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Report on the Field

The Anthropology of the U.S.: **Cited Not Nearly Enough and Still Earning Respect**

By Kate Masley

Abstract: This article provides an overview of some significant trends and developments in the anthropology of the U.S. The author, a young North Americanist, highlights the ongoing need to acknowledge and cite the extensive body of work that seems to get lost with each new cohort of anthropologists seeking to do ethnography "at home" in the U.S. Tracing the origins of American anthropology to its "Indianology" roots, the author sketches changes that came with World War II and the post-war generation of anthropologists calling for new, U.S.-based research. The role of black anthropologists in developing an activist anthropology located in the U.S. is often overlooked and underrated. The work of AAA sections such as SANA and ABA have helped provide a corrective to the erasure of pioneering scholarship based on U.S. research and of the pioneers and those who followed them. As more anthropologists engage in U.S. fieldwork, we must build on the scholarship of our anthropological forebears, especially those who may not have received prominence in their own time, but whose works have proven to be visionary.

Key words: U.S. anthropology, North America, urban, history of anthropology

In his "Report from the President" (NAD 2006: 14), SANA President Jeff Maskovsky notes "...the pioneering scholarship in our field is still not taught or cited as frequently as it should be. Indeed, it is still possible to read new monographs based on U.S. research that ignore the extensive ethnographic literature on the field. We must not allow the weakening of the area studies rubric to erase the deep understanding of the American scene that has been produced by decades of excellent scholarship." In this article, I seek to acknowledge that this extensive scholarship exists, and provide an overview of some significant developments and shifts that have taken place within the field. By no means is this review meant to be exhaustive, but is intended to provide an outline of patterns in the growth and development of the anthropology of the U.S.

Evolutionist Lewis Henry Morgan (1818-1881) was one of the very first scholars to conduct anthropological research in the United States, studying over seventy Native American groups (Bohannon and Glazer 1988). Like Morgan, Boas, Lowie, and Radin contributed to the anthropology of North America with their work on Native Americans, although theirs was an historical-particularistic approach, leading to a focus on religion, folklore, mythology, social organization, kinship, ceremonies, and customs. From the 1870s through the 1930s, an enormous number of research studies were conducted among Native American groups such that American anthropology could have been referred to as "Indianology" (Kan 2001).

In This Issue

Articles

- 1** **Report on the Field:**
The Anthropology of the U.S.: Cited Not Nearly Enough and Still Earning Respect (Masley)
- 6** **Report from the Field:**
***“The Other” and “The Enemy”:
Reflections on Fieldwork in Utah (Brugger)***
- 11** ***Casting Out Demons:
The Native Anthropologist and Healing in the
Homeland (Ceja-Zamarripa)***
- 15** **Report from the Editor:**
***Fieldwork on the Home Front:
Danger Zones and Insight at Critical Intersections
(Waterston)***
- 18** ***Of Hats and Switches: Doing Fieldwork “at Home”
(Nordquest)***

Announcements

5 & 10 **SANA Spring Conference**

Book Gallery

- 4** ***Taxes Are a Woman’s Issue***
14 ***Making Democracy Happen***

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The 1915 volume *Anthropology in North America*, written by Boas and colleagues, illustrates the depth, detail, and scope of anthropological scholarship of that period. The main focus of anthropological attention was on Native Americans who were perceived and depicted as the “exotic other” and whose groups were presumed to be homogenous. According to Beatrice Medicine, anthropologists were eager to document these “vanishing Americans” before they were no more. She notes that the Boasian emphasis on Plains Indian reservations and Northwest Coast villages was to “recapture ‘memory cultures’ that reflected the ‘golden days’ of Natives whose aboriginal culture was denigrated and whose future was seen as oblivion or civilization.” As a result, Medicine notes, “Many felt that American anthropology was built on the backs of Natives” (2001: 4).

Roots of U.S.-Centered Anthropology

During the early and mid-twentieth century, sociologists had a jumpstart on anthropologists by being the first to consider urban life a significant area of study, and to conduct systematic research among different socio-cultural groups in the U.S., particularly immigrants living in cities. During this period, most American anthropologists were eager to distinguish and separate themselves from sociologists and to preserve the anthropological rite of passage of fieldwork abroad. As a result, mainstream anthropological research on cultural phenomena in the U.S. was slightly delayed in its development (Beals 1951: 3). The U.S.-centered works of black anthropologists such as Nora Zeale Hurston and John Gibbs St. Clair Drake (who were among the handful of black anthropologists from the pre-WW II period), were pioneering on this as well as other fronts.

Studies of U.S. cities that came out of the University of Chicago had a significant impact on the formation of the anthropology of North America. The Chicago School, led by urban sociologist Robert E. Park and his colleagues, considered the city a “natural ecological system.” In *The City*, a classic structural-functionalist work, Park and colleagues argued that economic, ecological, and cultural processes were fundamental to shaping the nature of cities. These

studies, rooted in the positivist tradition, focused primarily on urban “social problems” manifest by members of the lower classes, and reinforced an assumed relationship between “social deviance” and the lower classes, approaches which were to have lasting effects on urban social policy as well as research (Waterston 1993). Anthropologist Robert Redfield, a University of Chicago graduate, was deeply influenced by the Chicago school. Redfield helped bring the study of cities and urbanism to anthropology, and he acted as a springboard for anthropological research on urban life and cultures in U.S. cities (Press and Smith 1980a).

WW II Era: Influences on the Field

World War II helped initiate the shift in anthropological research from “isolated tribal societies” to “agriculturally based communities” and “complex societies.” Edwin Eames and Judith Goode explain that “The war...generated a variety of anthropological activities which, under other historical circumstances, might not have developed as quickly or in quite the same direction” (1977: 7). Applied anthropology, for example emerged and became more established during this period. And because it was wartime, many anthropologists were reluctant to travel abroad. According to Eames and Goode, the sociopolitical climate during World War II had a tremendous influence on the formation of urban anthropology and the anthropology of North America.

In the decades following the war, two publishing events illustrate the American anthropological turn to studying one’s own society and culture. A special volume of American Anthropologist was published in 1955 titled “The USA as Anthropologists See It,” which contains articles by Conrad Arensberg, Cora DuBois, Solon Kimball, Walter Goldschmidt, and Melford Spiro, among others, on topics such as U.S. ethnic groups, immigrants, class, communities, values, acculturation, and urbanization. This volume provides a revealing look at the kind of anthropological research being done on the U.S. at that time, and what our predecessors identified as areas in need of further study. In The Nacirema, published in 1975, editors James Spradley and Michael Rynkiewich stood firm on the view that anthropologists be encouraged to conduct fieldwork in the U.S., not be denigrated for it.

Arguing that anthropologists have seriously neglected American culture as a topic of study, they urged anthropologists to take up the U.S. as a viable and critical fieldsite.

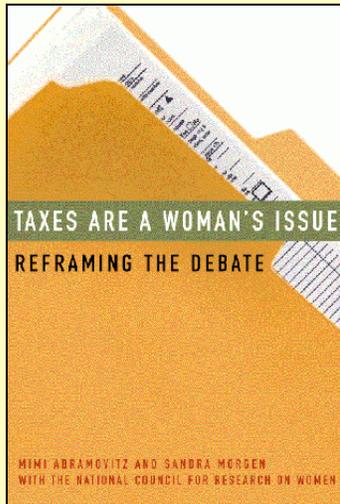
Anthropological Studies of Poverty

More recent literature reflects on the impact of Oscar Lewis on anthropological scholarship of poverty and the cultures of the United States. Goode and Maskovsky (2001) describe Lewis’s work as a catalyst for building the field of poverty studies. Negative stereotypes, victim-blaming, and other weaknesses mark Lewis’s notion of the “culture of poverty.” Nevertheless, as Goode and Maskovsky note, Lewis’s work sparked debate and pushed anthropologists towards a political economic analysis of poverty.

During the 1980s, the debate shifted from Oscar Lewis’s “culture of poverty” (1965) to the “underclass debate,” which was initiated by William Julius Wilson’s 1987 publication, The Truly Disadvantaged. As a result, more political-economic analyses of poverty emerged, and an increasing number of urban ethnographies began to surface in the 1980s, such as works by Ladner (1971) and Stack (1974) as well as Susser’s (1982) rich ethnography of a Brooklyn working class neighborhood which situated participants within a historical, political, and economic framework and viewed people living in poverty and in the inner-cities as active agents who engage in collective action. Also during the 1980’s, more anthropologists began investigating immigrant groups and enclaves within cities, including Robert Alvarez’s study of Mexican-Americans (1987) and Iris Lopez’s work on Puerto Rican women in New York Lopez (1987).

The 1990s ushered in dialogues and debates on race and on the complexities and intersections of race, class, and gender. Faye Harrison’s call for the expansion of the anthropological discourse on race (1995) and Lee Baker’s call for an increased anthropological awareness of the “color-blind bind” (2001) represent important contributions to the anthropology of North America. In their 1999 edited volume on African-American Pioneers in Anthropology, Harrison and Harrison helped bring out of obscurity the many black scholars who broke new ground in activist anthropology, as well as in U.S. anthropology, including Allison Davis and

BOOK GALLERY



TAXES ARE A WOMAN'S ISSUE Reframing the Debate

Mimi Abramovitz and Sandra Morgen
With the National Council for Research on Women

Book Description from Feminist Press, CUNY:

Taxes determine the quality of our lives; they are responsible for the health of our environment, the safety of the roads we drive on, the condition of our public services, and the security of our homes and communities. For every woman who pays taxes and uses public services, and every man who cares about an effective and fair tax system, *Taxes Are a Woman's Issue* dares to expose not only how tax policies shape the size of our bank accounts but also sculpt our government and the nation's identity.

Whether you are rich, poor, a corporation or an individual, taxes provide the resources we need to sustain the nation's civil, social, and economic life, and help support the basic welfare of all individuals and families. They also mirror the fundamental inequities that people of different races, classes, and gender experience when they try to access the opportunities that taxes provide. So when probed by the lens of women's diverse experiences, tax policy narrates some of the ruthless realities of our economy and our society.

Authors Mimi Abramovitz and Sandra Morgen, writing for the National Council for Research on Women, convincingly dispel myths about the current welfare system and expose how the IRS-supported tax system was created in, and caters to, a time before women entered the work force. By honestly discussing the many ways the current tax system disadvantages women, *Taxes Are a Woman's Issue* courageously teaches, as Linda Basch, the President of the Council, states, "about positive changes that will improve the lives of all women and therefore their families, their communities, and the nation as a whole." (2006)

Katherine Dunham of the University of Chicago, Arthur Huff Fauset (Pennsylvania), and W. Montague Cobb of Case Western Reserve (see also Jordan 1990).

In an article on poverty among African Americans in the U.S., Brett Williams urged anthropologists to follow the course fashioned by many of these black anthropologists and take on interdisciplinary and engaged projects in rich ethnography and social action. "We have offered almost no concrete proposals for how to eliminate poverty," Williams observes, "and the range, quality, vision, and policy implications of our work have been ignored both in our discipline and by poverty/policy researchers in other fields" (1992: 165). From the 1990s to the present, more and more anthropologist-scholars and activists have directed their attention to poverty, welfare reform and homelessness. We now have many U.S.-based studies that provide in-depth ethnographic investigations on critical issues that incorporate advocacy, social action, and political-economic analyses - "experience-near ethnographies" in which anthropologists carefully and successfully describe the tangles, intersections, interweavings, linkages, problematic areas, and inextricable ties between the various threads that make up our lived experiences on multiple levels (Cassell 2000).

SANA has played an instrumental role in institutionalizing North Americanist anthropology and the study of the U.S. by American anthropologists. Just as the Association of Black Anthropologists (ABA) has helped move black anthropologists from the margins to the center, SANA has helped bring the anthropology of the U.S. to a central position in the discipline.

As more anthropologists engage in fieldwork "at home," we must build on the work of our anthropological forebears, especially those who may not have received prominence in their own time, but whose works have proven to be visionary. We must look back to those who, like John Gibbs St. Clair Drake and Beatrice Medicine, paved the way, teaching us how to "be an anthropologist" and "remain native" (Medicine 2001). In citing only a few among those anthropologists, this essay offers a snapshot of this rich and "excellent" scholarship.

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Report from the Field
"The Other" and "The Enemy":
Reflections on Fieldwork in Utah
By Julie Brugger

Abstract: This paper is a reflection on doing anthropology in the United States, based on my research of the Grand Staircase-Escalante National Monument, a "protected area" in southern Utah. My research focuses on the meaning and practice of democracy in the United States by examining the impact of conservation policies on rural resource users. I question why social scientists who study conservation are able to see injustices in the protected area model when applied in "the Global South," but have not aimed critique at similar processes occurring in the U.S. Reflecting on the post fieldwork experiences of scholars Susan Harding, Faye Ginsburg, and James McCarthy, I suggest that, for American anthropologists, some "repugnant others" in the U.S. represent a threatening "enemy," while in other settings, they may not be perceived in this way. I conclude by suggesting we "write democratically" in order to overcome this limitation and realize the transformative potential of ethnography.

Key words: Grand Staircase-Escalante National Monument, conservation, protected area, resource conflict, democracy, ethnography, U.S.

Out of Africa

When I entered graduate school in anthropology, I planned to do fieldwork in South Africa. I had spent more than a year there at two different times and was present during the first democratic elections in 1994 - a profound experience for me. I wanted to study the developing meaning and practice of democracy in South Africa by examining the management of a national park or protected area. However, when I went back to South Africa in 2001-2002 to choose a specific field site, local residents repeatedly confronted me with the question: "What's in it for us?"

South Africans are very politically informed and astute, likely a result of their years of struggle against apartheid. Since 1994, they have been inundated with researchers from the North. In the aftermath of 9/11 and the U.S. "war on terror," animosity toward the U.S. has intensified. The subtext of the South African question seemed to be: Why are you coming over here to study us when YOU are the problem in the world today?

I could not offer a satisfactory answer to this question, and could no longer find justification for my project in South Africa. Instead, I chose the Grand Staircase-Escalante National Monument (GSENM) in southern Utah as the site of my research, but the topic, inspired by my South African experience, remained the same: democracy. The erosion of some of the accepted pillars of democracy in the U.S. - individual rights, transparency, free and fair elections - since I began my fieldwork indicates that the U.S. may be in greater need of insight into what democracy means than is South Africa. Julia Paley would concur. She argues that anthropologists need to critically examine democracy, "in places whose governmental systems have not been subject to massive change" (2002: 470). Paley underscores the need for "an interrogation of Western political ideals and institutions...given that the United States is regularly taken as the unexamined standard bearer for the rest of the world" (2002: 471).

Sensitized by my experience in South Africa, I asked the people in Utah with whom I wanted to work what they expected to get out of my research. "Just tell our story," people told me, "We just want our voices to be heard." I felt this was something I could do.

However, in carrying out that responsibility I have become aware of unspoken assumptions about "doing anthropology" in the United States and about complexities in the political sympathies of anthropologists.

Models of Conservation: Assumptions & Implications

In conducting research in a U.S. protected area, I joined the growing ranks of anthropologists, geographers, and other social scientists studying conservation worldwide. Many of these scholars critique current conservation ideology and practice which sees the formal designation of protected areas, exemplified by national parks, as a most effective and efficient solution to the problem of dwindling resources and biodiversity. Sometimes referred to as "the Yellowstone model," a centralized authority protects "nature" inside designated boundaries by excluding or tightly regulating resource use. Within the boundaries, "nature" is there only to be viewed, enjoyed or studied.

Some critiques of the protected area model of conservation center on its underlying “Western” assumptions about the relationship between humans and nature, and about whose ecological knowledge counts. Other critiques point to a four-point pattern of injustice common to the model currently in practice: 1) creation and management of protected areas are usually top-down whereby local people have no say; 2) local people, economically marginalized and dependent on resources in protected areas for their livelihoods, are often displaced or lose access to resources; 3) locals do not share in the economic benefit that results from the protected area; 4) locals are “demonized” by the state or conservation organization that is trying to gain control over resources it wants to “protect” from local inhabitants who use them. There are also questions about the efficacy of the model.

A majority of social scientists studying conservation focus on protected areas in the “Global South,” where poor rural resource users are being displaced or further disadvantaged by the creation of protected areas. As an alternative to the Western model of conservation, these social scientists propose a “community conservation” model based on the premise that when local people have a say in its creation and management, and a share of its economic benefits, conservation projects will be more just and more effective.

As I became more familiar with the social science literature on protected areas, I came to a striking realization: the attention and concern of these social scientists (with some notable exceptions) did not appear to extend to those living in proximity to protected areas in the United States. I wondered why this might be, since my research demonstrates that the people living adjacent to GSENM are experiencing many of the same injustices as rural resource users in or near protected areas in the Global South.

Grand Staircase-Escalante National Monument & the Four-Point Pattern of Injustice

The 1.9 million-acre Grand Staircase-Escalante National Monument (GSENM) in southern Utah where I have been conducting research since 2000 was created by Presidential Proclamation on September 18, 1996. In a surprise move, just before he was coming up for re-election, President William J. Clinton stood on the rim of the Grand Canyon in Arizona and, using the power granted to

him by the 1906 Antiquities Act, proclaimed the largest national monument in the lower 48 states. In creating the Monument, he was following a precedent set by other Presidents before him to bypass the legislative process required to create national parks and wilderness: a precedent that has been repeatedly contested locally and challenged judicially and legislatively (Righter 1989).

Local residents and Utah politicians, who were neither consulted nor informed of the President’s intentions beforehand, were outraged. They see this action as illegitimate and undemocratic because it overstepped the bounds of presidential authority and ignored the views of those citizens that it most affected. Local opposition to the Monument is based on the fear that the new designation will result in greater restrictions on grazing and other traditional activities in the Monument, threatening rural livelihoods and culture.



Photograph by Julie Brugger

Rural residents of the sparsely populated, arid, and rugged region are politically and economically marginalized. They are a minority in a population that is less than 25% rural and where less the 3% of the workforce is in primary production. Per capita income and average earnings per job in Garfield and Kane Counties, where the Monument lies, are well below the national average. Local residents have traditionally depended on timber, mining, and livestock grazing on lands that are nearly all federally-owned. While this is less true today, many local residents still augment their income by raising livestock, and many identify with a local “custom and culture” associated with ranching.

Local livelihoods and culture are also threatened by the general, ongoing decline of resource-based

rural economies in the American West. As jobs in ranching, timber, and mining decline, the main local industries have become tourism, recreation, and real estate. The tourism and recreation sectors typically offer low-paid service employment, while the real estate industry tends to generate tax increases. In the city of Kanab, adjacent to GSENM, many people were out of work in 1996, when the Monument was created, due to the recent closure of a lumber mill and a uranium mining company in the area. The community was looking toward the opening of a planned coal mine on the Kaiparowits Plateau for well-paid jobs that would revive the flagging local economy. When that area was included in the new monument, prospects for well-paid employment in the area disappeared.

Local residents have also lost access to historically used resources now within GSENM borders. A new prohibition on gathering fuel wood in the Monument has adversely affected local residents who heat their homes with wood in the winter. Residents can no longer dig gravel for road projects from the nearby washes, or gather clay for lining canals and ponds from clay pits they have had access to and used for years. The new designation also brought with it restrictions on the size of groups permitted to enjoy the park which interferes with the local tradition of "Eastering," outdoor picnic gatherings of extended families at favorite spots, now on the Monument.

Ranchers have been affected in more subtle ways. The number of hikers who visit the red rock canyons where their cattle graze has increased. Many of the hikers object to cattle grazing on these lands. Ranchers have had their line shacks burned, their cattle shot, and gates left open. Some have sold their permits on the Monument and moved elsewhere. Ranchers also report subtle changes in management that make it more difficult for them to operate. For example, they can no longer cut cedar posts for fences, they are restricted to certain times of the year for salting their cattle, and they have to go through official procedures that oftentimes take years in order to make range improvements. According to one rancher, "It's a lot of small little things" that add up to a lot of uncertainty. "So between the uncertainty and their regulations, what's it's done is just stifled the business."

While local residents have lost access to resources, the gains promised by promoters of GSENM have not been realized. One local

government official explained: "Arguments for the national monument...the ones that shouted the loudest were, 'Oh, you'll do so much more on tourism. It'll just be great. It'll be a lure for people. People will just come.' Well, statistics have shown us that that hasn't happened."

Misinformation and stereotypes of public land resource users abound. Local residents have been "demonized" both by the media and by environmental organizations that support the Monument, portrayed for example, as lawless extremists, vigilantes, and provocateurs. Environmentalists seem to imagine that small ranchers are "cattle barons," an image that rides alongside the one about the "welfare rancher," the cowboy on his ATV whose destruction of public lands is being subsidized by the federal government.



Photograph by Julie Brugger

But the media and environmentalists are not the only groups to hold these stereotypes. To my dismay, I have found many among my colleagues and friends in anthropology who dismiss the concerns of locals like those with whom I am working.

The four-point pattern of injustice is clearly present in the GSENM case. The prevalence of misinformation about and misrepresentations of local people may help explain the lack of understanding among these anthropologists, but does not sufficiently explain their lack of interest in the plight of rural resource users. In the following section, I consider why this may be so.

"The Problem of the Repugnant Cultural Other"

In reflecting on her study of fundamentalist Christians in the U.S., anthropologist Susan Harding wrote about "The problem of the

repugnant cultural other.” “The continuous inquiry by my colleagues into my background and my motives for choosing this and not some other, any other, ethnographic object,” led Harding to realize she had violated an unspoken convention that it is better for anthropologists to study “some ‘others’ and not other ‘others’...specifically those deemed inappropriately religious or otherwise problematic or repugnant” (Harding 1991: 375). Anthropologists would find “others” who are “inappropriately religious” repugnant because most anthropologists are not religious or if they are, at least they are “appropriately” religious. Harding argues that this policing of the boundaries of “otherness” has profound implications for anthropology: it leaves many of our “modernist presuppositions,” cultural stereotypes, and prejudices about “repugnant others” intact. A majority of the residents of the communities in southern Utah where I do my research are Mormon, but I don’t think it is simply their religion that spawns indifference to their situation. I don’t think Harding quite grasped what it was about certain “others” that makes them “repugnant” to anthropologists. If she had been studying evangelical Christians in Africa or Mormons in Tonga or Mexico would her research have produced the same reaction?

Faye Ginsburg, who studied the conflict over an abortion clinic in Fargo, North Dakota, also found herself “disciplined” by her colleagues. What made the “others” she studied “repugnant” was their “pro-life” stance, when most anthropologists are likely pro-choice. But Ginsburg also discerns that what makes her research problematic for some anthropologists was that these “natives” were not “at some cultural or historical remove” (Ginsburg 1998: xxxiv). They were “the enemy” in a “social and political conflict” where most anthropologists were on the opposite side, and this “enemy” could wield power in ways that might have direct impact on her colleagues’ lives.

Geographer James McCarthy, who studied the Wise Use movement, added another dimension to my understanding of the hidden assumption about doing research in the U.S. Wise Use is a term that encompasses a range of efforts by rural commodity producers to maintain their access to federally owned lands in the rural American West in the 1980s and 1990s. In an article espousing the application of political ecology theory and methods

developed in “Third World” contexts to resource conflicts in the “First World,” McCarthy observed that those who do research on international conservation and their audience, “nearly always share political and theoretical sympathy towards the rural resource users and a deep skepticism of the state and large conservation organizations” (McCarthy 2002: 1281). However, in the case of Wise Use, they did not. McCarthy proposes that this is “a result of geography:”

Wise Use was located in the United States - a late capitalist, ‘First World’ country - rather than in the ‘Third World.’ In consequence, I believe, most observers evaluated Wise Use within a very different analytical framework, according to a different set of assumptions and norms, than they would have applied to a group making similar claims in a Third World country (McCarthy 2002: 1282).

Rural resource users in the United States tend to be politically conservative. Utah, where I do my research, is the reddest of the red states. In conflicts over the use of public land in the U.S., rural resource users fight for economic and representational control of the same physical space as do environmentalists. Because these “ruralites” are “the enemy,” those who consider themselves pro-environment will judge them according to a double standard and, among other things, call for top-down (federal government) regulation of their activities.

Putting all of these pieces together, I came up with a plausible, but disconcerting, explanation of why so few anthropologists have taken up the issue of rural residents who are adversely affected by protected areas in the United States. These “others” represent “the enemy” because they are a “close opposition”: white, conservative, and politically powerful. For American anthropologists, a majority of whom are “liberal” (Kurtz 2005), to study and work with this “repugnant other” might lead to sympathizing with “the enemy.”

Uncovering this assumption should compel us to examine more closely the viewpoints, values, and cultural stereotypes that influence where we choose to do fieldwork and how we interpret what we find when we get there. This applies not only to anthropologists doing research “at home,” wherever that may be, but anywhere we choose to work.

Writing Democratically/ Democratizing Ethnography

Where does this reflection on “the other” and “the enemy” lead me? Considering my interest in democracy, I seek to “write democratically” (Nafisi 2004: 132), and to participate in the “democratization of ethnography” (Paley 2002: 488). I also seek to respect my informants’ request that I “just tell” their story.

Chantal Mouffe argues that the aim of democratic politics is to transform a struggle between enemies into a struggle between adversaries. An adversary is a “‘friendly enemy,’ one with whom we have some common ground because we have a shared adhesion to the ethico-political principles of liberal democracy: liberty and equality” (Mouffe 2000:102).

For me, writing democratically means allowing my readers to come to see these “others” as adversaries, not enemies. Towards that end, I must downplay the explanatory, authoritative voice of the anthropologist and provide a space for the people of southern Utah to tell their own stories. In this way, my anthropologist-readers can come to know them as I do, even if it means contradicting preciously held beliefs. This is the transformative potential of ethnography: by writing democratically, we promote democracy.

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Link to Preliminary Program:

<http://sananet.org/2007conf/forms/SANA%20Preliminary%20Program%202007.pdf>

Check This Out!

The following is a sampling of panels and roundtables to be presented at the upcoming Spring SANA meeting from April 19-21 in New Orleans:

**Failure to Prepare:
Seeing Disaster Like a State**

**Primitive America:
A Dialogue with Paul Smith on
Ideology of Capitalist Democracy**

**The Anthropology of Displacement and Loss:
Narratives of Life in Southern Communities**

**Local Knowledge:
New Orleans Artists and Activists Reflect on
the State of the City after Katrina**

**Women, Violence and Unnatural Disasters:
Rethinking Core Concepts**

**Natural or Unnatural Disaster? The Neo-
Liberal American City**

**“In the Eye of the Power”: The Challenge of
Daily Journalism in New Orleans**

**Rescue, Recovery, and Restoration of
New Orleans’ Cultural Traditions:
A Post-Katrina Discussion**

Queering the Disaster

**Anthropological Research for Social
Movements**

**Mediating Disaster: Representing and
Forgetting Cultures**

**Commodity Fetishism, Neo-Liberalism and
Disaster in Global Perspective**

***Casting Out Demons:
The Native Anthropologist and
Healing in the Homeland***
By Tanya L. Ceja-Zamarripa

Abstract: This article addresses academic and social costs experienced by anthropologists studying their own ethnic group. It explores how one “native” anthropologist navigates her roles as ethnographer and insider while researching *curanderismo*, a religiously inflected form of ethnomedicine within increasingly secular and commercialized Mexican American urban spheres. Is academic credibility weakened because the anthropologist shares the cultural history of her/his informants? When your community entrusts you with their spiritual, emotional and social woes, do they see you as ethnographer, insider, or both? To be privy to the ritual knowledge and practices of healers and the individual struggles of clients to find respite from pain is a great responsibility as *curanderismo* has often been pathologized by anthropology as a “primitive” tradition used only by the ignorant and backward. Given this history, the native anthropologist must find a way to manage allegiance to her cultural as well as academic community. I suggest that doing “native” research is its own form of “exorcism,” casting out demons in a field that often silences native voices and holds native anthropology in lower esteem.

Key words: Native anthropology, *curanderismo*, Mexican American, South Texas, ethnomedicine, medical anthropology

I was born a believer. My grandmother fought the American doctors who said my mother’s young legs would be amputated due to complications with osteomyelitis. Instead, my grandmother gathered family members to dress my mother in saint’s clothes, pray at her bedside, and wash her legs in a mixture of sacred herbs. Her recovery stupefied her doctors. She kept her legs. Life in my Mexican American neighborhood was interspersed with such stories: using *limpias* (cleansings) to cure *mal ojo* (evil eye), healing open wounds with spider web and summoning wayward spouses with love spells and prayer. These stories reflected a religiously inflected form of ethnomedicine called *curanderismo* in which illness is not confined to germ theory, but also allows for supernatural etiologies and interventions. *Curanderismo* is ripe with ghostly hauntings, pilgrimages to shrines of

famous *curanderos* (folk healers) and “culture bound” illnesses like *susto* (magical fright). So, I set off on an ethnographic odyssey amongst my own people in South Texas.

When I am invited to speak to classes, social service agencies or medical schools regarding *curanderismo*, I am asked, “Do you really believe in those things?” and I find myself hesitating to answer. If I say “yes,” will my research seem too subjective or appear to lack the rigor of classic anthropology? While our discipline defends the merits of cultural relativism and the “native point of view,” the belief persists that it is okay for them to believe such things, but for you (the anthropologist) to believe those same things seems taboo. After all, miracles and folk magic are not easily quantified in positivistic paradigms.



photograph by Tanya L. Ceja-Zamarripa

**Egg used in *limpia* (spiritual cleansing)
The blood and straight pins are considered
evidence that a *mal* (evil or bad) was extracted
from the client.**

What does it mean to anthropology that the anthropologist’s whole life could be characterized as one continuous participant-observation? Yes, herbs, *limpias* and even tales of exorcism punctuate my life, but does that mean that as a native anthropologist I do not possess the ability to question, reason and analyze that which is taught or told to me? Is this not the very essence of academic identity?

This article addresses the tense interplay between the roles of ethnographer and cultural insider, to cast out demons that confine “the best” anthropology to the study of the “other.” Whether you study people far removed from you or your cultural intimates, the enterprise of telling the story of any culture is a dangerous one, marked with the responsibility you carry for turning people’s lives into research.

Who will be my primary audience? My research subjects? The academy? Both? After all, my dissertation will not be read by native informants far removed by oceans or time zones -- it will be shared with cultural intimates at my kitchen table. While I could go home if I were banished from the Ivory Tower, where would I turn if driven out of my homeland? Do I have the academic fortitude to write against cultural stereotypes of a community overwrought with spirits and spells? To that end, I think of Zora Neale Hurston (1935, 1969), the pioneering native anthropologist. Did she write about conjuring death spells alongside hoodoo men like Frizzly Rooster or participating in initiation rituals marked by nudity and blood as a record of an ethnographic truth *or* was she evoking the only image of the American south that outsiders wanted to believe? Was it the only story about her “skin folks” that would be allowed to survive in the academy? Surely these battles did not die with Zora, as I had to repeatedly reassure my IRB liaison that *curanderismo* does not involve blood rituals, satanic worship and the like; but if it did, how long would the native anthropologist last?

In the urban sphere, *curanderismo*, once thought to be practiced in rural places by spiritually gifted healers providing services for meager donations, has morphed into something more secular and commercial. Once principally the intimate and powerful world of *curanderas* (women healers), *curanderismo* is increasingly dominated by male healers as the practice shifts to more urban, public spheres. Additionally, *curanderismo* is not practiced or experienced homogeneously within Mexican American communities. Some recognize its merits in relieving physical, mental and spiritual pain; others do not. Still others are fully engaged in their own cultural stereotypes, seeing *curanderismo* as a shackle of premodernity, the irrational native caught up in a web of superstitions, fertile ground for profiteering charlatans. Despite its strong affiliations with Christianity, specifically Roman

Catholicism, some equate it with *brujería* (black magic), designed to commit *daños* (harms). How can this cacophony of experiences be captured without providing fodder for academics, outsiders or the natives themselves to dismantle this evolving healing system or negate its historical importance?

As Appadurai (1988:37) has astutely observed, the native’s voice is not “without distortion or residue.” Even the native tells lies about her/his own culture, sometimes packing things away too neatly. But anthropology, I argue, is done best when all the frayed edges of culture show. We cannot forget, however, that to show fissures can make populations vulnerable. Documenting how *curanderismo* is changing may seem to fortify ideas that it is “dying.” By cataloging beliefs, coping strategies, culture bound illnesses and herbal remedies am I weakening my culture’s defenses against those that may pathologize them? Would some Mexican Americans be pigeonholed as slaves to “*Si Dios quiere*,” (If God Wills) and ideas of the limited good? Who are these people so superstitious, so fatalistic, and so obsessed with ghosts? Surely, they are not my people. But, in part, they are my people. They are the partial truth about the Mexican American experience, the partial truth that too often gets amplified by anthropologists and others who do not want to dive into the messy details of heterogeneity.

As Narayan (1993) has argued, the identification of the native anthropologist shifts between insider and outsider, sometimes not of your own accord, but because the informant casts you in the role they find most fitting. When interviewing informants, I was constantly shifted between expert, rookie, interloper and cultural referee. Don Miguel, a former seminarian turned *curandero*, would question my research methods, wanting to know my criteria for identifying *curanderos*, demanding that I get a representative sample of his clientele, debating whether or not he had intellectual property rights over my tapes and photos. While I pushed for cultural intimacy and told him of all my family members that he had helped cure or advise, he held me to my role as academic. He constantly reminded me that his daughter was getting her PhD too, that he “knew the ropes” and would not let me escape with his knowledge before testing my academic rigor. Within his small consulting office filled with smells of burning incense and scented

oils, he deconstructed any myth of the ignorant *curandero* that may have ever played in my mind. He had potent cultural knowledge and would not impart it to just anyone, even a cultural insider.



photograph by Tanya L. Ceja-Zamarripa

Don Miguel standing in front of his altar

My academic status also played heavily with Doña Maria, a powerful *curandera* and entrepreneur, who wanted to give copies of my IRB approval letter to her clients as testament that someone from as far as Wisconsin had come to study her. The fact that I was a South Texas native was not highlighted. While she used the letter as evidence of her legitimacy, she was equally concerned that I show academic audiences proof of the healing powers of *curanderismo* and, in a rare event, allowed me to take a photo of the egg she used to perform a *limpia* on me. As she cracked the egg into a cup of water, I could see blood and straight pins surrounding the yolk. Here was the evidence of her ability to extract evil and I was allowed to take a picture to prove it within the academic arena of Wisconsin and beyond.

Working with healers, I found, was no small task. I would search *botánicas* (specialty shops), newspapers and flyers looking for anyone claiming to be a *curandero*. The moment I presented myself as an ethnographer I was cast in the role of interloper, cultural insider or not, by those who

were abusing the title of *curandero*, playing on cultural tropes, and exploiting people who wanted to believe that they were being served by a person who had a spiritual gift. These pseudo-*curanderos* could not stand the ethnographer's gaze. Additionally, as I explained my research to healers and *botánica* owners, I also became the siphon for gossip as each of them relayed their stories about perceived charlatans or *brujos* (witches) in the guise of healers who would try to cast spells to disrupt their business or harm them in other ways. They entrusted me with concerns about staying in line with market prices for *limpias* and card reading as they found out that a healer down the road was charging less. They conveyed their fears about the growing number of healers and *botánicas* specializing in *Santeria* as the city's population welcomed more Puerto Rican and Cuban residents. Would *curanderismo* weaken in light of this change? Would they lose customers to *santeros* skilled in other healing arts? Feeling an affinity to me as a cultural insider witnessing changes in our community, healers often asked me to don my academic hat and explain why these changes were occurring.

Curandero/as attract strong followings as they build a reputation for curing illness, accurately forecasting the future via tarot cards or being skilled in protecting clients from spiritual harm. Sitting in waiting rooms cramped with chairs and sometimes as many as 40 or 50 clients, I would present myself as a researcher and ask to conduct an interview. Most people declined until I mentioned having grown up just a few blocks away or until my mother, who is my research assistant, would share her miracle story and clients would feel a kinship and slowly reveal the emotional, economic and spiritual demons that plagued them. Their captivating stories rivaled the *telenovela* that played in the background as we spoke of lost loves, domestic violence, drug addiction, infertility, the lack of money...the lack of many things. They, in turn, solicited me for information on "authentic" *curanderos* in the area. In effect, I became their 411 directory for all things *curanderismo*.

What do I owe them for entrusting me with their personal secrets? In truth, there were frequent times I would wait three or four hours for a consultation with a supposed "great" or "powerful" healer, only to discover otherwise.

What would this mean to the people who are convinced of the reputation of these healers? Are their emotional and spiritual victories now meaningless because I, the academic, have “power” to claim in articles and books that their *curandero/a* may be a fraud? This is not to say that I have not seen cures and heard testimonies that defy explanation, because I have been witness to many. But how can I document my findings and not disavow their stories? This is a question I have yet to adequately answer, as either native or anthropologist. But there are times that I have felt like a thief, or perhaps a cultural traitor, displaying the pains and woes of my community for all to see, and to whose benefit?

There is no easy way to do anthropology. The role of “native” does not shield you from tense relationships with informants, the need to conduct valid and ethical research or the responsibility to protect the community that has entrusted you with cultural treasures. These are the distinctions of any good anthropologist, native or otherwise. But if anthropology is to commit itself completely to the idea of diversity, then room must be made to turn the discipline on its head and to be reminded that the rite of passage of studying the “other” was itself a power play, where the odds of you being correct were largely stacked in your favor as the automatic expert. Native anthropology revamps the parameters of what is defined as “expert” to include voices that have often been silenced in the literature, silenced as the anthropologist touched pen to paper and constructed them in the realm of academia.

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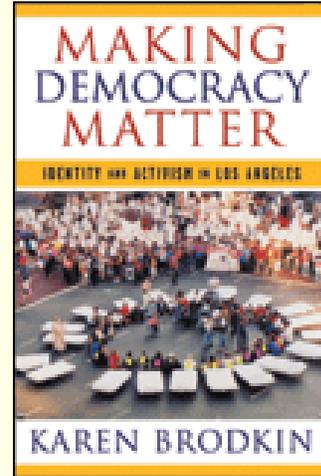
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HOT OFF THE PRESS!

BOOK GALLERY



MAKING DEMOCRACY MATTER Identity and Activism in Los Angeles KAREN BRODKIN

Book Description from Rutgers University Press:

What makes a social movement a *movement*? Where do the contagious energy, vision, and sense of infinite possibility come from? Students of progressive social movements know a good deal about what works and what doesn't and about the constituencies that are conducive to political activism, but what are the personal and emotional dynamics that turn ordinary people into activists? And, what are the visions and practices of democracy that foster such transformations?

This book seeks to answer these questions through conversations and interviews with a generation of activists who came of political age in Los Angeles during the 1990s. Politically schooled in the city's vibrant immigrant worker and youth-led campaigns against xenophobic and racist voter initiatives, these young activists created a new political cohort with its own signature of democratic practice and vision.

Combining analytical depth, engaging oral history, and rich description, this absorbing and accessible book will appeal to all those interested in social movements, racial justice, the political activism of women and men of color, and the labor movement today (2007)

From the Editor

Fieldwork on the Homefront:

Danger Zones and Insight at Critical Intersections

By Alisse Waterston

The articles in this issue of NAD were originally presented at a session titled “*So, you want to be an anthropologist?: The Politics, Pressures, and Pleasures of Fieldwork in the U.S.*,” held last November at the annual AAA meetings in San Jose. The articles here are based on four of the original 11 papers, and this editorial is based on remarks I made as discussant.

The subject matter of the panel and the work of each of these authors, point to the growing trend of “Anthropologists as North Americanists,” to use the title of a recent collection of articles that appeared in Anthropology News (January 2006). While preparing my original remarks last fall, the November issue of American Ethnologist (2006) arrived at my door which features 14 articles and commentaries on “the intellectual, practical, ethical, institutional, and political” issues (Dominguez 2006: 476) raised by IRBs particularly as more and more anthropologists are doing fieldwork on home turf. The AN articles, the AE articles, and the panel are among many other “happenings” that point to the growing incidence, significance and implications of research “at home.” The articles in this issue of NAD are another contribution to an ongoing dialogue about what it means to conduct anthropological research “at home,” including identifying key epistemological issues, key methodological tensions and contradictions, and key practical and political concerns.

It is important for us to remember that this dialogue did not just begin this year or the last ten years. There is a history, a tradition, a body of work, cumulative years of experience, all of which can easily be lost by a kind of amnesia in the discipline – an amnesia that has itself been brought on by the second-class status traditionally suffered by North Americanist research -- and yet all of this long tradition is very important to remember, to rigorously question and evaluate, and to recognize.

In her overview of developments and shifts in U.S.-fieldwork-based anthropology over the 20th century, Kate Masley (this issue) provides a framework for situating the research of young scholars and relatively novice fieldworkers whose research does have ancestry in anthropology. Masley’s overview offers an important acknowledgement of the intellectual and methodological history that oftentimes gets forgotten as the next cohort of North Americanists enters the field. As Masley points out, that intellectual history includes both the usual suspects – Lewis Henry Morgan, Franz Boas, the Chicago school -- but also anthropologists not as effortlessly brought to mind: Nora Zeale Hurston, James Spradley, Beatrice Medicine, Lee Baker, Ida Susser, Brett Williams – just to name a few cited by Masley.

I think it is safe to say that the climate for doing anthropological fieldwork in the U.S. is getting warmer despite the enduring legacy of “the discipline’s paradigm of leaving home” (Lederman 2006b:487) which privileges the funding and publishing of foreign over domestic research (Fennell 2006: 16). There are some encouraging signs: the establishment and growth of SANA; the growing number of funded North American projects by Wenner-Gren and NSF; and the growing visibility of established scholars whose fieldwork experience has been centered in the U.S., including Susan Greenbaum whose career was featured in an AN article titled, “Anthropology At Home Can Be Successful.” *more >>>*

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In the abstract for the AAA panel, organizers Kate Masley and Elizabeth Carpenter-Song write, “we are interested in moving beyond defensive stances toward U.S.-based fieldwork” to get at “new methodological and theoretical insights.” I applaud the effort to move from a defensive posture. At the same time, we need to tackle a range of issues so we can have greater clarity about what this anthropological enterprise is and what good it does, about what we are doing and why. It is no easy task to write honestly about motivations, intentions, processes, questions, hardships, struggles, anxieties, successes and insights while doing research “at home.” AN’s editor Stacy Lathrop reports “it was exceptionally difficult to find scholars willing to publicly comment on their reasoning in choosing to study the U.S., and the difficulties they encountered along the way” (2006: 16).

Perhaps talking about these issues, especially for graduate students and junior scholars, and in an honest way, is to enter a danger zone. Yet the most powerful methodological and theoretical insights seem to come from those places most vulnerable and exposed.

It is clear that the U.S. is a ripe fieldsite. We have and can still, as Catherine Fennell writes, “scrutinize the production of difference ‘at home’ and can ‘illuminate contemporary articulations of power circulating both within and well beyond North America.” We now live in one-world, not separate worlds, and the new “local” are those sites that articulate with structures of power and economy, but in different ways, producing new webs of meaning and ideas about how to fit into that one-world while retaining something about one’s past, experiences, traditions, activities. In the world as we now know it does exist, Goode says, it is simply “illogical to exclude the contributions of U.S.-focused work.”

So when it comes to field sites, Julie Brugger (this issue) describes her decision-making process. Brugger’s case is a wonderful example of how the research question itself took her to her fieldsite: “Right now,” she explains, “The U.S. may be in greater need of insight into what democracy means than South Africa” -- point well-taken. In the original version of her paper, Masley wrote about the very personal considerations that went into her decision to conduct research “at home” since her topic area was not bound by any particular

geographic place. She had other pulls -- private ones about family and home ties -- which we rarely disclose. To reveal this is to enter a danger zone, to go to a place that has been forbidden and off limits (and Masley eliminated this section from her public presentation and from her NAD article). That our choices are circumscribed can itself be theorized. There is an unspoken standard that to be considered a serious scholar, one’s intellectual interests and pursuits must be prioritized over other interests or demands. Of course, many women and some men don’t necessarily have the luxury to ignore private matters as Susan Greenbaum’s story in AN shows. The non-spoken standard is as gendered as it is rooted in the colonial model: the anthropologist as adventurer, unfettered, and free to pursue his quest for knowledge of the Other is an anachronism. We can attend to the personal, the political and the professional at once, not be torn into pieces, and at the same time, engage in “intellectually challenging and anthropologically relevant” research (Greenbaum 2006: 18).

Once we’ve settled on our research site “at home,” we start to see that the boundaries and distinctions between studying far from home, and close to home aren’t what we expected. The problematic of the “Other,” the processes of “estrangement,” and encounters with the “foreign” remain anthropological dilemmas.

Brugger’s article deals with this dilemma and more. In her research case, “the problem of the repugnant cultural other” and the “disciplining” we get from our anthropological colleagues for giving voice to the “loathsome,” bring up another danger. Yes, the story reveals that anthropologists can be just as hypocritical as anybody else. But, the real danger lies in if we get intimidated by the disciplining, and start to silence those “repugnant voices.” On the other hand, staying with them may lead us to greater clarity about how the broad structures of power and economy work to shape experience and worldview. In Contested Lives, Ginsberg did just that, showing the ways in which the abortion issue revealed the “economic vulnerability of pro-lifer” women in a political-economic world not of their making. Likewise, it is probable that the rural resource users who Brugger works with are struggling to make a living in a world that neither they nor the environmentalists have made. The forces (of private interest) that

create the need to carve out a protected land area are the very same forces that have created the economic conditions within which these folks must eke out a living augmenting their meager incomes by raising livestock (now restricted from protected land areas; no more public grazing) and dependent on gathering fuel wood (now prohibited) to stay warm through severe winters. If we can better see what underlies their lives, and the issues of concern to environmentalists, we can also better understand the ways in which tensions between these two so-called enemy groups reflect contradictions of capital. If this aspect is more fully examined, Brugger may come to the conclusion that there is less a problem for advocacy anthropology than she thinks, even though her point is well-taken that some anthropologists may have a knee-jerk reaction against the "cause" of "rural residents adversely affected by protected areas of the U.S."

Once we've settled on our research site "at home," other issues surface. Tanya Ceja-Zamarripa (this issue) beautifully captures the ambiguities of and ambivalences in the multiple social roles we all carry into the field, but which is especially present when we do research "at home" where, as Lederman notes, "the fieldwork-everyday life distinction is maximally blurred" (2006a: 479). Ceja-Zamarripa identifies for us the dangerous territory she is traversing: "'Do you really believe in those things?', people ask her about *curanderismo*. Ceja-Zamarripa hesitates, wondering that if she says "yes," will she be violating a taboo in the discipline - "it's okay for them (the natives, the Other) to believe such things, but not for you (the anthropologist)." But the anthropologist is the native, and in her study, it seems, many of the "natives" are asking her to ask anthropological questions. "Why are these changes occurring?" healers ask her. I understand that Ceja-Zamarripa is worried that her data will fortify distorted ideas about *curanderismo*, and that her findings may be used to further pathologize the people with whom she conducts her research and with whom she belongs. Perhaps she'd be better off focusing on the healers' question: "Why are these changes occurring?" If she can tell a story about what is happening, and very specifically why and how, and to whom and by whom -- what about all those "lacks" (lack of jobs, lack of housing) -- and how these social processes are reflected in this ever-changing cultural practice and belief system, she

won't be cataloguing, and therefore providing fodder for those who would use the information against her Mexican or Mexican-American informants.

In considering the nature of the native, Megan Nordquist (this issue) takes up the challenge to sort through that "maximally blurred fieldwork-everyday life distinction" to find the place where we can differentiate research from life. Doing research at home means that the possibility of encountering the field is always present. Without other, more concrete kinds of markers, Nordquist wonders what constitutes the moments of anthropological entries and exits that occur in the course of living one's life. She finds it useful to think of fieldwork as a state of mind which can then account for those moments when you put on and take off your "anthropological hat," turn on and off an "anthropological switch," or tune in your "anthropological antenna." "One has ethnographic intent," Nordquist explains, from which flows the anthropological actions of observing, experiencing and recording the field. Thinking in these terms might be useful for Ceja-Zamarripa who wonders if her "whole anthropological life can be characterized as one continuous participant-observation."

The articles in this issue demonstrate that the best of anthropology - whether on the home front or elsewhere -- comes from expanding the boundaries of investigation, from exploring border zones and places of ambiguity and uncertainty, a sometimes dangerous, but very human endeavor.

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***Of Hats and Switches:
Doing Fieldwork “at Home”***

By Megan Nordquest

Abstract: Call it what you may, the putting on and taking off of one’s anthropologist hat, the switching on and off of the anthropology switch, or the “tuning in” of one’s anthropology antenna, going “into the field” for anthropological investigation requires a necessary but often elusive shift in state of mind. This essay makes such a shift its object of investigation, suggesting how doing fieldwork “at home” might be uniquely situated to illuminate this anthropological mindset of “being in the field” as it offers daily entry and exit points. I discuss these “meta” entries and exits in relation to what Mary Louise Pratt (1986) has termed the “personal” and “scientific-objective” voices, and argue that though the balance of alternating identities may be inherent to all anthropological endeavors, it is especially profound and unique for U.S.-based researchers. Within the discussion, I reflect upon my own research experiences and accompanying shifts between “doing ethnography” and “regular life.”

Key words: Ethnographic intent, fieldwork “at home”, entry/exit stories, personal narrative, scientific-objectivism, U.S.

“Uh oh,” said my friend. “What?” I replied. “It’s just, you’ve got your anthropologist hat on, I can tell.” This exchange occurred one afternoon at the local mall. We were walking along, quite involved in the very non-academic activity of window-shopping, when I noticed a mother attempting to discipline her children, and I started observing the interaction. Without thinking, I had automatically connected the scene to the study on behavioral and emotional disorders in children that I was working on. My boss and I had recently been discussing the role of parenting styles in families facing such problems, and here was a prime example of parenting right before my eyes. In truth, I hadn’t realized a shift in my demeanor until my friend pointed it out to me. But my friend did. Before her very eyes, I had flipped some kind of switch.

At this point some readers may be thinking, that’s not an example of “doing fieldwork” or “conducting research,” and I will admit that it was perhaps not a key moment in my involvement on

the project. But for my purposes here, I draw on the situation intentionally to illustrate and evoke some general observations and questions about anthropological research. I ask you in advance to consider whether or not the answers to these questions would be different if I had been abroad versus in my home city. To begin with, does this bit of observation time count as research? Was I “in the field” for this small period of time, only to exit it quickly and return to shopping with my friend? It wasn’t a planned observation, it just happened. Does the material from watching this family count as data? To complicate this point further, I didn’t write down or record my observations, mainly because I wasn’t “working” at the time. However, I would say that the experience somehow added to my accumulating store of information related to this topic and probably indirectly informed some of my later contributions to the project as a whole. Further, if we were to decide my observations did count as data, if I had written them down, would they be IRB approved? Finally, there is the shift in my demeanor to consider, one that my friend actually observed. What are we to make of it? Though a stranger may have thought I was just zoned out, my friend recognized what was happening because she knew of the project I was working on. She saw that I was watching the interaction with specific intent.

Harry Wolcott has written that “what counts [in fieldwork] -- the only thing that really counts in the long run -- is what is going on in your mind, your sense of purpose among an infinite number of purposes that might be accomplished” (2001: 156). In this essay I want to consider this idea, that “Fieldwork is a state of mind” (Wolcott 2001:155), that it lies in a focused intent to study, observe, and engage in a scholarly way with a particular subject or group of people. Moreover, I want to question how we get into that state of mind. Call it what you may -- the putting on and taking off of one’s anthropologist hat, the turning on and off of the anthropological switch -- going into the field for anthropological investigation requires a necessary and often elusive shift in one’s mindset. Fieldwork “at home” is uniquely situated to illuminate this anthropological mindset of “being in the field” as it offers daily entry and exit points. I will discuss these “meta” entries and exits in relation to what Mary Louise Pratt (1986) calls the “personal” and “scientific-objective” voices in written

ethnography, arguing that though the balance of alternating identities may be inherent to all anthropological endeavors, it is especially profound and unique for U.S.-based researchers.

In her essay "Fieldwork in Common Places," Pratt addresses anthropologists' longstanding tendency to position, juxtapose, and describe the "science" that is ethnography in contrast to the similar, but more "casual" observations housed within other genres of writing such as travel books, memoirs, and journalism. In arguing that ethnography as a discourse actually borrows from these less scientific genres, Pratt focuses on the "vexed but important relationship between personal narrative and impersonal description in ethnographic writing" (Pratt 1986:28). She points out that though it is sometimes called "self-indulgent," the personal narrative in ethnographic writing continues to appear and be read with enthusiasm (Pratt 1986: 31).

It is conventional for the personal voice to appear in the author's fieldwork "entry" story. "It turns up almost invariably in introductions or first chapters," Pratt writes, "where opening narratives commonly recount the writer's arrival at the field site, for instance, the initial reception by the inhabitants, the slow, agonizing process of learning the language and overcoming rejection, the anguish and loss at leaving" (Pratt 1986:31). She then begins to hypothesize a relationship between the personal voice and scientific-objective voice within an ethnography, arguing that in many ways a statement of personal experience actually anchors and grants authority to the "scientific" descriptions with a book. With the inclusion of personal entries and exits, the stuff of personal narrative, ethnographers set up the "initial positionings of the subjects of the ethnographic text: ethnographer, the native, and the reader" (Pratt 1986:32). Thus, providing a proper and distinct entry and exit allows the reader to know who is who, and how the researcher is positioned in relation to the people described in the text that follows. Pratt also notes that as ethnographer-authors present the more formal or objective components of ethnographic description, they efface their personal voice.

While Pratt's analysis focuses on written product and the process of writing ethnography, I find it useful to borrow from her ideas in looking at the "doing of" ethnography; that is, the mindset

required of anthropologists as they enter the field. In this way, I'd like to think of how the personal narratives of entry and exit might be read as part of the methodology section of ethnographic monologues rather than sections to be debated for their scientific merit. They can be seen as descriptions of how the author got into the anthropological mindset necessary for doing all the exhaustive observation described in the final written product.

Though Pratt seems to posit that the process of shifting or converting from personal to scientific-objective voice takes place when one is writing the scientific/formal ethnography from one's more personal fieldnotes, I would argue that it actually starts earlier, when one decides that he or she is going to "study" a specific phenomenon, group of people, or issue. With this declaration and accompanying background research, one starts to observe, experience, and record differently. One has ethnographic intent. And with this intent in place, the process continues with continual ebb and flow between the personal and the scientific-objective, each voice, or mode, mutually informing the other.

Indeed, this process is complex and elusive, and inherent to it is the "dangerous" aspect of the researcher's subjectivity. For this reason, I think the shifting anthropological mindset is often overlooked or broad-brushed in stories of entry abroad, where inundation of "difference" and the elimination of many personal distractions affiliated with home can't help but attune one's mind. In this way personal experiences abroad can be recognized and provide legitimacy in the form of more classic entry and exit stories, but are, as Pratt describes, also marginalized to the book ends. However, accounting for such entry and exit while one is still at home does not allow for such an easy glossing over, but likely paints a more accurate picture of the ethnographer's constant passage between personal and scientific-objective modes of engagement. Thus, doing fieldwork in one's home brings to light the conscious and unconscious strategies one employs in shifting between "doing ethnography" and "regular life," and the parts of "regular life" which impinge on "scientific activity."

Because personal, physical entry and exit to and from the field is such an important, and from what Pratt observes, allowable place for the personal

voice, it is important to consider these issues for fieldwork done at home. At the start of this essay, I described what might be considered an example of how we unconsciously “enter” the field. We might also describe the routine daily entry patterns in which we consciously “flip the switch” or “put on the hat” when doing research at home. Ethnographers working near their home often enter or re-enter the field daily and are sometimes very familiar with the general location of their research site even prior to the start of their project. Thus daily entry and exit often occurs when one is traveling in and out of the field. It’s possible this travel may entail crossing a significant symbolic boundary of geographic familiarity, say if one is doing work in a different kind of neighborhood, but I would argue that the important crossing or shifting that occurs is located in the mind of the anthropologist.

For me, as I began doing exploratory research in my home city, entry into the field meant getting in my car, doing a quick mental review of what happened the day before, and a quick review of some themes I was observing. This was a conscious attempt to leave my personal life behind. The daily exit then started with the drive home characterized sometimes by my recording of the events of the day on a handheld tape recorder or by doing a mental review. Because I would write up fieldnotes when I got home, the exit wasn’t complete until the notes were complete. My car became a symbolic space, even though my field site was only minutes away from my apartment. I used my time in the car as a place to set my mind and shift between regular life and fieldwork.

Doing fieldwork “at home” also forces us to recognize that our personal lives do impinge upon our scientific-objective mindset. We know that much of the success of fieldwork has to do with the mindset and discipline one maintains because there are recognizable good and bad days in the field. Often times, the bad days happen when it seems impossible to leave one’s own personal narrative behind. On those days it might be you’re not getting much out of an interview or observation, or you don’t feel like you are really “into” your research, or your notebook seems full of useless information. Perhaps you don’t even recognize you’re not as sharp as usual until something big happens, something so big and important to what you’re studying that you are snapped back from the weight of personal distractions and are able to refocus and recommit to the role of the dedicated observer. I think it is fair to say that for fieldworkers “at home” there is a greater tendency for “regular life” to creep into the fieldwork experience. Self-reflection on one’s state of mind becomes an important activity, as the anthropology hat sometimes requires constant attention and self-check. One has to find a way to get oneself in the research mode.

To conclude, the shift between the personal and scientific-objective voice is not a distinct switch, but rather a continual flow. Locating this shift in the mind, as Pratt notes, “[brings] to the surface the anguished and messy tangle of contradictions and uncertainties surrounding the interrelations of personal experience, personal narrative, scientism, and professionalism” (1986:29), abundant in doing ethnographic research. Doing fieldwork “at home” is uniquely qualified to bring these tangles and uncertainties to light, providing important insights into anthropological methodology.

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