

ENGAGING CONTRADICTIONS

Theory, Politics, and Methods
of Activist Scholarship



Edited by

*For Michael Zinzun
in memoriam*

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Contents

<i>Acknowledgments</i>	<i>ix</i>
<i>A Note on Resources</i>	<i>xi</i>
<i>Foreword, by Craig Calhoun</i>	<i>xiii</i>
Introduction	1
<i>Charles R. Hale</i>	
PART I. MAPPING THE TERRAIN	
1. Forgotten Places and the Seeds of Grassroots Planning	31
<i>Ruth Wilson Gilmore</i>	
2. Research, Activism, and Knowledge Production	62
<i>Dani Wadada Nabudere</i>	
3. Breaking the Chains and Steering the Ship: How Activism Can Help Change Teaching and Scholarship	88
<i>George Lipsitz</i>	
PART II. TROUBLING THE TERMS	
4. Activist Groundings or Groundings for Activism? The Study of Racialization as a Site of Political Engagement	115
<i>Jemima Pierre</i>	
5. Globalizing Scholar Activism: Opportunities and Dilemmas through a Feminist Lens	136
<i>Jennifer Bickham Mendez</i>	
6. Activist Scholarship: Limits and Possibilities in Times of Black Genocide	164
<i>João H. Costa Vargas</i>	

7.	Making Violence Visible: An Activist Anthropological Approach to Women's Rights Investigation	183
	<i>Samuel Martínez</i>	
PART III. PUTTING ACTIVIST SCHOLARSHIP TO WORK		
8.	Forged in Dialogue: Toward a Critically Engaged Activist Research	213
	<i>Shannon Speed</i>	
9.	Community-Centered Research as Knowledge/Capacity Building in Immigrant and Refugee Communities	237
	<i>Shirley Suet-ling Tang</i>	
10.	Theorizing and Practicing Democratic Community Economics: Engaged Scholarship, Economic Justice, and the Academy	265
	<i>Jessica Gordon Nembhard</i>	
PART IV. MAKING OURSELVES AT HOME		
11.	Crouching Activists, Hidden Scholars: Reflections on Research and Development with Students and Communities in Asian American Studies	299
	<i>Peter Nien-chu Kiang</i>	
12.	Theoretical Research, Applied Research, and Action Research: The Deinstitutionalization of Activist Research	319
	<i>Davydd J. Greenwood</i>	
13.	FAQs: Frequently (Un)Asked Questions about Being a Scholar Activist	341
	<i>Laura Pulido</i>	
	<i>Afterword: Activist Scholars or Radical Subjects?</i>	
	<i>by Joy James and Edmund T. Gordon</i>	367
	<i>Contributors</i>	375
	<i>Index</i>	377

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7. Making Violence Visible

An Activist Anthropological Approach to Women's Rights Investigation

Samuel Martínez

Anthropologists live among the humans whose ways of life they study, but rarely do they treat these people as research collaborators rather than as research subjects. Why should this be so? Why should the people whose lives anthropologists study be left only reactive channels of influence (the answers they give to the scientists' questions) over how their own ways of life will be represented to the rest of the world? The "science hawks," who consider it a basic principle of scientific validity that research scientists alone should decide what questions are asked (Gross and Plattner 2002), are a minority within anthropology. Yet many more anthropologists, I suspect, simply do not think of questioning conventional limits to community members' research participation, even as these anthropologists strive to open up information and insights not attainable through interviews or casual interaction by participating as fully as they can in their host societies. The contradiction is clear: the anthropologist's participation in community life is valued even as the community's participation in ethnography is devalued. Considering also how much critical scrutiny has turned toward the production of knowledge in our discipline, it seems doubly odd that anthropologists so rarely ask why methodological authority remains concentrated in their hands. After all, cultural anthropologists should be particularly well situated, when compared to researchers in other disciplines, to activate the interested participation in the research process of the people we live among as we study their lives. Why, then, is "subject participation," participant observation's opposite and complement, not more often a salient approach to ethnography?

To my eyes, it is hard to explain anthropology's methodological exclusionism only if one views ethnography in the power vacuum of abstract

knowledge. A starting premise of my essay is, to the contrary, that power cannot be excluded from any full discussion of social research methods. I expand upon Merrill Singer's (1994) argument that ethnography is a "dominative" method of study when guided solely by the researcher's priorities and carried out through methods that make no sense to the people being studied. While anthropologists might not seem to be powerful people, it is a form of power to have the authority and ability to say what is important to study (and even more crucially, what is not important), dictate how it shall be studied, and decide to whom and in what forms the results of the research will be distributed. Self-limiting though it is to confine our power chiefly to the halls of academe, power even in small doses brings comfort. Conversely, risking your power, by sharing it with the people among whom you do your fieldwork, is apt to provoke anxiety, even if the risk brings opportunities to generate another kind of power: wider attention and respect for our research and writing outside academia. Power of another, institutional, kind inheres in the formulation and use of tallies of knowledge production, in deciding pay, privileges, promotion, and prestige. I will say more about those institutional constraints on activist scholarship below (see also Pulido, chapter 13 of this volume).

But I want first to hold onto the point that anthropologists can do much more than they generally have done to activate people's interested collaboration by involving them in designing the research to yield benefit to both parties. Charles Hale articulates a methodological justification for this kind of research in a recent issue of the Social Science Research Council's (SSRC's) *Items and Issues* (see also Speed, chapter 8 of this volume):

A sweeping claim to "better" results from activist research will no doubt prove difficult to substantiate. But it surely can be defended on at least two more particular grounds: a) people, who ultimately are the sources of social science "data," tend to provide much more, and much higher quality, information when they feel they have an active stake in the research process. Often, especially when the topic is charged or sensitive, they only provide information under these conditions; b) collective participation of these "subjects" in data collection and its interpretation inevitably enriches what we end up learning from the research. (Hale 2001, 15)

In what follows, I offer limited confirmation of Hale's points by relating some of my experiences in pursuing collaborative research on a highly "charged and sensitive" topic, the subordination and sexual exploitation of Haitian immigrant women in the Caribbean nation of the Dominican

Republic. From July through September of 2002, the middle months of a six-month field research trip to the Dominican Republic, I sought to forge a partnership with a leading advocacy and development group, the Movimiento de Mujeres Dominicano-Haitianas (Movement of Haitian-Dominican Women, MUDHA). This collaboration was but one part of a larger study, comparing the agenda and action strategies of Haitian-Dominican rights organizations, including MUDHA, with those of international human rights monitor groups that have worked on behalf of Haitian-Dominicans' rights. From the standpoint of my larger aims, the research was a qualified success in that I gained greatly from the time I spent with MUDHA staff. However, the collaborative dimension of the research was not in the end realized in the form that I had envisioned. My relationship with MUDHA continues. Since my field research, I have provided support for litigation they successfully pressed against the Dominican state in the Inter-American Court of Human Rights. I plan to do more field research on advocacy for Haitians' rights, and I look forward to collaborating more closely with MUDHA and other Haitian-Dominican nongovernmental organizations (NGOs) in future years. Yet I think it is not premature to attempt a sober assessment of our collaboration to date. My aim is not to question the desirability, validity, or even feasibility of activist scholarship but to evoke how complex Hale's proposed research agenda is and to ponder what obstacles may block its wider implementation in the anthropological discipline.

CONTRADICTIONS ALL AROUND

I was driven to craft a proposal for collaborative research with MUDHA partly by the personal motive of paying back part of my debt to the people among whom I had done my earlier fieldwork, Haitian immigrants working in the lowermost echelons of the Dominican Republic's sugar industry. Like many another anthropologist, I have on my conscience the asymmetry between what I and my research subjects in Haiti and the Dominican Republic have gained from the ethnographic relationship. In large part on the basis of my research among them, I have gained the highest academic degree, publications, and job security. They mostly remain in poverty, some even still living in the same ramshackle barracks in which I found them when I made my first of many fieldwork visits to the Dominican Republic in 1985. But in seeking this research partnership, I mainly sought practical help in getting at what lay underneath an odd silence, the inattention to the lives of the women of that community that

has characterized the reports of dozens of humanitarian, academic, and journalistic observers over three decades. The magnitude of the rights violations against women, involving human trafficking, sexual exploitation, sexual violence, and domestic violence, has gone almost totally unrecognized in what has been published about the situation of Haitians in the Dominican Republic. In earlier ethnographic research I had found that, as early as the 1940s up to the suppression of forced labor recruitment practices by the government of President Leonel Fernández in 1997, tens of thousands of women entered the Dominican Republic from Haiti, many of these women being relocated involuntarily to the sugar estates along with the men who cut the sugarcane. Though the women did not do much work in the sugarcane fields, they shared living quarters with the men and struggled for survival alongside them. During and after their detention and relocation, many of these women suffered sexual violence and exploitation. And, even now that the most flagrant abuses involving the forced relocation of Haitian entrants have been suppressed, these women's daughters of the second and third generation may continue to be preyed upon by men in power on the sugar estates. The evidence that the women's rights abuses affecting Haitians in the Dominican Republic were highly prevalent and serious led me to propose research collaboration with MUDHA, an advocacy organization with unsurpassed ties among Haitian immigrant women. Once I embarked upon this research, I would find that advocates for Haitians' rights in the Dominican Republic could provide detailed accounts of such abuses, amply confirming my suspicions about the seriousness of the wrongs. While I knew that finding women to provide their own, first-person testimony would be difficult, I was surprised that even organizing a research team to do these interviews was challenging.

Prior to this experience I had stuck pretty closely to the academic pole on the continuum from academic reflection to activist praxis. When Charles Hale first invited me to make a presentation to the L.A. workshop on activist scholarship, I harbored doubts about my qualifications to speak to the issue. I now think that my primarily academic pedigree is one good reason why my experience may hold lessons about the worth and challenge of doing activist anthropology. To the degree to which my experience highlights challenges likely to be faced by "wannabe" activist scholars, cautionary conclusions may be tentatively drawn from it regarding the likelihood of activist anthropology gaining converts among academicians like me. As someone whose practice had not previously involved either professional advocacy or collaborative research, was it enough for me

simply to feel dissatisfied with the lack of influence of my work outside academe and to have identified what seemed like an ideal case to break out of the box of ethnographic convention? My answer to this question can be instructive to others to the degree that it reflects challenges that other well-meaning, highly informed but advocacy-inexperienced anthropologists would be likely to confront in attempting to forge similar activist research collaborations. I am unsure how much can be generalized from the successes and failures of my fieldwork. Nevertheless, I feel certain that I raise a concern of wider applicability to activist anthropology: How easily can the research practice of primarily academic anthropologists be "retrofitted" to an activist mold? If the answer is "Not very easily," then what are the implications for activist anthropologists of the future?

Before taking up the case study, I will briefly compare activist anthropology with earlier broad programs of "real-world-relevant" anthropological research, giving particular attention to the hidden power imbalances behind conventional research models and how activist anthropology may redress these.

ACTIVIST ANTHROPOLOGY VERSUS APPLIED ANTHROPOLOGY

Dating back to the middle of the twentieth century but with increasing rapidity since the 1960s, a succession of proposals have been made to render the findings of anthropology more comprehensible, accessible, and useful to government, business, humanitarian outreach, and the philanthropic sector. Clearly the most influential—even hegemonic—has been "applied anthropology." From a mainstream anthropological standpoint, the question will inevitably be asked: What sets activist anthropology apart from applied anthropology and other prior approaches that claim to contribute to the solution of social problems?

Beyond the obvious features of activist scholarship (taking an explicit political stand, most often critical of harmful business practices or government policies, in alignment with the agenda of a group with which you have done research), I think that activist anthropology's distinctive characteristics are methodological and, even more importantly, institutional. For activist anthropologists, the methods and institutional contexts of research are as much political issues as the aims toward which the knowledge is applied.¹ Activist anthropology takes as its starting point an institutionally focused and globally contextualized critique of anthropol-

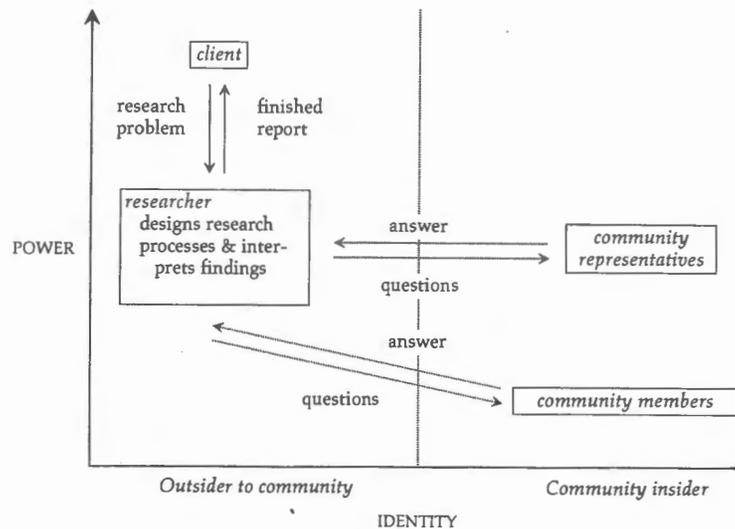


FIGURE 3. Structure of Knowledge Production in Applied Anthropology

ogy (building and amplifying upon the critical turn in the anthropological discipline of recent decades) and follows through on this critique of knowledge, not with a postmodernist shrug of futility, but with a novel and politically engaged program of study.² The latter program is defined not by a global theory or set of methods but by the resituating of the ethnographer as a political ally of the people among whom she is doing fieldwork rather than as a live-in inquisitor.

This contrast can be made clearer by briefly considering the institutional structure of applied research in anthropology. It is crucial to note that "applied anthropology" involves a vast and varied group of anthropologists, ranging from "practicing anthropologists," who hold nonacademic jobs, all the way to anthropologists with academic appointments who do "applied research" only in the sense that they study important problems—for example, in agricultural development or public health—but whose research and publication practices may be otherwise indistinguishable from the academic mainstream and whose findings may never end up being applied at all.³ While there is a continuum with shades of gray between the academic and practicing profiles, the far reaches of the continuum are clearly distinct, with contract research at one end and re-

search carried out under academic auspices at the other. For clarity's sake and with the caveat that I simplify greatly, I here consider the practice of the former—contract researchers and anthropologists who work full time for business corporations, government, and international finance and development institutions—as the distinctive institutional model for applied anthropology.

At this nonacademic pole of the applied anthropology continuum, a common institutional condition of contract and practicing anthropology is that both are client driven. Whether the client is his permanent employer or a contracting agency, in government, business, or an international financial, health, or development agency, the contract or practicing anthropologist does research to provide answers to the questions his client is asking. Dependence on clients imposes a strictly vertical structure of knowledge production and dissemination (see Figure 3). The contract/practicing anthropology model differs from the academic model less in the structure of knowledge acquisition than in the way in which knowledge is finally disseminated.⁴ Research subjects provide the raw material of information, which is passed through a value-adding process of refinement, processing, and packaging by the contract or practicing anthropologist. He, in turn, passes his findings, in the form of finished reports, upward to the firm or agency that is paying for the research, rather than "contributing" these, in the less obviously mercenary style and parlance of academia, to an amorphous "body of knowledge." The agency that commissioned the work decides whether, when, how much, and through what channels the information gathered by the applied anthropologist will ever be released to the public or shared with partner organizations.⁵ For the most part, the anthropologist conducts this contract research without forming partnerships with organizations based in the host communities, though that may be changing at least superficially as a result of the increased influence and prestige of NGOs in development, health, and humanitarian outreach.

None of this precludes the applied anthropologist's creation of horizontal links on his own. Practicing anthropologists doubtless get a lot of their best insights by talking with local practitioners and community representatives "on the side." Often, when the research setting is unfamiliar to them, the only way applied anthropologists can "get up to speed" on local affairs is to crib notes from the professionals who have on-the-ground experience. At times, sincere and lasting ties of friendship and collegiality are formed. Yet these horizontal ties remain optional and for the most part weak. Local advocates are not often given a say about what is to

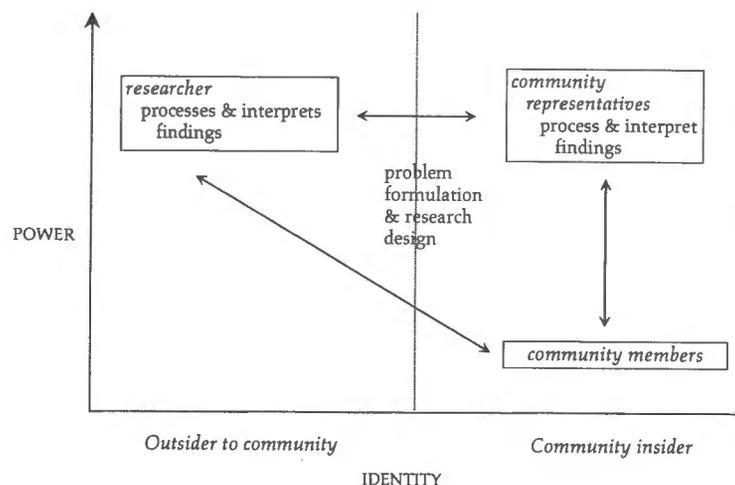


FIGURE 4. Structure of Knowledge Production in Activist Anthropology

be researched or how, a source of considerable frustration among the staffs of NGOs in the global South. These people often regard the visiting anthropologist, whether academic or applied, as a parasite that lands in their offices, sucks out valuable insights and information, and then takes flight, never to be heard from again (see also Pulido, chapter 13 of this volume on the importance of "accountability" and "reciprocity" within activist scholarship).

The activist model of knowledge production departs from the applied anthropology model first by lopping off the top of this vertical structure. Without a sponsoring client to set the research agenda, the activist researcher enjoys greater freedom to engage local practitioners and advocates, marginalized in the applied model, in a reiterative dialogue about the goals and methods of the research (see Figure 4). Realistically, it must be recognized that our erstwhile research subjects in low-income communities remain below us on the research power gradient. It is perhaps also unavoidable that, even when communicating through the medium of "base community organizations," community members do not always speak with a single voice and may be more reticent than hoped for, tending too readily to defer to their own leaders and to researchers and practitioners of higher socioeconomic status. Even in the activist research model, therefore, responsibility for knowledge formulation and dissemi-

nation may thus often rest unequally in the hands of the primary research partners. Yet one already noted goal of activist research is to bring members of the population targeted for action into the research-design dialogue. For what the activist anthropologist seeks at the outset is not answers to predefined questions but clues about what the research questions should be, vis-à-vis priorities for action that can be identified only through dialogue with community members and community organizers. Rather than a vertical chain, which assigns the actors at each level of power highly distinct and discrete functions and unequal power to set the research agenda, the activist research process ideally generates a dialogic triangle. In this triangle, even the most humble participant can provide information, insights, or objections that set in motion an agenda-modifying discussion among the research team.

Activist anthropology is also predicated on full information sharing between the researcher and her partner organization(s). Rather than a contract, the social relations of knowledge production of activist anthropology might be appropriately compared to a *convite*, the festive, collective agricultural work days still commonly practiced by peasants in the Dominican Republic and their neighbors in Haiti (who call this custom *konbit*). The *Almanaque folklórico dominicano* describes the *convite* as follows:

When a peasant is going to roof his house, plant his crops, or collect his harvest, etc., and does not have the means to pay, he invites his neighbors and friends to carry out the task in question between them all without receiving any monetary remuneration for this.

The organizer of the *convite* or *junta* is responsible for [providing] food and drink for the participants. Beautiful work songs are sung there, and at mealtime instruments are also played. (Domínguez, Castillo, and Tejada 1978, 118, my translation)

Like the *convite*, activist anthropology puts people to work alongside each other, each side maintaining a distinct project, the anthropologist hoping to harvest academic publications even as he helps activists cultivate political or organizational gains. As in peasant agriculture, the goal of activist anthropology is not generating maximum output but generating sustainable and equitably shared gain. And, as in the *convite*, reciprocity guides the activist anthropological partnership—at every stage of the research cycle, from problem formulation to dissemination of findings—rather than giving either contracting agencies or the researchers control over what questions matter and how to study them.

THE IMMIGRANTS AND THEIR ADVOCATES

Among the many imaginable venues for activist anthropology, few engage the special skills of anthropologists better than collaborating with not-for-profit organizations dedicated to formulating effective responses to social, health, and economic development problems. Anthropologists' knowledge and ways of learning are highly adaptable to these organizations, their work settings, and their goals. Our linguistic fluency, knowledge of local communities, and flexible field research methods prepare us well to collaborate with local partners in producing information that is reliable and relevant to household and community-level problems. We are used to spending large amounts of unstructured time in our research, listening to and learning from people of all social strata and walks of life. Our experience writing grants and reports is, by contrast, underutilized while we are in "the field," but this resource could also be put to good use in working with an NGO. These were the kinds of possible roles I envisioned developing in my Dominican Republic research in 2002. My immediate aim was to learn about the social organization and guiding concepts of two Haitian-Dominican NGOs by accompanying their staff in their everyday activities. It was understood by all that a further aim was to be the formulation of a viable participatory project for gathering information about the underexposed human rights abuses experienced by Haitian immigrant women.

It is significant that this way of doing anthropological research only crystallized in my mind in response to the challenge of competing for a grant that explicitly required such horizontal academic-practitioner collaborations. The seed of an activist research approach may have lain in my mind for years, but to grow into a proposal it needed the fertile ground of a reward comprehensible to an academician: grant money for fieldwork and research leave. The SSRC's Global Security and Cooperation (GSC) program, foreseeing the need to build bridges between scholars and practitioners in the broad field of international security, ran a two-year experiment with funding from the John D. and Catherine T. McArthur Foundation. They invited proposals by scholars to learn by participating in the activities of governmental, multilateral, or nongovernmental organizations. The idea was that, by contributing to ongoing projects, academicians could learn from the inside how such organizations work. It was anticipated that through these situated learning experiences trust would be built, communication across the academic-practitioner divide would flow more clearly, and new ideas for collaborative research would emerge through discussion. As I wrote the proposal, I felt that my

research aims fell easily and naturally into the program's situated learning frame. Perhaps latent in my mind for some time, the idea needed only an appropriate institutional stimulus to hatch.

With support from the GSC program, I carried out six months of field research in the Dominican Republic. I focused on the human rights agenda and modes of activism of two small NGOs, founded and staffed by second-generation immigrants from neighboring Haiti (Haitian-Dominicans), MUDHA and the Centro Cultural Domínico-Haitiano (Haitian-Dominican Cultural Center, CCDH). The CCDH was the first Haitian-Dominican rights organization, founded in July 1982 in the southeastern sugar port city of San Pedro de Macorís. Its main goals are (1) to promote recognition, in law and in fact, of the Dominican nationality and citizenship of all Haitian-Dominicans and (2) to affirm and valorize the Haitian origin of Haitian-Dominicans. MUDHA spun off from CCDH in 1983 but was not officially incorporated as an organization until 1992. Its coordinator, Solange Pierre, has always been a dominant presence in the organization and has emerged in recent years as the leading voice in international forums for her community's rights. MUDHA's main mission is to defend and promote the rights of female Haitian immigrants and to attain rights of full citizenship for their Dominican-born children. MUDHA's mission statement defines these rights in the widest terms: "civil, political, economic, social, cultural, and human." Both groups place primary emphasis on grassroots organization and capacity building among their constituent community groups. To gain further understanding of the organizations' goals, a few words of background are necessary about the history and political economy of immigration from Haiti to the Dominican Republic.

On the Dominican side of the border, the main sources of demand for Haitian immigrant labor fall into two broad categories. The first is the sugar industry, which since the early decades of the twentieth century has recruited men from rural Haiti as cane cutters (*braceros*). The second major source of demand has been industries other than sugar, which have pulled in Haitian immigrants both from the sugar estates and directly from Haiti. Historically, the two main nonsugar employers have been coffee and construction, but many Haitians, particularly women, have also found work in domestic service and petty commerce (Silié, Segura, and Dore Cabral 2002). As late as the 1970s, a combination of police vigilance, social isolation, and widespread discrimination restricted the employment of Haitian labor mainly to a few trades. Now Haitians are employed not just in the sugar and coffee industries but in agricultural sectors where

they were previously rarely found (Lozano 1993). Also, it seems that a variety of informal income-generating opportunities, including petty commerce and domestic service, are attracting Haitians to the city in growing numbers (Facultad Latinoamericana de Ciencias Sociales [FLACSO] 2004, 32). Evidence of the continued expansion of Haitian labor into new urban employment niches is observable in many parts of the Dominican capital, Santo Domingo. Also, sugar is no longer as central as it once was to the Dominican economy, and this industry's demand for cheap harvest labor has declined significantly.

The plight of the Haitian *braceros* became a topic of international scrutiny in the late 1970s and has gone in and out of the human rights spotlight repeatedly in the quarter-century since. Monitors from at least seven multilateral and NGOs, along with independent journalists and human rights advocates, have repeatedly brought the situation of the immigrants to international attention, often alleging that the constraints placed on the *braceros'* freedoms were so severe as to constitute *de facto* slavery. These reports have presented firsthand testimony of men, and even boys as young as seven, being recruited in Haiti by sugar-company touts with false promises of easy, well-paid employment in the Dominican Republic. They have reported undocumented Haitian entrants being captured by Dominican police and military agents, then forcibly relocated to state-owned sugar estates. And they have evoked images of these recruits being forced at gunpoint to cut sugarcane, for below-minimum wages, under dangerous and unsanitary conditions.⁶

The accuracy and completeness of these reports are questionable at a number of levels. Costly gaffes resulted from the errors in the approaches and analyses of the international NGOs. In 1991, the clumsy use of pressure tactics without adequate coordination with local Haitian rights advocates was undoubtedly the major proximate trigger for an official backlash in the form of the largest-ever deportation of undocumented immigrants to Haiti (Martínez 1995, 164–67). Until the mid-1990s, it could safely be said that international pressure had achieved little other than pushing the Santo Domingo government to enact cosmetic reforms in migrant labor recruitment and employment practices.

Perhaps most problematic among the omissions and simplifications that have characterized the international human rights reportage on this case has been the all-too-frequent silence of journalists, monitors, and academicians about the situation of both second-generation Haitians and female Haitian immigrants generally, attention having focused almost exclusively on the slaverylike conditions of recruitment and employment of

male sugarcane workers from Haiti. What led highly experienced international monitors, with relatively free access to the affected communities, to overlook whole categories of victims and types of abuses? Why did observers focus so narrowly, for more than two decades, on the civil and political rights abuses that constituted the alleged enslavement of male sugarcane workers, to the exclusion of other abuses and other segments of the Haitian immigrant population?

Brighter prospects for effective international solidarity have emerged more recently. A major turning point came in 1996 with the election of opposition candidate Leonel Fernández to the Dominican presidency. Under Fernández, reforms were instituted in the procedures by which Haitian immigrants were recruited as cane workers. Most importantly, his government ended the practice of forcibly relocating undocumented Haitians to the state-owned sugar estates. On the negative side, the Fernández government instituted routine deportations of tens of thousands of Haitian nationals each year, a practice continued by subsequent administrations.

In the late 1990s, the struggle for Haitians' rights assumed a major new dimension with the presentation of claims before the Inter-American Court of Human Rights (IACHR) by the Berkeley and Columbia University human rights law clinics and the Washington-based Center for Justice and International Law (CEJIL), in collaboration with MUDHA. The plaintiffs in these cases have sought remedies from the Dominican state on behalf of Haitian nationals who were repatriated without due process and for Haitian-Dominicans who were deported in spite of carrying valid legal identity documents identifying them as Dominican citizens. Partial victories have been won in the IACHR, and the Dominican government has been forced to facilitate the readmission of certain plaintiffs into the country (Pierre 2001). In March 2005 an important new case was brought before the Inter-American Court by MUDHA, with help from the Berkeley and CEJIL legal team. This case was filed on behalf of two Dominican-born girls of Haitian parentage who had been denied Dominican citizenship in spite of their mothers' having presented all the legal documents generally considered necessary for granting citizenship by Dominican civil registries. Like these girls, tens of thousands of other Haitian-Dominicans born in recent decades have been rendered *de facto* stateless persons and consequently denied basic rights of education, identity, and internal and international mobility. Important as this litigation has been, it is of comparable significance, in my opinion, that now at last international advocates are working as partners with Haitian-Dominican advo-

cates who are active on the ground. The days when priorities and strategies of advocacy were defined entirely by international NGOs, with minimal consultation with local activists, are fortunately now past.

Recent reports sponsored by the National Coalition for Haitian Rights (Gavigan 1996), the Inter-American Commission for Human Rights (IACHR 1999), and Human Rights Watch (2002) have laudably broadened the scope of the human rights reporting to at last include the second generation and immigrants living outside the sugar estates. Yet past and present sexual violence and exploitation still remain largely unexamined. To my knowledge, I am the only outside observer to have recorded firsthand testimony concerning the forced relocation of Haitian women to the sugar estates (Martínez 1995, 122–23), even though Haitian-Dominican activists will readily speak in detail about this practice and a gamut of other human rights abuses against women.⁷ While the very presence of Haitian immigrant women in the Dominican Republic is no longer overlooked, their stories of adversity and survival remain largely unrecorded.

MY RESEARCH

All this helps explain why the main collaborative research activity I put forward in my GSC proposal was a project to collect the life histories of Haitian immigrant women and their daughters born in the Dominican Republic. MUDHA's director, Solange Pierre, on repeated occasions expressed enthusiasm for this project. In an interview that I did with her in 1999 she eloquently characterized the immigrant women's stories as an as-yet-unwritten chapter in the history of relations between Haiti and the Dominican Republic. Rather than participating directly in the interviews, I envisioned organizing a team of female interviewers for this purpose, in the expectation that the women interviewees would sooner speak to women than to men of sexual abuse and exploitation and other wrongs of an intimate and psychologically traumatic nature.

Seeking to fill remaining gaps and uncertainties in my knowledge, I spent the first two months of my field research gathering background information from a variety of sources. Staff at both organizations also permitted me to accompany them to community workshops and meetings held on site in *bateyes* (company compounds for agricultural workers) in present and former sugar-producing zones, where the population of Haitian nationality and ancestry is most heavily concentrated. These site vis-

its opened up insights into the relationship between NGO staff and base community groups, as well as shedding light on *batey* communities' priorities for activism. More generally, my background research strongly confirmed my main hypothesis that the Haitian-Dominican NGOs pursue a much broader agenda of activism than the international NGOs have done. Both CCDH and MUDHA view cultural revitalization, citizenship rights, women's empowerment, and community social and economic development as inextricably linked goals that must be pursued simultaneously rather than giving priority either to civil-political or social, economic, and cultural rights.

I also found it useful to spend as much time as I could in the offices of CCDH and MUDHA even if I had no meetings to attend, documents to read, or any other particular reason to be there. My most frequent and in-depth contacts were with MUDHA staff, both because and in spite of an acute financial crisis in that organization after its funding was discontinued by two foreign governments' aid agencies. Recognizing that there was an opportunity to make myself useful in this crisis, even if it meant departing somewhat from my proposed research agenda, I volunteered to help in any way I could. I researched possible sources of funding and passed this information along to MUDHA, and I participated in exchanges of information via e-mail among overseas "friends of MUDHA" concerning avenues of outside solidarity at this difficult juncture.

While MUDHA's financial crisis presented an unexpected opportunity for me to render assistance, it also created limits. Their ordinary grassroots organizational work went on but at a diminished pace, due to sharply reduced staffing. The search for potential funding sources and preparation of applications put extraordinary demands on the time and energy of the remaining staff. The sheer lack of time and the almost palpable stress and fatigue in the MUDHA office made it at times difficult for me even to find opportunities to meet with particular staff, let alone find ways of involving myself usefully. The circumstances of our collaboration were complicated further by political concerns. MUDHA was the target of harsh criticism in the press and Pierre even received anonymous death threats following their presentation of a document at the 2001 Durban World Conference on Racism, identifying anti-Black racism as a basic element behind the hostile reception accorded Haitian immigrants in the Dominican Republic.⁸ MUDHA staffers normally interpret intimidation tactics positively, as a sign that their message is reaching its target. Yet the suspension of their outside funding so soon after the "Durban

controversy" led some in MUDHA to wonder openly if they were being quietly punished for being too outspoken in international forums.

I opened discussions aimed at recruiting a research team during the second week of July, two full months into my stay. I needed this time to get adequate confirmation from MUDHA and other knowledgeable practitioners and scholars that there were in fact no existing sources of testimony from immigrant women that might take the place of these interviews and thus obviate asking the women to recount psychologically traumatic events. CCDH director Antonio Pol Emil and others there agreed that MUDHA was better prepared than they were to act as partners in research on sexual violence and on women's rights generally. Meetings and conversations with MUDHA staff over the subsequent three weeks confirmed the need for creating such a corpus of testimony.

These discussions also broadened the scope of the project considerably beyond what I had initially proposed. The MUDHA staffers were emphatic that many more interviews and a broader geographical sample than I had envisioned would be needed for the interviews to be useful to them. They pointed out, correctly, that Haitian women in the more openly racist and anti-Haitian northern half of the Dominican Republic would not have the same experiences as their sisters in the south. I resisted the pressure to expand the study, citing the inadequacy of my funding and the large amount of time it would take to analyze each in-depth interview. Feeling that it would be too time consuming to try to obtain a representative sample of interviews, I wished simply to obtain corroboration and more in-depth testimony concerning the abuses I had uncovered in the mid-1980s and had later heard described in greater detail by Haitian-Dominican rights advocates. In the end, we agreed on a compromise that at least four interviews would be done in each of three regions: the southwest (near the Haitian border), the north, and the south.

Reaching agreement that the interviews would have mainly educational and advocacy aims, rather than evidentiary value, was easier, it being understood that few Haitian immigrant women would talk if they thought they or their abusers might later be identifiable. Besides, I lacked the legal training to judge what kinds of statements could be admitted as evidence by an international court. It was foreseen that the interviews, beyond possibly being published as an edited volume of testimony and contributing to my mainly scholarly publications, might yield material that MUDHA could use in community-level workshops with Haitian-Dominican women on the topics of gender relations, reproductive health, and self-esteem. They would gain knowledge as I gained knowledge, and a

major gap in the human rights record concerning Haitians in the Dominican Republic would be addressed for the first time. We got as far as discussing the logistics of identifying interviewers among the Haitian-Dominican women who had worked with MUDHA as community organizers and planning dates and procedures for bringing them to Santo Domingo for training and other interview preliminaries.

It is important to note that MUDHA's lead coordinator, Pierre, was out of the country for the greater part of May, June, and July. I was frequently reminded by the MUDHA staffers with whom I had been negotiating in Pierre's absence that MUDHA worked on a democratic basis of consensus building. Yet it did not immediately sink in that, without Pierre, a needed piece of the consensus was missing. I was therefore surprised that, during an evening meeting late in July with Pierre and the others, all the issues that I thought had been resolved were again set forth for discussion as problems. Geography, sampling size, and the fact that anonymous interviews could not be used as legal evidence ended up being the main focus of a discussion that I had hoped would bring us close to final agreement about the interview procedures and personnel.

In the end, agreement could not be reached with MUDHA, and I opted to assemble an all-female interview team without the hoped-for participation of experienced advocates. The interviews did not yield the anticipated testimony about abuses suffered at the hands of soldiers, police, and sugar company bosses, for reasons that I can only guess at. While the kind of first-person testimony I wanted was not produced, I did learn a great deal about these abuses from secondhand sources, through interviews with Haitian women's rights advocates. These interviews focused specifically on the human rights situation of Haitian women in the Dominican Republic and covered a wide range of topics, including forced relocation to sugar camps, sexual exploitation, and the denial of rights of citizenship to their Dominican-born children. These interviews lack the immediacy and impact of the kind of first-person accounts that I had hoped to bring back. Yet they do constitute a significant resource when seen against the dearth of information and analysis concerning Haitian women's lives in the Dominican Republic.

IMPLICATIONS FOR WOMEN'S RIGHTS INVESTIGATION

A few years after publication of Amnesty International's (1991) report *Women on the Frontline*, storms of controversy erupted concerning the

length of time it took for international observers to report the phenomenon of mass rape in the Bosnia and Rwanda conflicts (Human Rights Watch 1996; Stiglmeier 1994). Since that time, students of human rights have been more than ever concerned with why sexual violence in wartime and other flagrant mass infringements of women's rights so often escape expert detection and public notice. In dozens of articles, feminist critical legal theorists have challenged the gender neutrality of international human rights instruments and bodies of national law.⁹ Far fewer works have given comparable attention to gender bias in the monitoring and enforcement of women's rights. Lacunae regarding violence against women have been much more often remarked upon in passing from a human rights perspective or dealt with as if these were purely matters of investigative techniques rather than interrogated and explained in depth as products either of patriarchy or of global race/class inequality.¹⁰ Important gains have been made at the level of the international community's consciousness of sexual violence as a human rights challenge. And, in a landmark ruling in 2001, the International Criminal Tribunal for the Former Yugoslavia convicted three former Bosnian Serb Army commanders for rape, torture, and enslavement, defining these wrongs as crimes against humanity (Human Rights Watch 2001). But what are we to make of reports since 2001 that international human rights investigators were again slow to denounce mass rape in wartime, in settings as diverse as the conflicts in Sierra Leone, Congo, Sudan, Iraq, and Colombia (Human Rights Watch 2006; Amnesty International 2004)? Ten years on from the conflicts in Rwanda and the former Yugoslavia, the walls of silence around gender violence sadly seem to be resisting demolition by well-intentioned and hardworking human rights experts.

Though my own research did not take place during an armed conflict, the powerlessness of the survivors *vis-à-vis* the perpetrators, many of whom have been men in power, and the institutional barriers that have isolated the sugar company compounds from outside legal scrutiny and intervention are no doubt powerful disincentives for the survivors of sexual violence to reveal their truths to any human rights investigator. Considering that my interviews with NGO staffers yielded ample independent confirmation that Haitian women in the Dominican Republic were still experiencing a broad gamut of rights violations, I can only suspect that some of the immigrant women who were interviewed by my research team had experienced or seen *bona fide* human rights abuses but felt hesitant to divulge such sensitive information to interviewers with whom they had no prior acquaintance and rapport. One irony is that the

Haitian and Haitian-Dominican women whom I interviewed during my dissertation fieldwork were more forthcoming with accounts of human rights abuses than the women interviewed by female researchers fifteen years later. Trust and linguistic fluency, such as I built up over months of community fieldwork in 1985–86 (the lead interviewer on the research team did not know Haitian Creole), carry an undetermined weight. I think it is fair to point out also that women played lead roles in nearly all the human rights fact-finding missions that failed to uncover either the trafficking of Haitian women or their rape and sexual exploitation while on route to and in residence on the Dominican sugar estates. Though my interviews with members of these fact-finding teams are only at a beginning stage, what I have gathered thus far suggests, not that these monitors tried to gather information on women's rights abuses and failed, but that it simply was not brought to their awareness that the women were experiencing infringements of their rights any different from the men's. These observations suggest that the gender of the interviewer, though doubtless important, may matter less than I had assumed. It is not that the balance of sexes in fact-finding investigation teams does not matter (Gardam and Charlesworth 2000) but that still further changes in human rights investigation may be needed to bring testimony of sexual violence to light in a timely manner. Consultation with local human rights advocates might yield less in the chaos of wartime, but, in the case of Haitian immigrant women's rights, the silence of human rights reportage on gross and systematic infringements seems attributable in large part to international monitors' failure to consult earlier and more widely with organizers among the Haitian-Dominican community itself.

I sense a trend toward more partnerships being sought by academic anthropologists with local and international development and human rights organizations. I anticipate that this path will be followed especially often by our graduate students, more of whom than ever before, it seems, are coming back to academia to pursue advanced degrees in cultural anthropology after working for some time in the not-for-profit sector. For many, a commitment to one or more progressive political causes will be a primordial motivation for going into anthropology. For them, working alongside practitioners seeking positive change will simply expand their earlier political engagement into a scholarly program. Others—especially midcareer scholars like me, who are coming back to the field after completing their dissertations—will be motivated more by feelings of obligation to do some service to the people who have given them so much.

While my pursuit of this research partnership had a solid scholarly justification, I see now more clearly that my interest in applying for the GSC grant grew also from the realization that I was and would continue to be an extractive researcher until I surrendered some of the power to define the agenda of my research and its methodology to the people with whom I work.

Activist anthropology is no panacea for the practical and ethical dilemmas that stem from anthropology's identity as an intellectual project of the global North. The prevailing institutional structure of social science inquiry poses such great obstacles to implementing activist research projects that the growth of activist anthropology will continue to be impeded if incentives for globally equitable social scientific research are not developed, such as those offered by the GSC from 2001 to 2003. No research panel, from the university department level on up, could seriously consider funding a proposal that basically asks for money to go find out what the people in some far-off place would like an anthropologist to study. For the research to gain needed funding, it must be justified first and foremost on theoretical and methodological grounds deemed valid by the anthropological community. Once researchers receive funding, they must go out and get results that address the hypotheses set forth in their proposal, taking up time and energy that could be devoted to cultivating relationships with local advocates. Doctoral candidates and junior faculty in many departments of anthropology might also feel discouraged from admitting even the slightest influence on their research agenda from nonscientists, out of fear that this would diminish the importance of their findings in the eyes of their professors or colleagues. Funding, approval of the doctoral dissertation, publications, and ultimately tenure depend on our research being perceived as having scientific rigor. There are no corresponding institutional incentives for anthropologists to produce findings that are useful to the people who aid us in doing our fieldwork.

It is less fully appreciated that the world of advocacy and community activism also has its institutional structures of knowledge and professionalism, into which the free-floating academician may not always easily fit. Access, as my experience shows, presents a first set of potential pitfalls. Given the politically sensitive nature of the topic that I was studying, it is understandable that the collaborative dimension of the research proved more challenging than I expected. With organizations, such as MUDHA, caught in highly politicized struggles for rights or resources, I think it is to be anticipated that even highly informed and experienced researchers who have no previous work history with the host organization(s) may

encounter difficulty establishing an internal role and a relationship of trust. In some instances, the activist researcher will assiduously attempt for weeks or months to cultivate a working relationship with an organization, only to meet with rejection from the would-be partner. If setbacks of this kind occur frequently as more anthropologists seek research collaborations with activist organizations, greater uncertainty of meeting predefined research goals may emerge as one important limitation to the wider adoption of an activist research approach. Senior scholars who are secure in their jobs and reputations may shrug off the potential loss of time and effort more easily than our graduate students and junior colleagues can.

Once admittance is gained, my experience shows that it is not always easy for affiliated scholars to insert themselves into the host organization's set routines, or for underfunded and understaffed organizations to make time for scholars' needs. Also, community organizers who are used to collaborating with people in other organizations may find it hard to understand right away how to work with a lone academician. Unease may flow from ambiguity about the independent scholar's position in a field of power. Where external collaborations are generally either "vertical"—with international sponsors—or "horizontal"—in the form of networks or partnerships with like organizations—where is the anthropologist coming from, vertically or horizontally? We may propose an equalitarian exchange, but that does not necessarily diminish the tendency of our would-be institutional partners in the global South to see us as powerful outsiders.

The perceived power imbalance, as Joanne Rappaport (2002) points out, may be more real than most anthropologists would like to admit. For Rappaport, while North-South intellectual *exchange* is always possible, it is a dangerous illusion to think that horizontal *collaboration* may be achieved. Even when ethnographers think we are proposing an open and horizontal collaboration with indigenous or citizen intellectuals of the global South, our own research generally ends up taking priority, in effect converting our "collaborators" into "research assistants." Hierarchy will thus reassert itself in collaborations between northern academicians and southern research partners, submerging their concerns and approaches once more under our theories and methods. Rappaport's own preferred model of activist research, developed over years of work with indigenous historians of the Colombian Andes, is not melding the two parties' research aims but working in parallel with citizen intellectuals of the global South.¹¹ I harbor some skepticism about how often we may find the op-

portunity to work alongside the people with whom we do our fieldwork as intellectual peers, in the way she has. Her model has the limitation of assuming that our partners have the inclination and time to pursue their own independent research projects as we do ours. Certainly, at the time of my fieldwork my MUDHA partners sorely lacked the time to do extensive investigation. Yet I think activist researchers would generally do well to take into consideration her larger point that it may not often be possible to join our research goals seamlessly with those of the groups with whom we work as ethnographers. The effort to do so can even undermine our larger politically progressive aims if the agendas of northern researchers insidiously subsume the aims of southern activists.

The general significance of these observations is not that activist research is not doable or worth doing. I sooner think the following conclusions flow from my experiences in retooling myself as an activist scholar:

- Activist research, to a greater degree than other research models, depends upon the establishment of a relationship of trust between the researcher and the activists.
- His relationship of trust often cannot develop quickly but only over months or years.
- Trust will probably grow more surely if the visitor volunteers to work for a time purely as an activist rather than a researcher.
- The relationship of trust is all the more essential and time consuming to establish if the study takes on a highly politically sensitive topic or brings a researcher with a politically dominant national, racial/ethnic, or gender identity into collaboration with members of a subordinated community.

In the context of activist scholarship, the "situatedness" that Donna Haraway (1988) is responsible for popularizing in the academic lexicon is as much or more a matter of where you are coming from than a matter of where you elect to stand. Even as I approve of Jennie Smith's (2001) cogent prescription for "un-doing ourselves," as a first step toward reshaping anthropology into an instrument of social justice, I wonder how straight the path is toward this aim. We anthropologists always bring a lifetime of experiences with us into the field and confront an uncontrollable set of associations in the eyes of our erstwhile research subjects and would-be political allies, relating to nationality, race, gender, age, institutional affiliations, and more. It would be too pessimistic to say, "You are what you are," for you can always adjust and adapt your research practice

to new circumstances. Yet you cannot remake yourself into someone you are not, much less control how others will perceive you.

One crucial corollary is that more anthropologists must take up activist scholarship earlier in their careers. New generations of graduate students who enter anthropology with established practitioner allegiances and qualifications will mostly find the path to developing activist scholarship projects easier to negotiate than those who lack solid prior activist contacts and credentials. Academia will likely never reshape itself into an activist mold; to be so transformed, it must instead be colonized by activists from without. A major obstacle to realizing this goal of broadening the anthropological profession's profile to include more activist scholars lies in our professional gatekeeping criteria. The professional profile of the people who are most likely to succeed at activist research differs from what graduate school admissions committees and grant-giving foundations normally include among their criteria of merit. Clearly, we in the academy must give greater value than we currently do to professional advocacy experience, as well as to personal affiliations with subordinated minority groups, if more students and junior faculty are to be moved into activist scholarship early in their careers. All of our students should be directed where appropriate to seek out community activists at the earliest possible stage and to negotiate the incorporation of their concerns into graduate research. To the degree that non-North American or European, nonwhite, and nonheterosexual male researchers, depending on the social and cultural context, are more likely to find acceptance among would-be activist research partners, the intellectual justification for all measures to expand diversity among anthropologists is strengthened.

NOTES

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1. It must immediately be added that the participatory approach to which I give emphasis is neither unique to activist anthropology nor alien to applied anthropology. Any number of applied anthropologists have solicited community participation and attempted to tap into public opinion or knowledge as they define

their research goals. Methodologically, then, activist anthropology might easily be understood to overlap with another active and varied area of applied research, reaching well beyond anthropology: "action research" or "participatory action research" (Greenwood and Levin 1998).

2. Considering how deeply anthropology's academic critics are rooted in what they are criticizing, it should perhaps come as no surprise that they are generally more timid than anthropology's nonacademic critics. Charles Hale (in the introduction to this volume) focuses laser-sharp criticism on anthropological postmodernists' pretensions to do "politically engaged scholarship." Rather than focus on the macro-social power imbalances that have given the discipline its characteristic form and content, anthropology's academic critics usually have focused on the micropolitics of fieldwork or have critically analyzed the radically "Othering" effects of anthropology's dominant scientific realist writing conventions (Speed, chapter 8 of this volume). As Michel-Rolph Trouillot (1991, 18-19) observes, "current calls for reflexivity" within American anthropology are neither "products of chance" nor "a passing fad": "Rather, they are timid, spontaneous—and in that sense genuinely American—responses to major changes in the relations between anthropology and the wider world, provincial expressions of wider concerns, allusions to opportunities yet to be seized." For Trouillot, the critique of anthropology remains political only at the level of the discipline's "electoral politics," relating to who gains what rewards from academic institutions and professional associations. Few academic critics have followed Trouillot in suggesting that our writing conventions are not the root of the problem so much as the narrowness of the constituencies we address, the ineffectual, scholastic aims of our publications, and the exclusion from the circuit of knowledge production of the people with whom we do our fieldwork. Worse, anthropology's postmodernist critics fall into the trap of bootless cynicism by defining the discipline's narrowness and exclusivity not as shortcomings that can be redressed in practice but as unavoidable features of the ethnographic enterprise (Clifford and Marcus 1986).

3. The line between applied and practicing anthropology is important enough to have been recognized institutionally, within the American Anthropological Association, through the establishment of a practicing anthropology interest section, distinct from the larger and considerably older Society for Applied Anthropology.

4. We academic anthropologists, too, receive the "raw ore" of data handed to us by thousands of impoverished producers from the global South, take this home with us to the North, and there refine it and package it into finished scholarly products for sale in northern intellectual markets. At no point need we in academia stop to consider what information will "sell" in the largely low-income communities where we do field research. The people among whom we do our research are not the ones who hand out the rewards.

5. Not coincidentally, contract and practicing anthropologists tend strongly to take "clinical" rather than "critical" methodological/theoretical approaches. They frame their research questions around the needs of client organizations that seek to intervene constructively on behalf of individuals enmeshed in "social problems." The larger circumstances in which these problems are rooted matter less, to contract/practicing research, than do recommending better ways for providers of social services to provide outreach to individuals and communities "in trouble." The activist anthropologist generally tends, by contrast, to highlight perceived ultimate causes.

6. These reports include Americas Watch (1989, 1990, 1992); Human Rights Watch (2002); Inter-American Commission on Human Rights (1999); Lawyers Committee (1991); Gavigan (1996).

7. In an interview that I recorded in 1999 with Pierre, she recalled the following kind of scene played out each year in her childhood on a sugar plantation in the Dominican Republic's Cibao Valley when truckloads of new migrants would arrive from Haiti:

For example, in the *batey* where I was born and raised, a head *batey* on the old Ingenio Catarey, . . . in the corral . . . they would unload seven or eight trucks—the famous "Catareys," old flatbed Mercedes Benzes, that was the mode of transport!—there, four, six, even eight trucks would unload an enormous quantity [of people]. And then, after the boss and the authorities of the place picked their women, then . . . the *viejos* would come by to pick among the ones who were left there to live with them. Many of those women, besides serving the guy, [he] automatically became their pimp.

8. An English translation of selections from this document was published in the *GSC Quarterly*, no. 5 (Summer 2002), www.ssrc.org/programs/gsc/gsc_quarterly/Archive/.

9. Cook and Oosterveld (1995) is a recent bibliography; basic works include Charlesworth, Chinkin, and Wright (1991), Cook (1995), and Gardam and Charlesworth (2000), as well as essays in Cook (1994) and Peters and Wolper (1995).

10. MacKinnon (1993) and Stephen (1995) are important exceptions.

11. Rappaport's research partners have gathered legally admissible evidence via oral history and have studied the landscape for information regarding past land use and land tenure at the same time as she has studied the forms in which indigenous knowledge of the past is formulated and preserved. Their approaches have been braided together over the years through innumerable meetings at which each has brought forward information and ideas of interest to the other. Yet their aims, methods, and audiences remain distinct. Rappaport uses the same kind of metaphor for this approach that I introduced earlier as an alternative to the contract model of applied anthropology when she refers to her exchanges with local historians as "intellectual *mink'as*," the *mink'a* being an indigenous Andean form of festive labor exchange similar to the Dominican *convite*.

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