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Toward a Native Anthropology: Hermeneutics, Hunting Stories, and Theorizing from Within

Darren J. Ranco

As a Native person doing anthropological research within and for American Indian nations, I am not able to sidestep the many theoretical and ethical concerns that non-Native researchers face in doing similar research. I have had to defend the potential biases of my research, whether it is applied or action oriented, whereas my non-Native colleagues do not. Indeed, if anthropology is, as Thomas Biolsi and Larry Zimmerman state, "a quintessentially Western project" that "Westerners ask about themselves and their encounter with peoples they have colonized and liquidated,"¹ what use could I possibly find in the techniques offered by such a set of questions? In graduate school, I realized that my interests in anthropology differed from those of my peers who seemed intent on traveling the world and experiencing new things. I sought to capture anthropological "skills," understand my place in the world, and help my community communicate our struggles for survival. In what follows, I want to examine why and how Native and non-Native researchers choose the research questions they do, and how this relates to the colonial context in which they find themselves. To do this, I treat research itself, not just anthropology, as part of the historical and colonial context of contemporary indigenous people.²

A number of studies provide ethical guidelines for doing research for and within American Indian nations, and many of them provide good ideas for anyone contemplating a research agenda within Indian Country.³ What is lacking, I believe, is a deep theoretical rumination

on what indigenous researchers bring to these debates, and how they may shift the theories that underpin this research in the first place. As a Native anthropologist, I want to explore the nature of research, how it does and does not meet Native research interests, and propose some suggestions as to where we can go from here.

Thus, this essay is a confession—or you could call it an ethnography, although only a very partial one, of indigenous engagements with research. My struggles to understand matters that are close to home and to help my community regain control over our culture and resources brought me to anthropology, and this essay documents some of the problems I and others have encountered in trying to research and advocate for what is close to us. I see anthropology as a hunting story—a story about capturing something of the Other that the West desires and bringing it back for Western consumption. But what if the thing that we, as indigenous researchers, want to bring back is not Otherness but a set of tools to protect and enhance Native cultural and natural resources? Is this also a hunting story? Can anthropology, which privileges the Western outsider as a producer of knowledge, be useful to us?

**ENCOUNTERS WITH THE OTHER:
HERMENEUTICS AND
THE PRIVILEGED OUTSIDER**

Most non-Indians I meet who do research in Indian Country are not anthropologists—a discipline, for many good reasons, that has lost favor in Indian communities.⁴ Many of them are teaching or getting PhDs in departments of sociology, religion, education, history, folklore, and English literature. When talking to them as a Native person, I usually find they started their research to understand and experience something “Other” than themselves and are thus reinscribing the same anthropological desire for the Other, although without the negative label that anthropology has in some Native communities. I contend that this search for the Other is at its base a colonial desire with which we, as Native and non-Native researchers, must contend.

From where does this analytical desire to examine the Other come? The form of anthropological knowledge of which I speak is the same as that proposed by hermeneutics, or the study of meaning. Traditional hermeneutic theory, much like classical anthropology, postulates a subject (the analyst) who aims to understand an object (a text, a social practice, the Indian himself) as it is in itself.⁵ This means that the subject must be as open-minded and unprejudiced as possible, approaching the object without preconceptions. By introducing “their texts” (those of the “Other” under study) as well as “ours” (those of the analyst), recent theories have given up on the idea of the “unbiased”

observer while maintaining the privilege of the observer. For Clifford Geertz, Hans-Georg Gadamer, Paul Ricoeur, and others, in contrast to traditional hermeneutics, preconceptions or prejudices are what make understanding possible in the first place.⁶ They are bound with the awareness that history influences the effectiveness of the text; without this awareness, those doing the analysis would not understand it. For example, it is impossible to understand the Bible or the *Communist Manifesto* without knowledge of the role they play in history.

Anthropological hermeneutics maintains this belief that ignorance provides privilege. An anthropologist will eventually understand what Others are talking about, or so goes the positivist version of anthropology, because the anthropologist's understanding of the world will be changed by the exposure to their life world. The basis of anthropological analysis then becomes a comparison of his original subjective stories with theirs. In an attempt to nuance how we should understand "*what they say*," Vincent Crapanzano stresses that we should also be sensitive to *how* our informants say things. For Crapanzano, the life story is a combination of artistic form and informative report. For him, "the life history is a product of its author's desire for recognition by this essentially complex other. It is not simply informative; it is evocative as well. Its evaluation requires an understanding of the relationship between the author and his other, the inevitable interlocutor whom he is addressing."⁷ The information-evocation dichotomy comes from the fact that

in everyday life we collapse the conceptualization and the phenomenology of experience. . . . It is important to recognize that the two, the conceptualization and the phenomenology of experience, must be analytically separated if an epistemologically valid science of man is to be achieved.⁸

With this in mind, Crapanzano makes explicit the dilemma of anthropological understanding. On the one hand, the ethnographer must participate in the tradition of those she studies, while on the other hand, she must distance herself to interpret this reality and be a researcher.⁹ Crapanzano is able to maintain distance from (or, to see it another way, is unable to completely understand) his informant Tuhami's world because of the colonial encounter and the convenient placement of his interpreter, Lhacen. Crapanzano refuses to say definitively what we should do with this problem of distance, but he wants to allow some sense of mystery for the other, while at the same time arguing for intersubjectivity.¹⁰ For Crapanzano, we have to keep in mind our own intersubjective limitations, that is, our own subject position in relation to our informant's, if we ever want develop a science of man, or even something that could possibly resemble a critical hermeneutics.

The belief that understanding comes from a change of self by overcoming subjective difference has also been a key tenet in recent hermeneutic philosophy. Hans-Georg Gadamer, like Crapanzano, sees the interpreter as torn between "his belongingness to a tradition and his distance from the objects which are the theme of his investigation."¹¹ For Gadamer, in hermeneutic understanding, one moves back and forth in a dialectic between one's world and the object, constituting meanings, altering one's horizons, until the object and the world are unified into a coherent whole. One thus "understands" the object and one's own convictions within the same experiential event.

Despite its potential "hypothetical and fragmentary results,"¹² including the subjective stories of others in our analyses still maintains the privilege of the observer. Paul Ricoeur writes:

The intersection between the theory of texts and the theory of action becomes more obvious when the point of view of the onlooker is added to that of the agent, because the onlooker will not only consider action in terms of its motive, but also in terms of its consequences, perhaps of its unintended consequences. A different way of making sense with actions occurs then, and also a different way of reading it as a quasi-text. Detached from its agent, a course of action acquires an autonomy similar to the semantic autonomy of a text. It leaves its mark on the course of events and eventually it becomes sedimented into social institutions. Human action has become archive and document. Thus it acquires potential meaning beyond its relevance to its initial situation.¹³

Thus, hermeneutic understanding shares a lot with anthropology and a lot of other research on Indians. Distance from that which we study is not seen as a hindrance but as an important aspect of the process of obtaining knowledge.

The belief that we can only understand ourselves with respect to others is a fair enough psychologism. A corollary belief, that we can understand the Other better than she can understand herself because of our distance from her world, maintains the idea that knowledge is only brought to certain places by certain Western observers. Without the mediation of a professional anthropologist, the knowledge of Others would not be understandable to "us" and perhaps would not exist in any real form at all. As I will touch on below, a problem for many anthropologists has not been that they understand social life only through their own changing subjective stories; the problem has been that anthropologists believe that the Other must be something that is radically Other.¹⁴ I make this point because certain criticisms of anthropology's

affinity with the exotic have focused only on the *form* of the anthropological enterprise.¹⁵ I believe that it is much more than the textual form of ethnography that gives it an affinity for the exotic. The methodology of self-change and the notion of the privileged outsider also influence anthropology's fetish for the exotic. Anthropologists, in conjunction with hermeneutic theory, privilege the stories that have the potential for changing their subjective worlds. As a Native anthropologist, I have to ask, is it possible to understand the kind of story that does not have the potential to change us? What about those stories that are so Other that anthropologists cannot understand them at all? Are they even concerned with such stories?

**ENCOUNTERS WITH ANTHROPOLOGY:
THE LIMITS OF HUNTING STORIES**

We have been observed, noted, taped, and videoed. Our behaviors have been recorded in every possible way to Western Science, and I suppose we could learn to live with this if we had not become imprisoned in the anthropologists' words. The language that anthropologists use to explain us traps us in linguistic cages because we must explain our ways through alien hypothetical constructs and theoretical frameworks.

—*Cecil King (Odawa)*

As an undergraduate at Dartmouth College, I wrote my senior honors thesis in anthropology about "my own tribe." As a member/citizen of the Penobscot Indian Nation, I occasionally had to defend myself as "not biased" as an anthropologist studying people and places with which I had a personal connection. Initially, this was not a problem for me. However, I encountered difficulty finding an appropriate authorial voice for an experience that was both personal and "social science." The classical ethnographies I read (like E. E. Evans-Pritchard's *The Nuer* or Frank Speck's *Penobscot Man*) were all similar in tone—purportedly distanced, objective, all-knowing. I knew that I could not write about people I had known all my life in the same manner, but I experienced difficulty writing something that seemed anthropological without treating my friends and family in disrespectful ways. I kept thinking, how did the fieldwork situation change me? What did I learn? If I answered these questions, I knew I would be well on the road to writing an ethnography, but I remained stifled by both the question regarding how I had changed and the form of writing up my experience with friends and relatives as objective data. Like Odawa elder and educator

Cecil King in the quote above,¹⁶ I was also becoming increasingly aware that I did not want to create linguistic cages for the experiences of my friends and relatives back home—the stakes were too high for my community for me to make a mistake. I knew that anthropological experts sometimes used their “knowledge” against Native communities that asserted their rights—anything from land claims, fishing rights, and repatriation of human remains require anthropological intervention to be successful.

The voice I used in my undergraduate thesis was never to my liking—I tried to be a good ethnographer, and I felt like I failed because I was unable to break down the stories of my people into an objective analysis. A couple of years later, as I thought about my doctoral dissertation topic, I was faced with a related dilemma. I came to graduate school as someone who would ostensibly be doing “Native anthropology,” but some of the faculty in my department warned me that there might be problems doing work with “my own people.” Several professors in the department urged me to find a new ethnographic location, because I would “benefit from the comparative study.” Despite the self-critical turn in anthropology, where studying close to home has come into analytic favor,¹⁷ most social and cultural anthropology programs still strongly encourage dissertation fieldwork in remote locations, where one learns the anthropological tools to bring back to locations closer to the home society. Given this model of anthropological inquiry, and the advice from my instructors, it became clear to me that what I proposed was not exotic enough. I must experience something truly different than myself if I was to ever learn what it is like to be a *real* anthropologist. It became increasingly clear that anthropology *still* considered the exotic the most appropriate and interesting story to tell.

Despite these problems, I had become savvier in graduate school—I was now able to use anthropology as a tool to advocate an indigenous perspective. For one, I was becoming increasingly aware of works that challenged the classical tradition that seemed so stifling to me during my undergraduate research. Also, I had talked to my family and friends after my earlier ethnographic effort, and I realized that they thought my study of Penobscot identity was fairly accurate but ultimately uninteresting to them. The question I asked myself, therefore, was, what would be seen as an interesting or important work from the perspective of other Penobscots, and how could I make this a good anthropological story? What topic would truly help my community?

While I was writing my undergraduate thesis, my community was becoming much more politically involved in pollution issues in, and along, the Penobscot River, our aboriginal homeland and the site of our retained reservation lands and resources. As an undergraduate, I could never seem to fit this issue, which framed a lot of my interpersonal experiences while I was home doing fieldwork, with my an-

thropological analysis. As a graduate student, I knew that to study this extremely important political and cultural situation, I would have to employ a multi-cited examination of policy and politics. I was not going to put Indians on center stage—I would withhold the ethnographic gaze for the United States, a nation-state that continues to fail a Native population at risk after hundreds of years of colonial imposition. I knew that using anthropology to advocate *for* an indigenous perspective was totally different from using anthropology to think *about* the indigenous, which is what I saw my peers and professors in graduate school doing. Despite growing interest in local-global ethnography, I wondered if I could capture, in anthropological terms, the story of our attempts to protect our resources?

Other anthropologists have addressed the limits of anthropological stories. To engage them helped and continues to help my project. So, what happens when anthropologists encounter people whose stories they cannot make into good anthropology? Whereas I was faced with a fieldwork situation that was potentially too close to understand as anthropology, many anthropologists have been concerned with stories that are too Other, too difficult or far away for them to understand. Sometimes, no matter how hard anthropologists try, they cannot change enough in accordance with hermeneutic theory to understand the stories of their exotic informants. In her 1993 ethnography, Mary Steedly explores this type of situation. For Steedly, Karo women's narratives are

fragmentary and inconclusive, starting from an authoritative position (the "official version") and then shifting ground, swooping into uncertainties and narrative dead ends only to move the story elsewhere, or—sometimes—repeating a phrase or image. . . . Women's stories ended only to begin again, in a different key, with a different stress, so that narrative closure was always postponed, meaning always deferred.¹⁸

That women's stories "end only to begin again" reflects the fact that, for Steedly, Karo women's stories "were patient with the interruptions of everyday life . . . and attuned to the cadences of the perpetual open end—the 'to be continued' and the 'more to come.'"¹⁹ Karo women's stories, as conceived by Steedly, were built on the rapport and intimacy they encountered in everyday life. These stories were open-ended because they related experiences between people who already had very close, constant, although intermittent, contact. Because of this context, women told stories to each other that were open-ended and fragmented—stories that could always be changed and "subjectified" again.

Edwin Ardener has written precisely about this "problem with

women."²⁰ The reason women do not appear in our ethnographies, he explains, is because they have been muted. He states that one of the reasons women are not given equal voice in our interpretations of other societies is "they were rendered 'inarticulate' by the male structure; that the dominant structure was articulated in terms of a male world-position."²¹ Ardener is making a statement about the voice women may or may not have about the dominant ideological forms in a society. Are we concerned with the stories that do not fit with the generalized models they have of their society? Are such stories outside of the dominant culture's comprehension? Can we afford not to listen?

Steadly tries to understand the stories of these women, she tries to make sense of them on their own terms, but the narrative structure Karo women use on a day-to-day basis is one that does not fit into the most explicit, dominant models of Karo society *or* anthropology. These people are no more or no less sophisticated than the Greek men who have developed a superorganic tool for recognizing meaning in all sorts of situations,²² but Steedly has trouble soliciting and understanding their stories:

Generic standards of narrative authenticity (what counts as a story) and style (how a story should be told) are organized by reference to the social experience of Karo men—as patronimically identified subjects occupying stable positions in a relationally constituted and flexibly hierarchical social field, and so social actors *do* count for something in public discourse. This is, on both points, precisely the sort of social experience that is structurally unavailable to women, who are thus doubly discounted: first by the literal muting of their voices, which leaves them unpracticed in public debate and unheard in public speech; and second by the discursive limits of narrative plausibility, which require women's stories to be cast in the borrowed phrasings of men's interests and men's experience, if they are to gain an audience.²³

Thus, according to Steedly, the everyday stories of women are devalued by the Karo-Batak. The typical interests of anthropology also devalue them. What counts as good stories in both contexts are hunting stories. Karo women's stories do not speak to issues of politics and power, economics and ideology—issues at the core of anthropological knowledge. Steedly urges us to develop a language to deal with such stories. If we are to take Roger Keesing seriously, this should be part of the interpretive enterprise.²⁴ But as Steedly points out, ethnographers might not be ready for these stories. If "good" stories and dominant ideologies in Karo-Batak culture are about the hunt, so too are good

ethnographies. Who would want to read an ethnography that was "fragmentary and inconclusive?" This leads me back to my question. As a Native anthropologist, I was also rejecting the hunting story within anthropology. I was not going to a faraway place to retrieve data and take it back to the home of anthropology. I was trying to make anthropology work for my community. And the interactions with people in my community, and the stories we told each other, were probably a little like the Karo women's stories that Steedly encountered—they were ongoing . . . they would end only to begin again . . . the next time we saw each other . . . the next time . . . and the next time.

Thus, would ethnography by a Native anthropologist that treated "home" and "away" differently be incomprehensible? Stated differently, was being an indigenous anthropologist also a challenge to this preference for hunting stories? Most certainly it was, but I knew that engaging the discipline would be a way of asserting control over the politics of culture in which anthropology is so embedded.

TELLING OUR STORY: TOWARD AN INDIGENOUS ANTHROPOLOGY

On the other side of this dilemma in which anthropology only accepts certain kinds of stories is the story of the intimate—something too close to change us, something outside of anthropology's conventional gaze. I, like many other American Indian anthropologists before me, encounter this problem in my work. The late Bea Medicine was a leading voice in calling attention to the unique problems that Native people face in anthropology.²⁵ As she points out, Native people have done anthropology for some time, but our work has been swept under the rug as mere data gathering to be used by the "real," trained, unbiased professional. To her, the predominant attitude for most of anthropology's history has been that "native and female anthropologists are seen as potential 'tools' to be used to provide information to the 'real' white male anthropologist."²⁶ This attitude continues in respect to our work, which is generally seen as "advocacy" in anthropological circles, thereby making it immediately less serious or theoretical than that of our non-Native counterparts.

Currently, many of the difficulties faced by Indian anthropologists come from our communities of interest, those we are ostensibly trying to help or with whom we are trying to think through problems. Medicine locates this disavowal of anthropology to Vine Deloria's 1969 essay that was sharply critical of anthropologists.²⁷ She seems intent on saving anthropology for Indians, arguing that "Native readers seemingly do not go beyond page 100 of Deloria's manifesto entitled *Custer Died for Your Sins*."²⁸ She emphasizes that Deloria later states, "this book has been hardest on those people in whom I place the greatest amount of hope

for the future—Congress, the anthropologists, and the churches.”²⁹ Thus, we are supposed to have hope for anthropology inasmuch as anthropology, like Congress and the churches, has the power to allow us to control our lives for the better.

Medicine sees the role of Native anthropologists in anthropological research as repairing some of the key “problems” of anthropology identified by Deloria and others. Medicine points out that Native anthropologists can address and help educate other anthropologists to be more respectful of the communities in which we do our research. We, as Native anthropologists, can call attention to the real problems in revealing Native knowledge to outsiders. We can emphasize the need for more reciprocity and continued relationships after the fieldwork situation ends. We can make nonanthropological, community-oriented funding more of a standard practice. We can make anthropology more reliable by working with our communities in drafting our written work. We can use less jargon in our writing and make education of Native youth a priority in our fieldwork. Perhaps most importantly, we can go into our work with an open mind and let the community define our research. As Medicine states, “Native Americans often believe that most anthropologists already have a theoretical framework when they enter an indigenous social system and collect and report data in support of this prior formulation.”³⁰ Better research would be more collaborative. As anthropologists, we stand as critical examples not only to be better in our research ethics but also to consider the needs of our communities in order to provide better theoretical tools for the non-Native anthropologists in our midst.

Ultimately, Medicine believes that Native anthropologists must maintain their interest in applied research. There are important cultural reasons for this. She points out that some Native Americans, following Ed Dozier, went into anthropology to help their people: “this suggests strong interest in the application of anthropological knowledge and is tied to the Native idea of education, no matter in what field, as a means of alleviating problems and providing self-help among Native groups.”³¹ Medicine is fully aware, however, that this emphasis on applied work has been a detriment in making the work done by Native anthropologists more central within the discipline. Part of the reason applied anthropology remains peripheral within the discipline comes from the failure of the applied programs themselves: “the residue of previous applied programs has not been as efficacious as we thought.”³² More critical, however, is the fact that certain voices, perspectives, and people continue to remain dominant within the field. She attests to the fact that “the contributions of ‘people of color’ . . . have not been a strong feature in a sociology of knowledge in approaching our discipline, especially in the applied field.”³³

Thus, Medicine asks for more theoretical work in the applied field

itself, where indigenous anthropologists would take center stage. She argues that there "is little knowledge of what an applied anthropologist does. People seem to expect advocacy from any anthropologist."³⁴ She urges us to investigate the role of the Bureau of American Ethnology, Indian movements, gaming, and, in general, the structures of power in each of these institutions.³⁵ Conducting these types of studies with an emphasis on power, I believe, is the best way I know for Native anthropologists to work on issues relevant to our communities, to reverse stereotypes and curb exploitive forms of anthropological desire, and to make key intellectual contributions to a sociology of knowledge within anthropology. For example, I intend my work on the relationship between power and knowledge in Native American environmental policy to be of service to my Native community (and other Native communities) by communicating our needs to policymakers and scholars who have the ability to influence and change the ways that environmental policy is conceived and implemented. One of my projects exposes the ways in which the Environmental Protection Agency fails to fully assess environmental risk to Native Americans; traditional, land-based lifestyles expose Native peoples to high levels of toxic substances in the environment.

I think that this kind of project follows directly from what Medicine defines as the four primary areas of concern for both Native and non-Native anthropologists. First, we need to empower people. This can be done by "teaching and researching issues of race, class, gender, and power relations in ways that can be understood and utilized by 'target populations.'"³⁶ Second, we need to do more participatory research and not use Native people as consultants but as codirectors of research projects. "Thus, they can learn research techniques and initiate and implement their own 'needs assessments' and application strategies to improve the quality of life in their own communities."³⁷ Third, we must show our finished products to those we research, creating a dialogue wherever possible. Finally, we need to do new kinds of research that show the impacts of research itself on Native communities, especially in anthropology, health, and environmental matters.³⁸

BEYOND ETHICS: REVERSING THE HUNTING STORY

As Medicine writes, "people seem to expect advocacy from any anthropologist."³⁹ Thus, the position of the Native anthropologist provides a critical space from which to think about the possibility of research ethics in colonial and other contexts where power differentials exist between researcher (and what their knowledge represents) and those they research. We need to answer Medicine's call to theorize our advocacy, and this may pave the way for our particular sets of expertise

to take center stage within anthropology itself. In Maria Elena Garcia's 2000 article in *Anthropological Quarterly*,⁴⁰ she addresses precisely this dilemma—all anthropologists have to be advocates on some level, so how does this impact our research and the communities in which we research? Does this make our anthropology less legitimate than in previous times, where advocacy was discouraged or not disclosed? Our understanding and interest in the power dynamics involved in anthropological knowledge is where we will begin to move these debates beyond research ethics, start to reverse the hunting story embedded in anthropology, and return to Native communities the political and discursive control over their stories.

For Garcia, the issue of ethnographic responsibility should be a focus for both Native and non-Native anthropologists. She states that the "implications of incorporating the assumptions and the expectations of informants and their communities into ethnographic analysis . . . are not particular to native anthropology."⁴¹ Moreover, we must all, as outsiders, Native and non-Native anthropologists alike, deal with the ways "local elites" process and control our access to data. For example, she argues, "the pressure on me from activists to take a clear political stand . . . subsided only after I announced that my primary reason for working with them was to contribute (with my research) to efforts at raising the quality of rural education in the country."⁴² Thus, articulating the applied nature of anthropological research should be a requirement for all anthropologists, not just for the Native anthropologist, and we must be more vocal about this fact, both as anthropologists and as Native people.

While still maintaining the us/them, researcher/researched dichotomy, Garcia argues eloquently against the supposed political nonengagement of the unbiased field worker. As she stresses, "non-involvement in a community is not a neutral position, but rather one that can reflect a particular political stance."⁴³ By examining what is expected of the field worker in a complex manner, she opens up the way for a more ethical fieldwork situation, one that could potentially incorporate indigenous notions of respect and give Native communities more control. She points out, "although it may seem obvious . . . the expectation was one of reciprocity. In return for allowing the anthropologist to participate in their lives, to observe, and to conduct fieldwork, s/he is expected to provide something in return."⁴⁴ This counters the idea of the unbiased, disengaged anthropological researcher, which is still the dominant discourse in anthropology. For example, in their review of advocacy in the journal *Cultural Anthropology*, Kirsten Hastrup and Peter Elsass insist that "advocacy . . . is incompatible with anthropology as a distinct kind of scholarship."⁴⁵ Despite this incompatibility, they also highlight the fact that ethnographic practice can and does inform local change and observe that "no anthropologist can escape involvement."⁴⁶

Garcia represents a new openness to the productive subjectivity of all anthropological research and we, as Native anthropologists, will benefit when this becomes the primary discourse within anthropology. Garcia is also willing to think through the breakdown of different knowledge schemes, which may start to answer the call by Medicine to think hard about the theories that drive anthropological advocacy. But Garcia points to a potential problem in collapsing the distinctions between insider and outsider: "while I believe it is crucial to maintain the distinction between social science research and public policy work, increasing involvement of anthropologists in communities sometimes raises questions about where to draw the line between the two fields."⁴⁷

A better ethics, however, would be involved in shifting the privilege away from the outsiders and their knowledge to the insiders and their knowledge. Thus, for anthropology, the ultimate challenge still remains: can indigenous traditions be involved in the subjective making of ethical relationships, as opposed to being only the object of them? Maori scholar Linda Tuhiwai Smith has written an instructive rumination on this particular subject.⁴⁸ Smith argues that having research guidelines is not enough to remake the colonial relationship embedded in the research paradigms of disciplines like anthropology:

Even if such communities have guidelines, the problem to be reiterated again is that it has been taken for granted that indigenous peoples are the "natural objects" of research. It is difficult to convey to the non-indigenous world how deeply this perception of research is held by indigenous peoples.⁴⁹

Thus, no matter what legal requirements or ethical codes of conduct a discipline like anthropology comes up with, they will be rooted in traditions of law that will re-create the colonial relationship. Smith points out, "Indigenous groups argue that legal definitions of ethics are framed in ways which contain the Western sense of the individual and individual property—for example, the right of an individual to give his or her knowledge, or the right to give informed consent."⁵⁰ A partial answer to this dilemma for Smith may be contained within the Charter of Indigenous Tribal Peoples of the Tropical Forests, signed in Penang, Malaysia, in 1993, which includes statements referring to the collective rights of peoples to intellectual and cultural property, participation by indigenous peoples in the management of projects, promotion of health systems, control over languages, and an insistence that "all investigations in our territories should be carried out with our consent and under joint control and guidance."⁵¹

To think through ethical relationships, Smith calls on her involvement as an indigenous person and her participation in Maori

communities of research. She asks researchers to look at *Ngahuia Te Awekotuku*, a set of Maori responsibilities (not rights) when doing work with Maori communities. Her framework is based on the code of conduct for the New Zealand Association of Social Anthropologists, which in turn is based on the American Anthropological Association's guidelines. *Te Awekotuku* sets out fairly basic guidelines aimed at respect for and protection of the "rights, interests and sensitivities" of the people being studied. There are, however, some culturally specific ideas that are a part of what is referred to as *Kaupapa Maori* practices. These are not prescribed in codes of conduct for researchers but tend to be prescribed for Maori researchers in cultural forms: (1) *Aroha ki te tangata* (a respect for people). (2) *Kanohi kitea* (the seen face, that is present yourself to people face to face). (3) *Titiro, whakarongo . . . korero* (look, listen . . . speak). (4) *Manaaki ki te tangata* (share and host people, be generous). (5) *Kia tupato* (be cautious). (6) *Kaua e takahia te mana o te tangata* (do not trample over the mana of people). (7) *Kaua e mahaki* (don't flaunt your knowledge).⁵² Formulating ethics in these ways also defines what types of knowledge can and should be produced within indigenous communities, and it is much more than a set of guidelines—ultimately it will reformulate the power relations between those who study and those who are studied.

These research ethics codes can, and do, go beyond the generalized ethics guidelines of professional organizations. They reflect the research needs of specific communities, and both Native and non-Native researchers should facilitate their creation. For example, my own native community, the Penobscot Indian Nation, has recently formalized a process for evaluating outside researchers and their research. Our Cultural and Historic Preservation Committee has developed an application and review process. The very first question the committee is asked to address in reviewing outside research is "Does this research challenge, undermine, or jeopardize tribal sovereignty?"⁵³ This query serves as a critical place for me to start as a Native researcher articulating the needs and desires of my community. Personally, I take this as the baseline for my research—my internal guideline is something like, "how does this research endorse, elaborate, or enhance tribal sovereignty?" If I cannot answer that question, it is simply not an appropriate project.

CONCLUSION:
BRINGING ANTHROPOLOGY HOME—
WHAT SHOULD IT LOOK LIKE?

Theorization, enhancement, and elaboration on ethical research paradigms are just some aspects of what a Native anthropology can do. In general, we, as Native anthropologists (and researchers in general), must think clearly about the positions we occupy both in our communities

and in our academic institutions, particularly if these are geographically distant from one another. Maori scholar Linda Tuhiwai Smith asks us to clearly differentiate between the "community action projects" within our Native communities and the research we formulate within academic institutions.⁵⁴

The distinction between these two centers of research is useful, to a point. Smith identifies four key aspects of the community-oriented research projects: (1) The community defines the needs and definitions of the research; (2) they must be collaborative; (3) the process of research is as important as the outcome; and (4) local institutions must be involved and help coordinate the research.⁵⁵ These are critical and important themes, and clearly follow on the research-ethics paradigms for indigenous research. For university-based indigenous research programs, she suggests five principles: (1) That we, as indigenous academics, promote research that will "make a positive difference"; (2) that we develop research that will influence indigenous education policy; (3) that we train indigenous researchers; (4) that we disseminate research to our indigenous communities through publication and contact; and (5) that we create an environment for change within the institution where we work.⁵⁶

I agree that these distinctions are important and useful for framing the research programs that we develop, but as indigenous researchers we are often asked to blur these lines—I am just one person doing each of these kinds of research, but all of my research, whether or not it is a "community action project," is supposed to be part of my academic profile and should lead to publication, and this can often present a problem. I am engaged, like many other Native researchers I know, in community-based research projects that involve proprietary information (because it is culturally sensitive or potentially part of a future lawsuit or other legal action), which I cannot, and would never imagine, publishing in the public domain, and thus it will not count in my academic profile. Both Medicine and Smith intimate that there is often an irony in these forms of research projects, which often have to use anthropological and other forms of Western research methodology so that they will be efficacious in defending our lands or resources—a fact that requires us, as Native researchers, to be trained in these disciplines for our own and our communities' self-defense.

While I believe these forms of community-based research projects should "count" in my academic profile, I do not think that the publication of our ideas and research in the academic setting is peripheral to the kinds of projects I am working on with my community. While Smith sees some of this research as attempting to make an impact on policy, Bea Medicine, as a noted above, wants us to develop research programs on "race, class, gender, and power relations in ways that can be understood and utilized by 'target populations,'"⁵⁷ as well as do research that

will show the impact of power structures and research itself on Native communities. As a Native anthropologist, I believe to follow each of their suggestions I must *do research* and *write publications* that will show the ways in which anthropological research paradigms and notions of culture have impacted Indian communities in the areas of, for example, tribal sovereignty, federal recognition, resources rights, and environmental justice. These can all be, and should be, primary areas of inquiry for Native anthropology and will, I believe, impact not only policy-makers but also other academics doing work with Native communities. The impact of these types of publications might also erode the necessity that Native communities use anthropology to defend themselves in the first place, by revealing the biases of these forms of knowledge. In the end, these were the tools I came to anthropology to get—to give some control back to my Native community over the past and future imaginings of our culture and history.

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- 11 Gadamer, "The Problem of Historical Consciousness," 155.
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- 14 I draw this distinction between Other and radically Other because hermeneutic theory postulates an Other simply as another person (see *ibid.*). Anthropologists have been traditionally concerned with the cultures and worldviews of those that are radically unlike themselves.
- 15 James Clifford and George Marcus, *Writing Culture: The Poetics and Politics of Ethnography* (Berkeley and Los Angeles: University of California Press, 1986); George Marcus and Michael Fischer, *Anthropology as Cultural Critique: An Experimental Moment in the Human Sciences* (Chicago: University of Chicago Press, 1986).
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- 17 Marcus and Fischer, *Anthropology as Cultural Critique*.
- 18 Mary Steedly, *Hanging without a Rope* (Princeton, N.J.: University of Princeton Press, 1993), 180.
- 19 *Ibid.*
- 20 Edward Ardener, *The Voice of Prophecy and Other Essays* (London: Basil Blackwood, 1989).
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- 27 Vine Deloria Jr., *Custer Died for Your Sins* (Norman: University of Oklahoma Press, 1969), 78–100.
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- 29 Deloria, *Custer Died for Your Sins*, 275.
- 30 Medicine, *Learning to be an Anthropologist*, 10.
- 31 *Ibid.*, 5.
- 32 *Ibid.*, 325.
- 33 *Ibid.*, 328.
- 34 *Ibid.*
- 35 *Ibid.*
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- 49 Ibid., 118.
- 50 Ibid.
- 51 Ibid., 119.
- 52 Ibid., 119–20.
- 53 Penobscot Nation Cultural and Historic Preservation Committee, "DRAFT Research Evaluation Form" (Indian Island, Maine: Penobscot Indian Nation, 2003), on file with the author.
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- 57 Medicine, *Learning to be an Anthropologist*, 329.