and she is warmly remembered by them. Richards, wrote Caplan (1986:68),

anticipated some of the trends in post-modernist, reflexive anthropology in her scepticism about objectivity, her understanding of the subjective nature of the ethnographic process, her awareness that what the anthropologist writes is also affected by the nature of the readership, and her realization that the ethnographer has an effect on the people studied.

Gladstone (1985:10, italics added) has commented on

8. Historical studies of the standard of living in European countries show this clearly (see, for example, Mintz 1985:144–45).

Richards's skill as a teacher, and those remarks cast additional light:

Working with students today, I often remember the slow and sometimes penitential road we trod with her and I realize what we learned from her. It was a characteristic brand of *self-effacement*. It permitted her students to discover new knowledge, new perceptions, for themselves. This attitude is the principal debt we who now teach owe her as a teacher.

Imagine being able to teach students *more* by being self-effacing!

#### ANASTACIO ZAYAS ALVARADO

Anastacio Zayas Alvarado, a Puerto Rican sugarcane worker with whom I was able to work for nearly 50 years, had no academic training in anthropology. In fact he had been able to attend school only irregularly, at most for only about four years, and had had to begin to make his living at physically demanding manual labor while still a child. He started instructing me in his culture before I could even understand or answer him properly in his own language, and those lessons continued in different forms until shortly before his death, in 1996. Years after completing a community study in his village, I asked him if he was willing to have me record his life story, and he agreed. Before we began, however, he gave me a brief *written* outline of what he saw as the principal milestones in his life. Those pages no doubt represented an enormous effort on his part. As I have said elsewhere (Mintz 1989), I think that at that moment Taso (as he was called) had a better grasp of the social science implications of what I was trying to do than I did.

His skill as an observer and instructor merits mention, I think, because these seem to me to be related to his ethnographic sensibilities. In a detailed and lengthy description of the oxen on the old sugarcane haciendas of the 1920s and the terrific labor of breaking and training them to the plow, he concluded: "As for the animals which could no longer work because they were old or injured, they had the custom of taking them to sell in Salinas, to the butcher shop, or to Santa Isabel, or one of those towns, and there they would sell them for meat since the oxen—well—could no longer work." And then added, with a sweet smile: "That was their pay" (Mintz 1960:60–61). The implicit comparison with those men whose brute labor broke the oxen—and indeed, with all of the people like him, themselves broken and consumed by crushing physical labor—could not be lost on any careful listener.

Taso used his own language in a highly dramatic, even poetic fashion, sometimes misusing words but always expressing himself in a richly revealing manner. He constantly separated his own behavior from any normative statements I might try to elicit from him concerning family members, neighbors, his village, or Puerto Rico. When I might inadvertently ask him how something was done in his village, he would predictably reply: "Well, I

can't tell you how it was done, only how I would do it." Yet he repeatedly drew upon events and personalities in local life in order to explain some problem or concept. In his own way, he employed the case method Barton pioneered in *Ifugao Law*.

Taso once collected for me from local people a valuable body of information that I had been too disorganized to collect for myself. In tables that he himself had made, he presented me with detailed information on kind of stove, water source, and other technical domestic features for nearly 60 individual households in the community where we lived. It was done better than I could have done it. It enabled me to revisit the households in question in order to interview the household heads more effectively. But one reason I call Taso's perspective ethnographic, and him an ethnographer, is different; it is his remarkable outsider's sense of his own culture.

I do not know to what to credit his curious outside/inside way of looking at things, but I think that it is traceable to his being a highly intelligent person who had succeeded in meeting, in local terms, every challenge his culture and class presented yet was compelled to live an intellectually unchallenging life.<sup>9</sup> I refer to the work he did for me to stress once more my conviction that one need not be a professional ethnographer to do ethnography.

#### COMMONALITIES

My purpose here has been more than simply to recount for you some facts about four colleagues who distinguished themselves by the ways in which they collected and made sense of data. In addition to their painstaking data collection and the kind and quality of data they collected, these fieldworkers are admirable, in my opinion, because they had solid commitments of a personal kind to the work they were doing. I want to be clear. I do not mean that ethnography *must* turn on some acceptance by ethnographers of the cultural values of the people they study. Nor do I mean that ethnographers must become advocates of the people they study. Indeed, some of the best and most trying fieldwork has been done with people with whose values their ethnographers were *not* in accord. But ethnographers must believe that the work they are doing is worth doing and must identify enough with the people at least to feel that what they are learning, perhaps at some cost, will enable them to provide a reliable account of their culture.

The preceding accounts probably show this most

9. While we worked on his autobiography, I lived in a little shack behind Taso's house. When I visited him some months after our work was finished, he told me that when he looked out his kitchen window and saw my little house he would miss me and the work we did together. It was the closest he ever came, I think, to reproach. I will always take pride in having been allowed to record Taso's story. His perceptions throw light, I think, on debates concerning native anthropology, perhaps particularly in the case of economically stratified societies. Who should "speak for a people" is a genuine issue in today's world, in which the illiterate and the inarticulate seem so often to have been gagged by their verbose fellow citizens.



clearly in the instance of Mooney. In Barton's case, his affection for the people with whom he worked is unmistakable. Richards clearly wanted her research to be of some genuine benefit to the people who helped her. She took her charge seriously, though I suspect that she would have disapproved of any labeling of this sort of her position. It was different for Taso, because he "studied" only his own culture. In his case a consistency between his moral outlook and the details of his life is revealed, I think, in his own story. But it is seriousness of purpose, an attitude about the work one does as an aspect of oneself, as well as compassion and a willingness to work hard, that marks them all.

## Implications

I want now to consider the implications, as I see them, for the ethnography that we do of the three large-scale social changes to which I referred at the outset: (1) the growing permeability of national and cultural borders and the effects of that change on typologies of societies; (2) the declining significance of observer-centered disciplines such as ethnography in popular thought and in the common view and the consonant invalidation of ethnographic authority; and (3) the increasing diffusion and simplification of the concept of culture, as it has become, together with ethnography, more and more the bread-and-butter of nonanthropologists both within and outside the academy. These changes have changed anthropology's tasks. Because more people move greater distances and with greater speed and frequency than in the past, we ethnographers need to develop new techniques to study them, particularly if they spend most of their time moving or reside alternately in two or more different cultural locales.<sup>10</sup>

But we should not have to be reminded that even in this rapidly changing world—this globalized village—there are still literally billions of people who have never gone anywhere and have no plans to go, either.<sup>11</sup> They are experiencing other kinds of changes, and we need to learn what they think—why they stay where they are and how they and their mobile relatives feel about each other. These stay-at-homes are not likely to become a minority, no matter how big or numerous the airplanes become. To come to know them well will still require traditional fieldwork: the same willingness to be

10. But I have also suggested (Mintz 1998) that these are not wholly novel conditions. Africanists in particular, accustomed to dealing with people who spent much of their lives in the mines, struggled to study and theorize about such situations. Unfortunately, people don't read that literature much any more.

11. The other side of this being, of course, that people have been traversing the world in enormous numbers for a very long time—about 100 million crossed oceans in search of work in the 19th century (Lewis 1978; Mintz 1987, 1989), half of them non-white. The history of New World slavery ought to be a lesson in transnational movement, one going back to the 16th century. Leaving out such massive fragments of the past in discussing globalization, as if what is happening today were totally unprecedented, almost looks like a racist tactic.

uncomfortable, to drink bad booze, to be bored by one's drinking companions, and to be bitten by mosquitoes as always. As always, such fieldwork will go better, too, and produce more if fieldworkers learn the language, read beforehand what is available and relevant to the place and problem, and think seriously about their rapport and their ethical obligations in the places where they work.

That ethnography and the fieldwork it requires may not be taken quite so seriously as they once were has many causes, some not readily apparent. It owes in at least some measure to the highly critical attention devoted to ethnographers in recent years. It is therefore worth saying, obvious though it is, that ethnographers and ethnography are not the same thing and that good ethnography is worth reading and worth trying to do whatever critiques are made of the ethnographers. The movement to demystify ethnographic authority has surely had some positive results, but it has had some side effects relevant to the future doing of ethnography—and to the reception of ethnography by the general public—that merit additional scrutiny.

All of us are now aware that some anthropologists of the past century and the first part of this one were condescending or ethnocentric. But we should not let this awareness lead us to underestimate the hard work behind their ethnography or the skills it required. If we wish to call these ethnographers into question, we should nonetheless use caution in trashing the results of their fieldwork. It was the careful research they had done that led our predecessors to develop the honest respect many of them felt for the peoples they worked with, even if that respect did not rule out an accompanying inclination toward condescension. It was research on technically simpler peoples in particular that first led our predecessors to assert that it is not so much what people have that matters as what they do with what they have. On these aspects of the life of non-Western peoples—for example, their astonishing dexterity and precision in working with indigenous tools and materials—scholars who, on theoretical issues, might be at loggerheads, such as Boas and Malinowski, were not in any real disagreement. When we read Malinowski on the agricultural techniques of the Trobrianders or Boas on the material culture of the Kwakiutl, we find in their work their equivalent respect for the skills and industry of the people. Such ethnography requires great skill and prior knowledge.

So much for the past. As for the future, there remains considerable ethnography that needs and deserves to be done. We should continue to be interested in what our informants know that we do not, in what they know how to do that we do not. Gathering that information continues to turn on skill and prior knowledge; without them we cannot understand what we see or are told or record it. Among the tasks, I might mention only one small omission. So far as I know there are no adequate collections anywhere yet of the utilitarian objects that have been made—and are still made—out of Western trash; nor are there adequate ethnographies of the techniques employed in the manufacture of these objects;

nor are there enough data on their makers and inventors or occupational histories or life histories of these people, whose handicrafts are eloquent testimony to the vivacity of cultures caught up by the world system at various junctures between what once was and what is coming.

Successful challenges to the ethnographic authority of our predecessors may have contributed to some dismantling of the ethnographic enterprise itself. The repudiation of the individual character of ethnographers is a decidedly different enterprise from the repudiation of ethnography—or at least it ought to be. This would matter less were there not quite so many untrained but enthusiastic nonanthropologists ready to ragpick in the intellectual garbage dumps that we have made. If we do not do ethnography as we have learned to do it, they will do what will supplant ethnography, some of it good and some bad, without us.

The apparent abandonment of the concept of culture has also had some curious consequences. In practice, I think its renunciation has helped to deprive the discipline of a voice of its own: a distinctive anthropological voice. Meanwhile nonanthropologists have discovered the usefulness of the term “culture” to describe just about everything, much as happened with Freudian talk in the 1940s. We now have the culture of factories and companies, political culture, the culture of the Congress, the culture of football teams and locker rooms, culture wars, and so on. I think the evacuation of this central concept—making the term “culture” synonymous with “someplace where somebody does something”—poses substantial problems for the ethnographer-to-be. More important, perhaps, our growing lack of a distinctive voice will, in the long term, probably mean fewer and fewer nonprofessional readers. This may be a development that some anthropologists endorse. Yet I suspect that much of what we write now is likely to prove less interesting to nearly all but fellow professionals and possibly to fewer and fewer of them. I think that having readers, especially nonanthropologists, who read what we write is a virtue. There was, after all, a time when anthropology was read by many nonscholars as well as by scholars outside the profession. I think it would serve our discipline well for that still to be so. Of course being read means writing clearly enough and making enough sense that people will want to read us. If we do not owe that to our teachers and those whom we study, at least we owe it to our students. If we write without making ourselves understood, we run the risk of letting down our constituents.

The partial dismemberment of ethnography is taking somewhat different forms inside and outside the academy. Outside, in the world of anchorpersons and talking heads, we need to do ethnography, I think, not only because it needs to be done but also because as we do it less Dan Rather—complete with traveler’s vest, water bottle, and interpreter—can be counted upon to do it more. Inside the university, that anthropologists show little concern for the concept of culture has certainly not deterred others from doing what are now commonly called “cultural studies.” Our discipline’s concepts, hav-

ing helped to nourish history, political science, and sociology, are now helping to sustain other disciplines within the social sciences and humanities as well. Indeed, anthropological concepts now seem to be helpful to everyone except possibly anthropologists. Even if the culture concept is dead, as I am told, nonanthropologists appear to do well by battening upon its corpse.

Under such circumstances, what is the status of ethnographic fieldwork? My own view is that good fieldwork may be the silver lining to any cloud that hangs over our discipline. Fieldwork has always been what we do—and what we have learned to do—best. Our theories acquire their strength, elegance, and conviction in accordance with the quality, honesty, and reliability of our fieldwork. Our contributions to higher-level theory will be persuasive only as the quality of our fieldwork is persuasive. Ethnographic fieldwork, our sort of fieldwork, seems to me close enough to the core of our identity as a discipline to be worth preserving—reading, teaching, doing—at any cost. If we turn our backs on it, there is no doubt that nonanthropologists will do it in our place. Some of them—like Barton and Mooney—will do it better than we do, or anyway just as well, but most of them will not. Few will understand what our profession and its history represent or what battles about the nature of human nature or the scientific basis for a belief in human equality our forebears helped to fight and win. We anthropologists most of all should know that the very definition of what is human is still contested. Because that is so, the field research we do is in the interest of our own humanity.

But if ethnographic fieldwork is the silver lining, what are the sows’ ears? I think that we ourselves may become the sows’ ears if we try to be what we are not—to be less than what we were trained to be. Like Antaeus, we need contact with the earth to avoid strangulation; our earth is the field. Anthropologists have a choice. Though the world isn’t what it used to be, we can still be the world’s best ethnographers. We need to do the best fieldwork that we can and to make the contemporary relevance of its findings both understandable and accessible. That what we can do and discover should be plundered so joyfully by others all around us is eloquent evidence not only of our promise but also of what we must do in order to add to the enduring contributions of our discipline.

## Comments

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*Que viva ethnographic fieldwork! Or—to be more precise—long live a version of anthropology’s participant-observation methodology that leads to political and practical relevance as well as to interesting, comprehensible*

theory! Mintz deserves our thanks for his humanist championing of political engagement in our sometimes postmodern world, where a globalizing capitalism is crucifying the traditional subjects that anthropologists too often exoticize.

My only slight disagreement with Mintz is over his concern with (1) saving anthropology, (2) caring about the vulgarization of culture, (3) worrying about the fashionable stature of cultural studies, and (4) distrusting encroachment by journalists into ethnography. I agree that a disturbingly large cohort of anthropologists is now spouting jargonized gobbledygook in the name of subversive, destabilizing theory. Worse yet, the future looks bleak because this energetic theoretical triviality is especially pronounced among the most ambitious students (who are sometimes the smartest) in the most prestigious doctoral programs (which are sometimes the best). I am not convinced, however, that it is worth arguing within the framework of the irrelevant elitist debates dominating much of our discipline. Furthermore, (1) ethnographic authority desperately needed debunking; (2) cultural studies *combined with ethnography* offers exceptionally creative insights; (3) the rigor with which we debate the meaning of the culture concept does not weigh heavily in U.S. "popular culture," because the vulgarization of culture is driven by a historically rooted North American political economy built on genocide, conquest, slavery, and ethnically segmented immigrant labor markets that has racialized understandings of inequality and that foments cultural nationalist essentializing discourses; and, finally, (4) it is appropriately humbling for us to recognize that much of our ethnographic work is merely an inefficient, time-consuming version of investigative journalism. Sadly, the best ethnographic books on contemporary urgent social problems are being written by journalists not by cultural anthropologists. Of course, most journalists are also writing the worst books on the controversial relevant topics that anthropologists usually fail to address. The television anchors who make the world safe for U.S. multinational corporations "complete with traveler's vest, water bottle, and interpreter" are indeed a travesty that we need to confront with an ethnographic perspective in the widest possible public forum.

From a more personal perspective, Mintz's article makes me wonder what exactly we mean by ethnography and what is the interface between theory, methods, and politics. Indeed, I know that participant-observation methods and the tenet of cultural relativism are what motivated me to become an anthropologist. They are also what I most enjoy teaching. I find myself feeling happiest, most productive, and most at home when engaged in participant-observation fieldwork—which, incidentally, is how ethnography needs to be defined in anthropology so that it is not reduced to "open-ended, semi-structured" survey research dryly completed on a nine-to-five schedule.

It disturbs me that my students prefer to argue over theory rather than to cross class, ethnic, gender, and other boundaries that are structured by power in order

to talk humbly with real, live humans. Sometimes I suspect that the decreasing prominence of fieldwork among younger anthropologists is due to an upper-middle-class (postcolonial) distaste for violating cultural and class apartheid. Perhaps it is simply a resistance to giving up creature comforts or a privileged flight from the painful realities of social suffering and everyday violence that require political subjectivity, not to mention concrete engagement.

Returning to why ethnography is so crucial to anthropology, I remain a bit confused by our intellectual dichotomy between methods and theory when viscerally it is clear to me that anthropology's most important contributions to theory and politics have emerged from its cross-cultural—and usually bleeding-hearted—means of collecting data. Mintz's legacy in anthropology arises out of just such an interface between human engagement, theoretical rigor, and critical creativity. Over the past five decades, in complementary ferment with colleagues, classmates, co-ethnographers, and friends—such as the late Eric Wolf and the late Anastacio Zayas Alvarado—he has established a safe space for Marxism in a U.S. academic environment that had been disabled by McCarthyism and is still poisoned by the legacies of the cold war. Indeed, somewhat ironically, regardless of this article two generations from now Mintz will probably not be primarily remembered for his commitment to good ethnography. More important will be his path-breaking theoretical contributions to a historical political/economic and global perspective on capitalism, culture, exploitation, and slavery. (Of course, it is also significant that he is an empathetic human being with a good politics that leaps from the pages of his writings.)

Having written this comment I am now more eager than ever to run back outside to hang out on street corners sipping malt liquor with drug dealers, addicts, and articulate hustlers. At the same time, inspired by Mintz's theoretical approach, I am committed also to taking field notes on my experiences prowling the corridors of corporate power in the heart of the beast and to linking that material to the everyday social suffering I encounter on the street.

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21 VII 99

In elegantly simple prose, Sidney Mintz reminds us of basic truths—ones so basic that we have sometimes forgotten them. And in perhaps his signature style—a winning politeness coupled with a deep mischievousness—he makes his critical points indirectly. It takes a while for it all to sink in. As Clifford Geertz once commented of Ruth Benedict (one of Mintz's teachers), it would be a pity if readers didn't appreciate the subversiveness.

In essence, what Mintz presents is a history of anthropology "from below," from the point of view of the

work of four more or less marginal individuals. I have to confess that I have never read James Mooney or Roy Barton. I know of “Taso” only through Mintz’s work, and Audrey Richards I read for my Ph.D. exams. None of these men or women ever occupied a chair of anthropology or is remembered as having founded a school of thought or a theory. When I taught a graduate seminar on the history of social and cultural anthropology this past academic year, I may have mentioned Richards (probably in relation to Malinowski), but I never suggested that any of my students could read her in place of the “canonical” anthropologists on the syllabus. Yet each of these four was, according to Mintz, a great anthropologist.

Stories about the past are often indirect commentaries upon the present. And herein lies the “hook” of Mintz’s story: these four individuals practiced ethnography with a depth and seriousness that is often lacking today.

Has, in fact, the practice of ethnography declined? Historians of anthropology—ones who are more distant from our time and know how our story turns out—will have to answer this question. But my (insecure) sense is that Mintz is correct: ethnography has recently lost depth.

A great many anthropologists understand this change as the effect of something they call “postmodernist” theory. After all, a certain kind of postmodernism celebrates surfaces and explicitly rejects depth. But I am not convinced that such abstract ideas have the power, on their own, to transform anthropological practice. Mintz mentions changes in the world that we study: Globalization has made the task of ethnography more complex. But as he points out, globalization has been with us from some time. Is it only our own racial preconceptions that condition the fact that when someone says “globalization” nowadays we think of the Internet rather than the slave trade? And then there is the loss of an integral concept of culture. I would put this matter slightly differently from Mintz. What has been questioned is not much the notion of “culture” (although the critique has often been stated in such misleading terms) as the assumption of “a culture.”

To all of these factors I wonder if we should not add attention to how the structure of academic life has changed since World War II. To simplify, it seems to me that bureaucratic rationality, an emphasis on efficiency and production—in a phrase, the market—invaded academic life over this period in a qualitatively new way. What was once a relatively leisured, elite existence—a world in which a department chair could offer a job to anyone he wished (and it was a “he”), in which there were no student evaluations of teaching, in which tenure was more or less automatic once one got a job, in which one’s status was determined more by one’s position than the volume of one’s publications—this world has vanished, even at the most elite academic institutions.

Such transformations may appear to exist at some distance from the practice of ethnography, but I am not so sure. Recently, I was taken aback when an editor at one of the most prestigious university presses in the country told me that she was reluctant to publish anthropolo-

gists’ first books. She was looking for manuscripts by more established “names” that would sell copies. But of course anthropologists’ first books are often their most enduring contributions to ethnography. To be published now, a young anthropologist must sell enough books—and that means, almost by definition, enticing a transdisciplinary audience with currently fashionable theory and/or topics. Ethnographic depth does not necessarily do the trick. The structure of the market in fact works against it.

But it is not only the youngest generation that is affected. My own generation, the baby boom just beginning to enter the senior ranks of the profession, is also involved. Mintz eloquently illustrates how the love of fieldwork underlies ethnography. But there is probably some hate here as well. Faced by the continuing pressure to publish—yet encased in older bodies with more social obligations—few anthropologists of my generation have chosen a second field project abroad. Fieldwork “at home” has become an honorable alternative, as it should, but perhaps the “easiest” solution is to write about, not do, ethnography or to take up theory or even history.

All of this has meant that anthropologists are currently doing much more than ethnography. In the future, some of this work will be probably be seen as naïve philosophy or history or literary criticism; after all, those disciplines require their own arduous preparations. Faced with uncertainty about how to evaluate these new kinds of anthropological work, the academic market has not always favored depth. One small fact does much, I think, to explain why: the proportion of faculty members at the highest-ranking academic departments who actually read and come to some independently informed judgment about the dissertations or books of those they are considering hiring or promoting. I would guess that we would be surprised by how low this proportion actually is. Deep writing depends upon deep reading, and in a world dominated by efficiency and bureaucratic rationality there is never enough time to go around. One doesn’t have to “celebrate” the surface in this setting. It is built-in.

Mintz’s student Rolf Trouillot coined the phrase “the savage slot” for the conceptual schema with which anthropologists approached ethnography earlier this century. But we ourselves have “slots” for one another. Occupying such a slot requires becoming an ambassador, sometimes a war leader, often a jobs patron for a certain (usually simplified) theoretical position. It requires strategic overstatement—not the meticulousness of the ethnographer. I am not suggesting that wholesale change has occurred within our field. There have always been great anthropologists who were not great ethnographers (Radcliffe-Brown, for example). But the balance between theory and ethnography within the discipline does seem to have shifted somewhat lately.

“Postmodernism” may, then, be not so much the cause as the result of more mundane changes in academic life. If my speculations—and they are nothing more than that—have any value, then the conclusions do not appear encouraging. What is to be done? After reading Mintz, I

for one am going to change the way I teach the history of social and cultural anthropology. I want to spend more time on great ethnographies rather than on recounting the interaction among theoretical slots. Maybe I'll even find time, amidst all the other things I have to do, to read Mooney and Barton.

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I had the pleasure of hearing this address when it was delivered, and it is instructive to see it now in written form—although always, where Sid Mintz is concerned, we are missing a lot when the performer is absent. I certainly agree with him that ethnographic fieldwork is at the core of anthropology and must be preserved and that the quality of our theory is closely related to the quality of the fieldwork upon which it draws. If I disagree over such matters as the relationship between method, fact, and theory and whether these can be discussed separately from notions of purpose, sorting these differences out would require more space than we have here and would not weaken our more basic agreement on the central questions. I would, however, like to caution against what I see as a likely misreading of the address, hoping that he too will see it as a misreading.

I think it would be a mistake to read Mintz's address as arguing that because the discipline as a whole needs to be grounded in fieldwork, the contribution each of us makes requires grounding in our own fieldwork. Such a reading, marked by the fallacy of division, would substitute a mechanical for an organic solidarity (admittedly a solidarity that is very loosely structured), and I think it is this organic character that contributes significantly to the interest and excitement that anthropology generates and our attraction to it. We all know people who spend long years in the field and publish little (although they often find other ways to convey their experiences), and we all know others who spend little time in the field yet publish voluminously, basing their writings on the fieldwork of others. The contributions of both of these groups, in conjunction with those of the more traditional fieldworker-writer, have been essential to the richness, variety, and freedom of activity that characterize the discipline.

There is another aspect to this. I'm sure that anyone who has seen Mintz in the field (or on any sort of unfamiliar terrain) and observed his penetrating curiosity, his physical dexterity, his attention to detail, and, perhaps above all, his great good humor, openness, and generosity—in sum, his wonderful capacity to establish a strong, winning presence wherever he is—cannot fail to think, "Here is the real item, the truly gifted fieldworker." I'm sure, too, that few of us will want to stack our own knowledge of the discipline against his and challenge his view that many of the outstanding works of anthropology have been built on fieldwork marked by "seriousness of purpose, an attitude about the work one

does as an aspect of oneself, as well as compassion and a willingness to work hard." But I am also sure that many of us, while deeply respecting Mintz's example and the standards he sets, are not able to measure up to them and/or conceive of our activity somewhat differently. During fieldwork some may tend to be "identifiers," some may be exhibitionists; others are more "interventionist" (perhaps preferring, as Audrey Richards's sister did, to chase the flies away from a child's face rather than count them); some may engage in fieldwork as a relatively playful "liminal" experience (which is often how informants behave with us, welcoming the relief we may provide from their everyday tasks), while others approach it, perhaps too sternly, as a "wager"; and still others may be staunch "objectifiers" (one example that comes to mind is that of a well-known anthropologist recently described to me as organizing his fieldwork like a doctor's office, with informants ushered in for consultation in rapid succession, at strictly appointed times).

While each of us has preferences and aptitudes and some of us may adopt different stances in different situations (choosing these sometimes spontaneously, sometimes deliberately) and while each of these stances (and the many more that might be cited) has its own personal, cultural, and political implications—which we are free to challenge and argue against—I think most of us will agree that one of anthropology's virtues has been its capacity, even as it explores the variety of human experience, to accommodate great variety among its practitioners and to show us aspects of self we might otherwise not confront (some of which Mintz has referred to elsewhere as those of "infant, victim, and tourist"). The way each of these styles, stances, aspects informs our relationship with and presentation of the "other" and the way they are later reworked and represented in writing, lecturing, and other forms of communication are central creative tensions in anthropological work.

In sum, then, and I hope Mintz will agree even while he winces at the expression, there are many ways to skin a cat. Certainly the adoption of any particular style does not ensure or preclude success, but the variety, taken together, provides a benefit that no single one can provide on its own. In judging the results of our efforts, we usually start by saying that each work should be judged on its merits, that the proof of each pudding is in the eating. But we should also go farther and recognize that although a particular ingredient, say, sugar, may be very desirable and even essential in desserts, it would be a mistake to mandate it for every course in the meal.

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It is good to have this reminder of the importance of ethnographic fieldwork at a time when there is so much doubt and concern about the future of ethnography. Mintz identifies some of the factors that have contrib-

uted to this uncertainty. He argues quite convincingly that even though the conditions under which we work have changed enormously, this has not made it impossible or unnecessary to continue the tradition of sympathetic, committed, and ethically informed fieldwork.

However, there are some questions raised by the way in which this argument is presented, putting up a parallel history of ethnographic-like practice originating well before anthropology became a discipline and continuing to this day outside that domain. Many anthropologists would recognize the portraits drawn here, of outsiders similar to James Mooney and Roy Barton, crossing cultural boundaries with sympathy and understanding in the backwash of European expansion, or cultural experts like Taso, combining the wisdom of experience with a rare reflective sensibility. It is as well to be reminded that the ethnographic perspective has been shared more widely than we have been prepared to admit. Having had it drawn to our attention like this, what we might consider here is the difference between the ethnography outside anthropology and the ethnography that is integral to the discipline. Mintz provides some pointers on this but concentrates more on what is shared (when both are done well) rather than what separates the two. There is a danger in trying to typify them like this because of the great variation found in both. Who would want to characterize the ethnography being done today by anthropologists, let alone nonanthropologists?

There is one aspect of this that Mintz alludes to but does not develop and that is critical to the difference I am talking about, and that is the commitment to theory. We are much less inclined towards the kind of empiricism criticized by Willis (1997), less likely to trust implicitly in our ability to make sense of other cultures through the experience of fieldwork. Mintz himself has been a pioneer in developing the alternative described by Willis, a more theoretically informed ethnography. I raise this point because I think it is very much understated here. While it is as well to acknowledge those “ethnographic practitioners without ethnography” who share, in a fundamental sense, the same interest in and feeling for the human condition as anthropologists, we also have to make it clear that much of the ethnography we do now goes well beyond this, being driven as much and closely informed by explicit theoretical interests. How else are we going to understand the large-scale social changes having such an impact at the present time and take account of their bearing on the people we study? What I am suggesting here is that it is not enough for us to operate just along the lines being advocated here. By all means, good fieldwork should continue to lie at the core of the discipline, so long as it does not stand alone.

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With characteristic boldness and attention to nuance, Mintz paints a cautionary portrait of the state of contemporary anthropology. In a time in which the “border crossings” of interdisciplinarity and globalization have become the central mission of many universities in this country, one might imagine that anthropology would be experiencing a heyday. Instead, Mintz sees two of our most sacred cows—the concept of cultural and the practice of fieldwork—as neglected by the discipline and usurped by nonanthropologists. Where Mintz takes a grim view of our future, he is more generous with our past, in which there were also nonanthropologists doing fieldwork and anthropologists who had active careers as public scholars. As his examples of Mooney and Barton illustrate, sometimes ethnography of the highest order comes from outside anthropology. His own career is an excellent case of productive “interloping” across disciplinary boundaries. His first book, *Worker in the Cane* (1960), was a rich and evocative ethnography of life in the cane fields of Puerto Rico. It demonstrated, as he somewhat mischievously puts it, his own “willingness to be uncomfortable, to drink bad booze, to be bored by one’s drinking companions, and to be bitten by mosquitoes”—his coming of age as an anthropologist. Mintz’s fieldwork in the Caribbean (not only in Puerto Rico but in Jamaica and Haiti as well) inspired him to research and write countless articles in the classical anthropological genre. It also led him to venture outside its conventions. Having spent his anthropological career studying those whose lives had been shaped by sugar’s production, it is not surprising that he then turned his attention to the form and meanings of its consumption. His *Sweetness and Power: The Place of Sugar in Modern History* (1985) is not only not an ethnography but focuses on Europe rather than on our discipline’s usual subjects in the “developing” world. His own years of fieldwork on peasants and plantations in the Caribbean gave rise to a historical analysis that has arguably been as important for historians as for anthropologists, demonstrating the importance of sugar in the modern world and, more broadly, the relationships among production, consumption, and cultural meaning.

I raise this rather obvious point to demonstrate that even within the career of a committed fieldworker there is room for cross-disciplinary and nonethnographic forays. Recent engagement in history by anthropologists has been extraordinarily fruitful. Just as we find among the subjects of our research, the life and work of anthropologists seem to evolve in cycles or stages. Most cultural anthropologists, I think, would still agree that detailed ethnographic fieldwork constitutes a necessary, important, and distinctive grounding for a career in the discipline. That being said, the tendency toward historical or theoretical work not explicitly based in ethno-

graphic fieldwork is a common trajectory among anthropologists in the later stages of their careers.

Perhaps the real question Mintz raises here is that of the shape of ethnography itself—what we are or should be intent on salvaging, cultivating, and even sharing and how we might better train anthropologists and nonanthropologists alike to do rigorous fieldwork. The debates in anthropology from the 1970s on about the politics of practicing and writing ethnography have given rise not only to the dismissive critiques of ethnographers, as Mintz says, but also, one could argue, to a greater consciousness of the need for precisely the sort of good fieldwork and solid ethnography he is determined to preserve. For instance, feminist ethnography, some of which has been particularly hard-edged in challenging the premise of “ethnographic authority,” has generated some of the most powerful, rich, and theoretically sophisticated “on-the-ground” ethnography in our field. Lessons from the “reflexive turn” have not merely stymied fieldwork; for some, these critiques have inspired a better integration of theory with empirical description, narrative with analysis, and critical politics with ethnographic richness.

If we dispense with the inclination to be proprietary (or silent) out of dismay over the thinness of “cultural studies,” we might seize upon today’s culture frenzy to reinvigorate good fieldwork and turn toward a new range of contemporary concerns. The profiles Mintz has sketched of Richards, Mooney, Barton, and Zayas Alvarado embody precisely the sort of caring and careful intellectual work that today’s students of anthropology can be most drawn to. Indeed, his own scholarship and that of many he has trained have illustrated that globalization presents anthropology with an opportunity to engage some of its most fascinating challenges.

I have no doubt that good, solid, historically grounded, and meticulously performed fieldwork among those who live transnational lives as well as the more numerous who are “stuck” in one place represents a major area in which anthropology is uniquely positioned to contribute to our understanding of globalization. I share Mintz’s conviction that we must not allow long-term fieldwork, serious language training, and a solid command of the existing literatures and histories of the regions in which we work to be replaced by journalistic sound bites or by sweeping and faceless statistics. However, it seems clear that in light of the complexities of globalization, which Mintz himself has helped to reveal, we must also be willing to develop flexible approaches to anthropological scholarship which include rigorous fieldwork without being shackled to a limited notion of its applicability. If anthropology is not to be left aside either within the academy or outside it, this flexibility will better allow us not only to produce work of the highest caliber but to find interested audiences both among ourselves and our colleagues across the disciplines and also in the wider public arena.

Mintz’s retrospective is tinged by a worried sense of loss where he might, instead, enjoy a sense of pride. His own pioneering work demonstrating the importance of political economy and history to the fabric of contem-

porary culture has inspired many to take up these relationships ethnographically and examine them in the field of today’s complex forms of globalization. At the same time it has helped to make possible a rapprochement between anthropology and history. Looking forward, perhaps the silver linings outshine the sows’ ears.

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29 VI 99

Mintz explores the relationship between anthropology and ethnography today, largely through capsule biographies of four outstanding ethnographers. Three trends make this inquiry necessary: the blurring of distinctions between societies once believed decisive, the fundamental reassessment of all disciplines of observation, and the generalization and vulgarization of the term “culture.”

The common core of anthropology and ethnography, Mintz proposes, is the direct, up-close, full-bodied observation of human practices and ideas. And when both anthropology and ethnography are effective, it is usually when they are hard to separate. The issue is not that either anthropology or ethnography has ceased to grow—on the contrary—or that ethnography has disappeared but that the two have parted ways. Anthropology has moved deeper into academia, becoming ever more the laughingstock of popular culture, even as it leaves others to talk about “culture.” A recent movie in which a fundless anthropologist disguises his family as a primitive tribe (a powerful metaphor, actually) and wows the profession with videos of “rites” performed in his own backyard is only the latest twist.

There has been a sea change, Mintz insists. In his crisp, clean style, he characteristically takes on large and complex issues and makes them come to life, on this occasion through the capsule life histories. The accounts suggest that while the changes in the location of ethnography have not been straightforward, change undoubtedly has occurred. There was unscrupulous as well as honest work in ethnography—or rather, “ethnology,” as it was generally called at the time—as there would be in anthropology. Much of the important research, for or against the interests of the “natives,” went on in U.S. government agencies, especially the Bureau of Ethnology. And researchers such as Mooney and Barton were openly persecuted or marginalized in ways they would perhaps not be today. However, as Mintz argues, on the whole until World War II ethnography was intellectually if not institutionally respected (Mooney and Barton being, in some ways, atypical) and its products rather widely read. Mintz longs to recapture that era, indeed believes this indispensable.

This points toward a fourth trend that Mintz does not quite address, though it underlies his argument: the profound transformation of anthropology as a discipline in the context of equally profound transformation in the U.S. university system. One need not belabor the sheer

quantitative explosion, the pressures to publish and to obtain funding; the conferences with hundreds of panels, multiple concurrent sessions, and ten-minute presentations; academic encounters turned into job fair and homecoming; and a genuinely distinguished speaker shaking consciences—has all this been looked at ethnographically enough?

The relationship between ethnography and ethnology is also intriguing, especially since “ethnography” came into use in the United States in relatively recent times and students such as Mooney and Barton may have seen themselves as ethnologists. Does Mintz hesitate to associate ethnography with ethnology, perhaps considering the latter “unscientific,” or does he view the terms as synonyms?

Mintz may be too quick to tend fences around anthropology or ethnography in other ways. In his argument, “untrained” practitioners of ethnography seem to remain second-best, simply inevitable. But unlike death and taxes, “untrained” ethnography may be an essential interlocutor of “trained” ethnography and anthropology. Mintz does not quite stress this and instead calls for anthropologists to be the “best” ethnographers. In any case, how exactly does one “train” to be an ethnographer, given the importance of personality and many other imponderables that Mintz himself recognizes? Incidentally, one wonders how many trained anthropologists would make good informants (one thinks of the doctor-patient relation).

Also perplexing in a thinker who writes so well is the indifference to the literature that the article would appear to suggest. For instance, especially fecund connections existed—indeed, in personal ways—between early ethnography in the United States and elsewhere and regionalist short-story writing. Such links are continuing, and they challenge the conventions of anthropology as much as they do those of literary criticism or cultural studies. My suspicion is that anthropologists—no matter how much they take to an “ethnographic turn”—can hardly trump the better fiction writers at what they do and how they say it.

Mintz commends Audrey Richards for being self-effacing, but here the (co)author of *Worker in the Cane* is nothing but. Besides the relationship with “Taso” Zayas—who taught Mintz a thing or two about ethnography—Mintz’s links with Mooney are especially arresting. Indeed, it would be fruitful to trace Mintz’s ongoing dialogue with the ethnography of Native Americans generally—the deepest and richest tradition in North American ethnography and perhaps its major contribution globally. This tradition was still powerful when Mintz did his graduate work in the 1940s. Mintz’s complex relationship with Julian Steward in the 1940s also points in that direction, as also do the significant comments on Native American ethnography scattered throughout his oeuvre.

And when the time comes to place Mintz’s work in long perspective—a moment that, given his energy, he will surely see—we now know better some of the people in whose company he would like to be remembered, cri-

tiqued, and appreciated (besides, of course, Eric Wolf, and Melville Herskovits). It is quite a crowd.

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I welcome Mintz’s reflections on the challenges to the centrality of fieldwork in the context of contemporary trends in anthropology, although some challenges are not in fact new. Given the limits on academic funding and the pressure to deliver quick results, anthropologists’ proclaimed need to carry out long-term fieldwork is constantly being questioned. When colleagues in cognate disciplines can apparently carry out research without leaving their offices or sitting rooms, the empiricist and apparently unmethodological leanings of anthropology might seem an unnecessary expense in time and money. I can’t count the number of times I have had to argue on academic committees that anthropology is different, only to be greeted by indifference or outright hostility at the way the discipline refuses to fit into mainstream bureaucratic structures. Apart from such pragmatic considerations, there is a real hostility in some quarters, as Mintz implies. I was recently greeted with disbelief and outrage by a renowned scholar in cultural studies when I confirmed that in Britain at least it is virtually impossible to get an academic position in anthropology without having conducted long-term fieldwork.

And yet, in many ways anthropology is successfully defending its right to fieldwork. It is perhaps no longer the *raison d’être* of the discipline in the sense that most anthropologists can be identified solely or even primarily as ethnographers. However, even though anthropological research on the basis of archives, texts, and visual artefacts as well as secondary sources is increasingly common, anthropology’s centre of gravity is still firmly located in fieldwork. In Britain, where the higher-education funding councils periodically evaluate all academic research, the Anthropology panel has established that engagement in long-term fieldwork can count as evidence of satisfactory research activity even if it has not yet resulted in any publications. At the same time, there are other conundrums in addition to those highlighted by Mintz. It is not easy to get across to new research students what fieldwork involves and what good ethnography depends on. Perhaps it never was, but these days young researchers expect to be in weekly if not daily contact with their own familiar world. Pressure to demonstrate “methodological rigour” is ever greater; at the same time, young anthropologists see researchers from other disciplines having a much less personally gruelling experience, and the difficulties of sharing the daily lives and practices of those one is working with are ever harder as the private sphere grows more private and encompasses more of the world’s population.

For Mintz it is the personal qualities of good fieldworkers that make them good—Mooney’s emotional en-



agement, genuine friendship between Barton and his Ifugao collaborators, self-effacement in the case of Richards, Anastacio Zayas Alvarado's double position as insider/outsider. I am sure this is true, but it would be a mistake to deduce from this that certain types of person make better fieldworkers than others. The common anthropological gossip of "it's hard to imagine X in the field" is misplaced. Anyone who wants to be a good fieldworker. It's just that wanting to be one involves a unique degree of personal commitment to subject one's own desires to an alien lifeway, to endure a high level of uncertainty and often discomfort, to be ignored or the butt of constant jokes or the recipient of outright aggression. And at the end of it is an impossible aim: to reorient one's intuitions so that they begin to coincide with those of the people one is working with.

As Mintz notes, it is no longer appropriate to present this strange vocation as a heroic vision quest. In fact, if viewed through the lens of an individualist utilitarian kind of ethic, it can easily be portrayed as a political outrage—making one-way demands on people, invading their privacy. Perhaps the most important thing about fieldwork is that it is an extension of and intensification of the everyday human relationships that we are all involved in unless we live as complete monads, and as such it is every bit as messy and ambiguous.

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Mintz humbly offers us this celebration of ethnography, for he does not press the case for his own wonderful fieldwork or his inspiring teaching of ethnography. Let us pause a moment to celebrate his contributions also. In his honor, I offer further questions about ethnography that complicate its performance and position in the discipline—and they are *not* about reflexivity and writing! U.S. anthropological romanticism (Wolf 1974[1964]: 11–12) is deeply shaped by the wonderful experience of ethnography. An earlier generation expressed this romance in depicting isolated and integrated cultures, but given the undeniable forces of change to which Mintz alludes, along with his valuable labors (with Eric Wolf, Angel Palerm, and John Murra) on historical approaches, current anthropology radically inverts its prior romanticism so that every manifestation of globalism, transformation, and irony is trumpeted, its novelty overstated. The two extremes are awkwardly juxtaposed, so globalism and localism remain conceptual opposites. This period of wide-eyed amazement at flux and flow will not last. Is a more enduring relationship between ethnography and world system to be had?

Let us suppose, as Mintz does, that anthropology draws on two very different self-conceptions, the totality of human history and the specificity of field research. We might then envision ethnography as a way of learning from the people who actually perform processes of wider

sweep. Concretely, we might ask what in the flow of activities and expression of ideas informs us about strategic processes that we cannot learn from taking a generalized and arm's-length view, biased by reliance on secondhand or official sources, marked by absence of a sense of practice? He finds in ethnography a deep and abiding interest in people that creates knowledge and understanding. This interest in activities and viewpoints transcends the stultifying effects of officialism on the study of grand processes. To build on Mintz's essay, we might further explore why antiformalism and deep substantiveness are so productive of rich and enduring theories.

An emphasis on ethnography of strategic processes poses the danger of reducing people to a single activity or quality, no matter how subtly described. As the cases recited by Mintz demonstrate, people perpetuate complex and rewarding activities even as productive or political duties squeeze them into narrowly functional roles or brutally constrained situations. Ethnography is our best way not to tear people apart, to remain humane in our vision of humans. Its holism holds not only for local webs of meaning but also for the equilibrium of activities, the allocation and interpenetration of time, tools, places, etc. (e.g., Heyman 1994). Yet there are compelling analytical advantages to abstracting strategic processes from whole lives. This poses for us a productive tension to which I suggest a possible resolution. Strategic processes value one-dimensional people over complex ones, and narrow activities and definitions of identity encroach on that equilibrium of whole lives expressed in Malinowski's (1973[1939]) concept of "needs." We need to document this process and its effects, and we also need to be circumspect about our scholarly reproduction of it when treating people as bearers of analytical labels, for instance, gender seen exclusively as power when the experience of gender is richly humane (Errington 1990).

Ethnography has had a provocative but ambiguous influence on the value assumptions of anthropologists. U.S. anthropology is profoundly influenced by the intensely friendship-based quality of fieldwork. Hence, in political controversies we tend to value the specific people and practices to which we bond in the field. Nancy Scheper-Hughes (1992, 1995) articulately offers localism as a value stance. Still, ethnography teaches us that local populations do not have one set of interests or speak with one voice, and also it seems to me difficult to sustain a value approach that commits various among us to diverse and often clashing peoples. As I wrote about the dilemmas of my fieldwork, "it mattered with whom I related, the frightened undocumented Mexican boy crossing the Otay mountains or the Border Patrol agent . . . bemoaning organizational politics that impede easy arrests" (Heyman 1998:9). Rather than deriving a moral compass on immigration issues from the empathy inherent in ethnographic encounters, I argued for a shared anthropological humanism. Whether that will prove a helpful position I do not know, but here I reconsider what ethnography does for our value engagements with extreme localism shed. The deep and abiding engagement

with people, the dual attitude of curiosity and respect that Mintz himself embodies, forcefully plants in our collective experience a vision of panhuman worth that underwrites a tacit and pervasive anthropological bias in favor of human rights (e.g., Mooney). That may well be ethnography's greatest reward.

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For those who admire Sidney Mintz as a Marxist scholar whose career has been devoted to the study of slavery and commodity fetishism, "Sows' Ears and Silver Linings" may seem surprisingly apolitical. Others, less sympathetic to his work, may find the treatment of transnationalism superficial and his sidelong attacks on reflexivity outdated. But both groups of readers will be wrong; Mintz's defense of fieldwork is a critical response to the present fragmentation of anthropology as a discipline and the concomitant diminution of its political impact.

For Mintz, a historical materialist, fieldwork is the product of labor, time, and raw materials. First, it is physically and mentally demanding: words like "skill," "craft," "effort," and "experience" appear throughout his account. Even when he lauds good fieldworkers for their "ethnographic sensibilities," "sympathy," and subjective "understanding," these are qualities achieved through "careful listening," "serious study," and "enormous effort." Secondly, time matters: good fieldwork is "lengthy," "long-term," and "painstaking." And thirdly, fieldworkers engage with and understand particular raw materials and manufacturers, as in Richards's careful documentation of sexual inequality through close attention to hunger and work and to land, labor, and diet. Good fieldwork, then, is like good cooking—an activity for which Mintz has an impressive reputation (Keiger 1998; see Mintz 1996). Jeff Tobin has observed that "employing a culinary model for ethnographic research . . . subverts the typical class relations between mental and manual labor. Cooking is usually considered manual labor, whereas ethnography is considered mental labor" (1996:9). Mintz wants to drag ethnography back from its heady theoretical preoccupations to the basic labor of researching what people make and do and how and why they do it. His own class origins, by the way, peep through here: he is telling us to do our work as his own father, a cook in a New Jersey diner, did his.

Mintz thinks such origins matter: he quotes John Swanton, who attributed James Mooney's "unflinching courage" in defense of a subjugated race or an oppressed people to "the circumstances of his ancestry" as the son of Irish immigrants. Taso, too, empathizes with "all of the people like him, . . . broken and consumed by crushing physical labor." Most anthropologists, Mintz points out, do not have class or ethnic backgrounds like these

men's or his own; this difference may account for conflicting views on the politics of *writing culture*.

Like his friend the late Eric Wolf, Mintz dismisses reflexive anthropology as self-absorbed: hence the laudatory comments about Richards's "self-effacement." He is less interested in the symbolic atrocities we might commit in our writing than in whether we are willing to be "an intemperate ally of the Indians" like Mooney. But here he gives anthropologists of a reflexive bent too little credit. In Amazonia, for instance, anthropologists of all theoretical stripes have been devoted activists on behalf of those they study; Alcida Ramos is among those for whom critical reflexivity in ethnographic writing and fieldwork is of a piece with her other political work.

Another political failing Mintz addresses is that anthropologists are poor pedagogues when they refuse to write for a nonprofessional audience. I am not sure that interdisciplinarity is the root of this problem or that the concept of culture can cure it, but I share his concern. By encouraging opacity of style, our heightened awareness of the politics of ethnography has fostered elitism. Anthropologists today speak unselfconsciously of a *we* that refers to other academics—their presumed only audience; Mintz's earlier writings speak to a *we* that includes all Americans including the subjects of his fieldwork (1974).

To move forward, anthropology must build dialectically upon its past, incorporating the insights both of historical materialists like Mintz and of anthropologists of a more interpretive bent. Many of the latter, after all, still ask questions about inequality and injustice even if the questions and the tools used to answer them have changed. If Mintz were to read the impressive series of recent books about masculinity in Latin America, for example, including (among others) Roger Lancaster's (1992) *Life Is Hard* and Don Kulick's (1998) *Travesti*, I think he would be surprised and pleased. Internally varied in subject matter and approach, these works display the same collective political passion for writing about injustice, for doing on-the-ground fieldwork among the dispossessed, and for the holistic integration of their particular topic with broader questions of culture, class, and politics that were the hallmarks of earlier ethnography.

The comparison between fieldwork and cooking is apposite if we consider recent trends in both. So-called standard ethnographies are as rare as everyday plain cooking, and just as the American bourgeoisie has developed a sophisticated ability to discourse upon haute and exotic cuisines, anthropologists have developed an appetite for elaborate theoretical dissections of the fieldwork process. In practice, however, quick and superficial fieldwork, sketchily presented in the form of anecdotes, is becoming more acceptable, just as on a daily basis we grab convenience foods while our gourmet cookware gathers dust. Among anthropologists, both behaviors reflect our uneasy class position as we aspire to a secure professional status that fewer and fewer of us attain. Mintz speaks for a group of anthropologists born into the working class who ended up at elite institutions; many in his audience grew up in the middle class and now,

uninsured and underemployed, earn far less than their parents.

Mintz's belief that anthropology is a form of labor and that it should remain committed to writing about other kinds of labor is a timely response to this crisis. The mythology of the "Information Age" rests upon a pretense that laboring bodies do not exist; fieldwork provides a means to rebut this false claim by documenting the lives of those whose underpaid and largely invisible work underwrites America's current prosperity. The triumphalist rhetoric of neoliberalism hides an impersonal and destructive set of policies that view the working poor—as well as scholarly workers—as inherently disposable. The restructuring it proposes has little room for anthropologists or their inconvenient habit of speaking up about uncomfortable social realities. As Mintz suggests, a reaffirmed commitment to the insistent historicity and obdurate materialism of fieldwork is our best possible response.

## Reply

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I am grateful to the commentators for perceptive and genuine criticisms. I want to refer to some of the things they remark on that I had failed to make clear in my paper.

Kevin Dwyer wants assurance that I'm not limiting my conception of ethnography to a simple connection between fieldwork and theory. I hadn't meant to; nor did I mean to argue that all anthropologists or all anthropological theorists must do fieldwork. I think that there have always been different kinds of anthropology, different kinds of fieldworkers, and different kinds of fieldwork. The profession has had anthropologists who contributed importantly to our discipline who either did not do fieldwork or did it indifferently. Some turned out to be superb chairs of their departments, some were theorists who read the ethnographies of others and built brilliantly upon them, some became historians of anthropology, and so on. The profession was advanced by them all; they all knew there was a profession.

Thus, for example, I would be surprised if anyone were to assert stoutly that Robert Redfield or Ruth Benedict was a great fieldworker. (In fact I believe that both Redfield and Benedict knew that their fieldwork was not strong and that they viewed this as a genuine shortcoming.) They both did other work, including theoretical work, of enormous importance to our discipline. At the same time, there are some living ethnographers the quality of whose work I find absolutely staggering. Yet they are not necessarily the best theorists in anthropology or the best anthropologists, either, for that matter.

I didn't mean to argue that every anthropologist must do fieldwork (even though Olivia Harris suggests that

the views in this regard may be more exigent in Britain than in the United States). Yet to me the significance of fieldwork in the epistemology of anthropology seems undeniable. The field would be crippled theoretically if we were to lose those theorists who build their work only on the fieldwork of others. We would also be much the poorer without colleagues who may do pedestrian fieldwork yet make important other contributions as teachers, advisers, administrators, and representatives of anthropology to the public. But I also think that we could not exist as a profession without any ethnographers. Then all of our data would soon be secondhand, and none of our students would need to learn how to do fieldwork. Under those circumstances I believe our discipline would lose one of its main reasons, probably its main reason, for existing. I have no doubt that ethnography will always be done by somebody, and at least some of it is going to be very good. But as Bourgois suggests, much of the best of it—probably a larger proportion of it all the time—is now being done by smart, sensitive non-anthropologists who have taught themselves how to ask good questions, how to hypothesize relationships, how to make friends, how to observe closely, how to cope with discomfort, and how to write. (I think Harris is right in asserting that anyone can become a good fieldworker.)

Ian Frazer hopes that I do not mean to omit theory from the ways in which I think about fieldwork. I hadn't intended to separate fieldwork and theory so coarsely. I think it is, on reflection, a major failing of the paper. But another word on ethnography as I see it may be of help, even though mine is very likely a minority view. The relationship between theory and fieldwork is commonly seen to be intimate. It used to be that we went to the field with a theoretical position in mind or to test a theoretical premise; that made it more than "just fieldwork." Theory was thought to raise our data to a different level; by using theory we specified the nature of our fieldwork ahead of time; later, our analysis of what the facts meant would not be possible without theory.

Much of that received perspective is now in disrepair or disrepute. It seems almost as if people wanted to have "the field" wash over them like a fresh ocean wave. If there is a new cardinal rule it is "Don't essentialize." Holding back on theory is one way of not essentializing. But nonessentialized fieldwork is not a new idea; much of it was done long ago, and some of it was excellent. Woodwork, ceramics, basketry, horticulture, housebuilding, hunting, fishing, cooking—no end of objects and activities were described by highly competent ethnographers who were not disturbed because the ethnography they did was unencumbered by theory. To name one example, Leslie White's ethnography was utterly divorced from his theories. Our libraries are full of such work.

I think a strong case can be made for that sort of fieldwork, which corresponds in some ways to one kind of natural history research, even if its theoretical yield, case by case, seems meager. The published outcomes may be arid and uninspiring; usually they fail to stimulate the generalizing mind. Yet Claude Lévi-Strauss tells us how

thrilled he was to read the annual reports of the Bureau of American Ethnology, and those certainly stimulated his generalizing mind. Many others find solid rewards in reading a competent description of how to build a canoe, put up a house, or catch a seal—all of it crucial to thorough documentation, even if there is not a large theoretical apparatus attached. It is a separate argument whether “thorough documentation” should be provided by us; I think it should.

But there are also the results of fieldwork that do count on connections to theory or are used to build theory. I need not provide readers with examples of those; we read them all the time, and some of us make our students read them, too. Perhaps many people are not so tolerant of purely descriptive ethnography as I am. But even if not, I think that substantial numbers of us still share a high regard for ethnography that does employ theory in its analytic aspects or that shows clearly how theory was employed in establishing the terms on the basis of which fieldwork was carried out. So I mean to say to Frazer that I view the links between fieldwork and theory as intimate, even though not all fieldwork may invoke theory in its design or in its conclusions. I think that lots of fieldwork, even some unilluminated by theory, can be good and useful if the ethnography itself is competent, serious, and documentable.

I think Don Donham is right on target when he says that “bureaucratic rationality, an emphasis on efficiency and production—in a phrase, the market—invaded academic life over this period [since World War II] in a qualitatively new way.” That is the unvoiced background to my case, and I wager that every field of rational inquiry about human behavior is undergoing a homologous process and similar pressures today. Donham’s detailing of the courtly, uncompetitive world he thinks we academicians have lost is not quite on the mark, if I have to rely on my own experiences of that “relatively leisured, elite existence.” His description applies better, I fear, to some movies I’ve seen about Ivy League institutions. But I think that it is true that academia is not what it once was, and I much doubt that it will ever be like that again. As for the time period involved (since World War II, he says), I think the end of the cold war produced a noticeable ratcheting up of previous trends. Corporate America now runs academia almost entirely. University parking lots, cleaning staffs, grounds maintenance, security, and what passes for food service are, more and more, the work of national corporations. It is not beyond imagining that they may soon supplant as well the hurly-burly of our annual meetings, our so-called slave market, with a much more efficient system of hiring and firing faculty. “Hey, George, Texas Gentile needs a physical anthropologist who specializes in *H. erectus*. Didn’t Euphoria State just let theirs go?” Our Association, assisted by Nike, Marriott, or some big fertilizer outfit, could continue to build in-group feeling (or maybe *in-group* in-group feeling) at the annual meetings.

The image conveyed by such grotesqueries is not wholly foreign to the ways we think about lots of things already. Were it ever to come to pass, it would be con-

sistent with Donham’s take on the direction in which capitalism is heading, and it would strengthen his hypothesis that “postmodernism” is more a result of “mundane changes” than itself a cause of the decline of the importance of fieldwork. I hope that in the future he will tell us in greater detail what he thinks those mundane changes amount to.

Juan Giusti views the recent history of anthropology much as Donham does and asks whether we have adequate fieldwork on the ways anthropology organizes itself. That’s a good question. Now that the American Anthropological Association itself has seen fit to campaign by e-mail for more nominations for a seemingly infinite number of nonsalaried jobs, it would be nice to have a session on how the AAA got to where it is, wouldn’t it? (Would you give a paper? Would you attend?)

Giusti raises a nice issue when he suggests that “‘untrained’ ethnography may be an essential interlocutor of ‘trained’ ethnography and anthropology.” I guess so, but why “essential”? Any careful reader of Mooney, Barton, or Richards—the list could be much longer—needs no intermediary in order to understand. Of course there have always been different writing styles, different analytic perches, different objectives. As long as writers write well and put comprehensibility high on their lists, I don’t really think we need amateurs in order to understand professionals. If none of the professionals can be understood, then even amateurs aren’t likely to be much help. But if Giusti means that the same ethnographic task can be done differently and equally well by different people, I just know he’s right. Others more sensitive to literary genres will have thought fruitfully about this. Life history might be a sphere in which the differences among variant sorts of description might be important to read and understand in relation to each other, and some of those are certainly the products of “amateurs.” But that’s something I need to learn more about.

Joe Heyman writes movingly about the sort of fieldwork that involves the fieldworker in contrary or contradictory human situations, and he comes up with a humanistic vision of the possible. Like the other commentators, he has no doubts about the worth of ethnography itself, but he points to the dilemmas to which it so often gives rise. The polarities of localism and globalism pose special questions for ethnography, as Heyman tells us. He exposes an issue: how to get at the “strategic processes” yet do so in a way that lets us learn the immediate, the personal, the everyday, the richly human ways in which those processes are experienced, interpreted, and reacted to by “real people.”

This is certainly difficult. I remember from my youth a *Daily Worker* cartoon by a brilliant cartoonist who signed himself “Hoff” in the *Worker* and “Redfield” in *The New Yorker* (or the other way round). It depicted a man sitting on a bed in his undershirt, his cop’s hat hanging on the bedpost, saying to his wife, “God, am I tired. I’ve been clubbing strikers for nine hours.” There, it seems to me, is the problem with the immediate, the personal, and the everyday, revealed by the living, breathing mortals we come to know, as we are getting

at the strategic processes. But I know that Joe Heyman knows this already.

Still, it is worth trying to do, time and again, as Heyman himself has shown in his work. The local and the global are not just polar opposites. It is because we can get new information from individuals who are willing to help us or at least socialize with us that the personal, the everyday, and the human become available to us in the field. Other disciplines get their data otherwise. We get it that way, whether we're in some little village somewhere or traveling with people on their way to sell or buy 10 or 10,000 miles away. The local may be inside the global, or it may be quite decidedly unglobal, in spite of all the brouhaha. We may work at seeing the local and global in relationship to each other, and I think that at times we can overdo their opposition.

I was recently asked by a former student, himself now an emeritus professor, whether anyone had noticed that two books I'd written were at the extremes of the same subject: one a Puerto Rican life history and the other on the global transformation of a product, sucrose. I replied that I thought the two books were really pretty much alike—differing mainly in that one turned on ethnography and the other did not. The life history, I thought, aimed to reveal how one man had experienced the transformations, some of them global, that the other book described. Heyman's work, it seems to me, does similar things. His affection for fieldwork and his desire to do it better are what matter to me here. Dissatisfied with my own attempts, I recall the Welsh composer Ralph Vaughan Williams, who said that anything worth doing at all is worth doing [even] badly. I intend to keep trying.

Mary Weismantel's pen is perhaps the sharpest and most penetrating of all. She justly catches me out in passing over the progressive work of reflexive anthropologists, and she calls to my attention two recent works on masculinity in Latin America (which I had in fact read, and liked). I do applaud her insistence that "to move forward, anthropology must build dialectically upon its past, incorporating the insights both of historical materialists . . . and of anthropologists of a more interpretive bent." Weismantel's allusions to labor and to cooking as labor help me to understand better my own motives, and I thank her warmly for those. She may ennoble my origins, though. My parents were both members of the working class, yes, but my mother's ambitions for her children were, I fear—and despite her politics—unabashedly petit-bourgeois.

Carla Freeman suggests that the real question my paper raises is "that of the shape of ethnography itself—what we are or should be intent on salvaging, cultivating, and even sharing . . . how we might better train anthropologists and nonanthropologists alike to do rigorous fieldwork." Yet, that is the question, if the central importance of quality fieldwork is honestly acknowledged. Freeman thinks that recent critiques have deepened and enriched the doing of ethnography. I think that Freeman, like Weismantel, is a solid ethnographer. Both are telling me to open my eyes; I promise to try.

Finally, Kevin Dwyer says I'll wince when I read his

assertion that there are many ways to skin a cat. Well, yes, there are, and yes, it does make me wince. (My beloved Marcello, purring next to me, would wince, too, if he understood.) But—and now ethnographically speaking—why skin a cat? And how many ways? What do we know of the tools employed? Are there age or gender distinctions when it comes to eating the cat? And what (besides banjo strings?) do they use the skin for? And is that usual, or exceptional? And how do we know? We anthropologists have a heritage of our own. Our predecessors not only told the world but also showed the world that all peoples are equal—equally human, equal in what they are, equal in what they have done for humankind. Nobody else at that time had said it and demonstrated it; anthropologists did. It does not befit us children of that enlightenment to turn our backs on the method that was used to make those ideas accessible to all of us.

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