"Side by Side or Facing One Another": Writing and Collaborative Ethnography in Comparative Perspective

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Introduction

In 2000, I began a collaborative project with Cheryl Seidner, the chair of the Table Bluff Wiyot Rancheria, and her sister, Leona Wilkinson, who heads the tribe’s Culture Committee. Seidner, Wilkinson, and I agreed to collaboratively formulate the methods and goals of a project of mutual interest and to also work together to shape the final written form that resulted from our work.

Their great-grandfather had survived the 1860 Indian Island Massacre, where more than sixty and likely as many as two hundred Wiyot men, women, and children were killed by six white men, “known to be landowners and businessmen” from the nearby city of Eureka, California (Kowinski 2004, D1). The Wiyot village of Tuluwat, located on what became known as Indian Island in Humboldt Bay, had been a site where Wiyot bands had conducted World Renewal ceremonies. The massacre was perpetrated while a large number of Wiyots had congregated at the village for these ritual dances. In the years following the massacre, the site was overtaken by a shipyard. By the middle of the twentieth century, it had become desolate, trash strewn, and forgotten except by the descendants of the survivors. Since the time of the massacre, the Wiyot people had not enacted the ceremonies of World Renewal. Much of the remnant population of Wiyot who survived the massacre, and the ravages of alcoholism and endemic poverty that followed, settled at the Table Bluff Rancheria.

Under Seidner’s leadership, the Table Bluff Wiyot of the late twentieth century and early twenty-first century have conducted a determined
campaign to repurchase land on the island, including the site of the massacre, and heal through a cultural renaissance the psychic traumas caused by their near genocide. The Wiyots purchased and rehabilitated just an acre and a half of the island in 2000, having raised funds through the Wiyot Sacred Site Fund. In 2004, the Eureka City Council voted to return forty acres of Indian Island to the Wiyots, “the only city in California to return a sacred site to a native people” (Vogel 2004).

I met up with Seidner and Wilkinson because I was trying to figure out the significance of the abalone mollusk to contemporary California Indian peoples, working with tribes and individuals in those tribes who also thought this was an important research question. The Wiyot telling of the story of Abalone Woman, a spirit-being important to several tribes in northwestern coastal California, is well known in both anthropological and Native circles in California. The story was recorded and translated by linguist Karl Teeter in the 1920s (see Teeter and Nichols 1993).

Seidner and Wilkinson were willing to discuss the story of Abalone Woman, although initially they were unsure of what I was looking for. I asked them what the story meant to contemporary Wiyots, and they spent many hours with me discussing Wiyot history and culture, and how they planned to make Indian Island once again the center of Wiyot ceremony. Neither of them were strangers to collaborative work with non-Wiyots. They had been promoting public remembrance of the massacre among the wider community in Eureka and Arcata for more than a decade, and through extensive interviews with Ron Johnson, contributed to an important volume about the weaving of basket caps (Johnson and Marks 1997). Seidner and Wilkinson have assumed very public profiles in pursuit of healing and remembrance, which also means that their feelings about the genocidal experiences of their people and their consciousness of the need to represent that suffering to the non-Wiyot world remain close to the surface.

In discussions with both women, it became clear that the significance of Abalone Woman’s story had not diminished, and Seidner believes the story’s importance will grow as the Wiyot repair longstanding historic traumas. In response to my queries about Abalone Woman’s meaning for contemporary Wiyot people, Cheryl wrote three installments of a prose-poem reflection on Hiwat (Abalone Woman), Wiyot history, and her own personal struggles and milestones over the course of our three-year discussion. This poem, along with a transcript of
Teeter’s rendering of the Abalone Woman story forms the core of one chapter of *Abalone Tales: Collaborative Explorations of Sovereignty and Identity in Native California* (Field 2008).

After reflecting on the work that Seidner, Wilkinson, and I did, I would say our collaboration was characterized by three principles. First, while my research questions may have sparked conversation, the goals of the collaboration were oriented toward the tribe’s own current struggles. Second, the importance of older, anthropological texts from a different era in anthropology’s own history was acknowledged by both me and the Wiyots, for whom works such as Teeter’s had become less the interventions of outsiders and more documentations of important tribal heritage and intellectual property (see Hanson 1989 for a social constructionist analysis of how anthropological texts written during the apex period of anthropology’s “literary turn” became part of Maori tradition). Finally, all of us accepted as natural and important the use of writing as a collaborative strategy. This final component has been a feature of my work in both California and earlier in Nicaragua, and it is the focus of this article.

The fact that two books of which I am the main author oblige “us” anthropologists to read what “they” (collaborating Native people) write, does not derive from an attempt to pursue a new innovative collaborative technique. Few if any of the individuals I consider most effective in developing increasingly collaborative forms of anthropology (including Joanne Rappaport [2005], Luke Eric Lassiter [2005], Lassiter with Clyde Ellis and Ralph Kotay [2002], Robin Ridington and Dennis Hastings [1997], Joe Watkins [2000], and Gelya Frank [2007]) claim that they are doing something entirely novel or radically at odds with all of the anthropology that has come before this moment in time. Thankfully, that sort of post-modern hubris, which not so long ago claimed for itself both the cutting edge of the present as well as the future of the field, now seems quite dated. By contrast, the work of many collaborative anthropologists has been predicated upon demonstrating the lineage of the ideas and methods that are the foundations for their work. The individuals involved in the current wave of collaborative work acknowledge how collaboration has always been a part of ethnographic research, and in many instances a part of the writing of anthropological texts. This could be true even when the anthropologists who were engaged in collaborative research did not publicly acknowledge or detail the nature of
that collaboration. Several authors have argued that this was the case in a number of foundational ethnographic careers, such as George Hunt’s, with his role in the making of Franz Boas’s anthropological oeuvre (see Berman 1996), and Francis La Flesche’s, with his involvement in the work of Alice Fletcher (Ridington and Hastings 1997).

In the following exegesis of collaborative writing strategies in my work, I first elaborate one possible lineage of such strategies. Through an extended discussion of writing as a part of doing ethnography, which draws on a number of wide-ranging examples, I want to point out both advantages and problems for collaborative research. This discussion does not aim to suggest that such a strategy has a transcendent value for all collaborative work, much less all ethnographic research, but rather its purpose is to help illustrate why it did have such value in my own work. I then return to a treatment of specific deployments of collaborative writing projects in my work in Nicaragua and California. I end with ideas about future directions for this kind of collaborative work.

Critical Reflections on Working Collaboratively

Anthropologists developing collaborative approaches to ethnography and anthropological analysis are exploring and developing a number of different strategies. On the one hand, many of the anthropologists engaged in collaborative projects are concerned about how the foci, goals, and purposes of anthropological work are formulated. In other words, this strategic area focuses on the question What should ethnography be about? This arena of collaborative enterprise has seen the expanding use of anthropological tools and analyses by communities in the pursuit of their own goals, and a simultaneous refashioning of the role of the anthropologist as one among two or several primary investigators, or even as an accessory investigator, in the coalescing of research projects and programs. On the other hand, there is also a great deal of work focusing on the development of new epistemologies and methodologies for planning, conducting, and carrying out both ethnographic research and anthropological writing. That is, this focus concerns the question What is ethnographic knowledge and how is it to be investigated and represented? This arena is increasingly shaped by the distinctive and complex worldviews of the communities with which anthropologists work. The conversation about and reforging of epis-
temologies and methods between communities and anthropologists working on collaborative projects refers to what Joanne Rappaport has named “co-theorizing” (see Rappaport 2005).

It is quite clear that separating these two kinds of strategies as I have done is entirely artificial because in discussing epistemology, one is inevitably led to a discussion of research foci and goals, while any discussion of foci and goals requires a discussion of the methods used to reach those goals. The inability to maintain a separation between these efforts becomes clearer as I focus on the use of collaborative writing as a strategy I have developed and deployed in the two most important and longest-term research programs I have pursued over the past two decades. That is, I have in my work among artisans in Nicaragua, and among Native American tribes in California, asked people with whom I work to write their texts that identify and explore their own analyses of issues and events about which our collaborative projects have focused.

Several possible motivations and factors could bear upon an anthropologist adopting such a strategy. There are, for example, what I would consider unseemly motivations for eliciting the writings of those with whom anthropologists work and conduct research. Anthropologists have routinely used the oral statements and extended discourses of their interlocutors to support, buttress, and sustain their own interpretations and analyses of societies in which they are working. This formulation of ethnographic texts, I would argue, stems from deep-seated ambivalences and ambiguities in our discipline dating from at least the time of Bronislaw Malinowski (see Valentine and Darnell 1998 for an alternate accounting of ethnography’s history), who is frequently represented as the founding father of participant observation ethnography in many cultural anthropology textbooks, and also from some of the work of other iconic ethnographers such as Clifford Geertz. Those ambivalences include a concern for “the individual” as an essential unit of analysis but also an aversion to referring and relating to actual individual people whose opinions and perspectives are distinct, positioned, and perhaps even opposed to one another.

Likewise, the importance laid upon uncovering “the native point of view” in the work of our founding and iconic figures has historically been accompanied by a skepticism bordering upon distrust of the analyses of anthropologists’ “informants.” This sentiment is summed up in the term “folk theory,” which was impressed upon my generation of
graduate students to describe the analyses anthropologists encounter among their “informants.” It is accompanied by the assumption that the anthropologist, because of his or her training and sophistication, inevitably derives a superior analysis.

The lingering effects of these deep-seated binary oppositions in the discipline could lead an ethnographer to elicit written texts from “informants” in the service of the same old exploitative utilization of one’s collaborators’ discourses, but now via an even more fixed and reliable format, that is, the written word. Needless to say, I and the other anthropologists I cite who are developing collaborative approaches are not engaged in this sort of work. But a deeper, more insidious conundrum might also be detected in the elicitation of written texts from those with whom one works in the field. That has to do with the ways that writing shapes narrative by its particular telling of events and the unfolding of ideas, shapings which might under certain circumstances be radically at odds with the conventions for orally relating and explaining narratives in a given society. The significance of writing’s entrance into the narrative traditions of any given society is varied. The historical value of an imperially imposed literacy, for example, might be immeasurable, as in the cases of the indigenous intellectuals, such as Guaman Poma (see Adorno 2001), the authors of the Huarochiri Manuscript (see Salomon and Urioste 1991), the Popul Vuh (see, for example, the translation by Tedlock [1986]), or any of the creators of the books of the Chilam Balam (an example is the Chumayel book; see Alvarez Lomeli 1974). The authors of these texts were survivors of the systematic demolition of indigenous civilizations by the conquistadors, who with the tools of Spanish literacy recorded information about their societies that have extraordinary significance. But those who know those texts well caution the rest of us to be aware of the double-edged valence of literacy in these cases and not to use them blithely or naively. I am certainly not the first anthropologist who has expressed such anxieties, which may become even more acute in situations in which anthropologists are working with indigenous peoples who until very recently have not used writing or understood their histories and traditions via written texts.

Regina Harrison’s book (1989), which provides an excellent introduction to the texts of Guaman Poma, offers one critical perspective on the complex dynamics of the nexus between narrative, writing, and collaboration. More recently, I encountered a well-known text that I im-
mediately understood as collaboratively produced, even though it is not
discussed as such in the interpretive literature that deals with this text,
nor is this text usually brought up in the context of anthropologists
working with indigenous peoples. I found this text, The Kalevala (1835),
relevant and provocative primarily because it emphasizes the historically
framed value of collaboration, including frames that appear to render
irrelevant the question of collaboration.3 By implication, this realiza-
tion means that the value placed upon collaboration recently, however
much I and others appreciate its advantages, is also historically context-
tualized and must be specifically and particularly understood.

The Kalevala comes from Finland, a country about whose sociocultur-
al history (not to mention language) I am far from being a specialist in,
but of which I am instead a novice student.4 In the early nineteenth cen-
tury, a Finnish medical doctor and scholar named Elias Lönnrot assem-
bled a very large number of songs and stories he had elicited from indi-
viduals living in rural areas of Karelia.5 His unique edited compilation
of these spoken texts forms the body of The Kalevala, which has been
for decades considered by many the “Finnish national epic” (Kirkinen
2007, 3), and it is still described as such nowadays perhaps more than
ever. But even in my novice’s reading and study of The Kalevala and the
innumerable critical commentaries of it, it became evident that the
methods and underlying theory of knowledge behind Lönnrot’s work
are not identically reflected in how it was received in his own time nor
since then. In what follows, I review what I have learned to see what
this yields for our understanding of collaborative research and writing.

First, let us consider Lönnrot’s own understanding of his work, re-
corded in his 1835 introduction to the book. According to Lönnrot, he
“collected” and then organized songs from individuals who would sing
them to him with the following criteria:

First, I followed what I observed the best singers paid attention to
in matters of arrangement; and second, when no help was forth-
coming from that quarter, I sought a basis for arrangement in the
songs themselves and arranged them accordingly. (Lönnrot [1835]
1963, 366)

Lönnrot attested that he was searching for “original” and “pure” render-
ings of the songs, and in the preface to an 1849 edition, he elaborated:
Remembering well that these poems are coming to be the oldest specific memories surviving for the Finnish people and the Finnish language as long as these exist at all, one is called upon to arrange them with all possible care and diligence and to concentrate them as well as possible and to include in them everything the songs have preserved in the way of information about the life, customs, and vicissitudes of that time. (Lönnrot [1835] 1963, 374)

Lönnrot’s concern was clearly contextualized by similar efforts starting in the eighteenth century, such as the compilation The Poems of Ossian (Macpherson 1819) in Scotland. The influence of the scholarship of J. G. von Herder, also considered an important figure in anthropology’s own peculiar history, and the emphasis laid upon our discipline to salvage disappearing folk narratives by the Boasians and others, is unmistakable. In Scandinavia, there is an even deeper historical urge to salvage, record, compile, and publish oral and written texts of the vanishing past, seen in the work of Icelandic scholars as far back as the thirteenth century in the case of Snorri Sturlason, although I have not as yet encountered scholarship that describes a relationship between the work of Icelandic scholars on the one hand and Lönnrot on the other. Lönnrot was certainly aware of the implications of his work for anchoring Finnish language and narrative tradition in history for a people who had been under the imperial control of Sweden for seven hundred years followed by the rule of the Russian empire. Even so, Aarne Anttila in 1935 (in Lönnrot [1835] 1963) wrote that because Lönnrot wrote mainly in Swedish, and publicly affirmed his appreciation of and for the Swedish language, he was in his own time considered insufficiently nationalist. In effect, Anttila’s observation highlights the differences between Lönnrot’s intentions in conducting research and compiling its results, and the way his work has been interpreted ever since.

The comments of other critics writing about The Kalevala, such as Salminen and Tarkiainen (1933, in Lönnrot [1835] 1963) drive to the heart of the issues surrounding Lönnrot’s research and the book’s authorship:

There has been some debate as to whether the Finnish people or Lönnrot is to be regarded as the maker of the Kalevala. Although Lönnrot conscientiously preserved the manuscripts and rough drafts which he used, the issue is sometimes viewed as not a little obscure.
The homogeneous epic is the work of Lönnrot. But Lönnrot put the *Kalevala* together not as a real scholar or literary artist but as a singer of traditional songs. He departed from the singers only in that he used writing as an aid to his memory so that it was possible for him to command a great number of song variants. . . . Lönnrot did not wish to add to the *Kalevala* anything at all out of his own head. . . . The verses composed by Lönnrot . . . are only five per cent of the whole. . . . he also had the advantage of literacy and models, especially the Homeric epics. (354–55)

In this passage we see a host of issues concerning methods, knowledge, and interpretation brought into high relief with respect to the making of *The Kalevala*. Because my fieldwork in Nicaragua formed around the shared experience of making pottery with San Juanense master potters, it seems evident to me that the book derives from what I and other colleagues would call a work of collaboration, and that Lönnrot’s methodology—his participant-observation, if you will—formed around his presence in rural Karelian communities as a fellow singer. In the volume I own, the 1963 translation by Francis Peabody Magoun Jr., there are several eighteenth-century engravings and early nineteenth-century photographs of “folk singers” in the act of reciting the songs that Lönnrot compiled and concatenated. In these pictures, as Henrik Gabriel Porthan wrote in 1859,

> the singers sit either side by side or facing one another, close enough to bring in contact their right hands and also their knees (the right knee, of course, of one, the left of the other), on which they prop their clasped hands. While singing they move their bodies gently as if wanting to touch heads, and they assume a reflective and serious expression. On the rarest occasions they sing standing up, and if they ever happen, as if moved by some poetic afflatus, to start singing in a standing position, they soon go and sit down hand in hand and continue their singing in the usual way. (Lönnrot [1835] 1963, 381)

The physical, artistic and emotional intimacy of the collaboration in which Lönnrot was clearly obliged to engage in order to “collect” the songs that form the basis of *The Kalevala* was accompanied by the sharing of these research methods with the other scholars with whom Lönnrot worked. By Lönnrot’s own reckoning, he worked with a team of at least seven researchers.
In the early 1960s, Magoun considered Lönnrot’s achievement to have been the creation “for Finnish posterity [of] a sort of poetical museum of ancient Finno-Karelian peasant life . . . that was, like the songs themselves, already in Lönnrot’s day destined for great changes if not outright extinction,” ([1835] 1963, xiv). By the early twentieth-first century, Kirkinen and Sihvo’s (2007) little volume of Kalevala criticism sought to represent the book not only as the national epic of Finland but also as a universal narrative “for all mankind.” None of the interpretations of Lönnrot’s work, including, I would argue, his own, can triumphantly claim The Kalevala for itself.

What I have learned in my brief but intensive encounter with this extraordinary book is multifaceted. First, I extended my ability to see a collaborative project even when none of the scholars I was reading were looking for or seemingly valued that characteristic. The fact that Lönnrot’s interlocutors were “unlettered” was not ancillary to the way that the collaborative character of The Kalevala has been unworthy of mention in the sources I have read. My own view, which by contrast makes much of the collaboration I see in this book, must also be contextualized in its appropriate time and place just as other interpretive work, such as the nationalist interpretation or the antiquarian salvage approach, derives from different historic sociopolitical contexts. I am not posing anachronistic or utopian propositions to the making and interpreting of The Kalevala, such as wondering how things might have turned out differently if Lönnrot and his team had metaframed their work as a collaboration, or speculating how the outcomes would have been different had Lönnrot’s Karelian interlocutors been literate. Rather, my encounter with The Kalevala underscores how in my work (and the work of other anthropologists) the fact that we are emphasizing the value of collaboration and the fact that our interlocutors are literate exercises a determining influence on the methods and outcomes of these collaborations. Seeing and emphasizing collaboration becomes a part of the epistemology of doing collaborative work in specific research relationships at specific moments in historical time.

It is therefore important to emphasize again that collaboration and collaborative work are not transcendent, inherently superior, supra-historical methods and forms of knowledge that can be applied to all projects in a single manner. The collaborative projects I have undertaken, and the ways writing was a part of them, must be understood in their
particular specificity rather than being considered as examples of an overarching, universally prescribed collaborative approach to fieldwork and analysis in anthropology.

**By Sharing the Writing, We Find What We Don’t Share**

These concerns bring me back around to discussing the specific circumstances in which I first solicited the writings of individuals in communities of Nicaraguan artisans where I had been working. Let me first pay due respect to the early inspiration provided by James Clifford’s critique of ethnographic authority and his call for anthropologists to produce “polyphonic ethnographies” (Clifford 1986), which offers a utopian possibility that contrasts with other approaches to “informants’” words and ideas whether spoken or written. Such a utopia was especially appealing in the context of the revolutionary transformation that a young ethnographer like myself was witnessing in the Nicaragua of the 1980s. In that milieu, a key part of the process of individual and collective empowerment from the beginning, as far as both the revolutionary state and the conscientizado (consciousness-raised) social groups that supported that state were concerned, was the drive to create a literate majority. Literacy was understood on individual, local, and national levels as a means to access knowledge and advance social and economic progress and development. That emphasis made particular “common sense,” from a Gramscian perspective, in terms of the history of Nicaraguan cultural production. For such a small, extraordinarily impoverished, and embattled country, where literacy has always been the preserve of less than half of the populace, there has been an astonishingly long and distinguished parade of poets, playwrights, and novelists who are held in high esteem by the largely illiterate population (Beverley and Zimmerman 1990). Anecdotally speaking, in no other country in Latin America, have I ever routinely encountered more eccentric local historians and storywriters, only too willing to share their laboriously hand-written texts with strangers from foreign countries.

Under such circumstances it was not such a conceptual leap for me to ask several of the people with whom I had been working for over a decade to take pen and paper and put in words their own ideas about what had transpired in their community during the revolutionary period. The camaraderie that had formed over the many months of work-
ing in the ceramics cooperative with San Juanense potters also led me to think about sharing the experience of writing with them just as we had shared the clay. Asking the artisans to write their versions of these narratives also seemed to me part and parcel of the overall revolutionary process and the “democratization of culture” which Ernesto Cardenal had theorized, advocated, and attempted to implement.

What I discovered upon receiving the essays and letters written by my collaborators, and translating them for inclusion in the book titled The Grimace of Macho Ratón (1999) was two-fold, at least. On the one hand, because I retained ultimate authorship of this book, this limited and shaped this collaborative exercise, as I admitted and wrote about at the time, and as Luke Eric Lassiter has also commented about (see Lassiter 2005). More to the point for this discussion, however, I realized that the perspectives of my collaborators and my own mostly converged. But this should hardly have been surprising. My curiosity about what my friends would write followed more than a decade and a half of conversation between me and my collaborators about the events we had lived through together. Those conversations had resulted in much agreement about several important substantive conclusions about the events that had occurred in the mid-1980s when the Sandinista Front decided, ever so briefly, to favor the National Artisans Union over its own Ministry of Culture. Our agreement was not disappointing but nevertheless enlightening about the nature of co-theorizing through the written word. The stories and accounts related by San Juanense artisans were indeed narratives of social transformation and therefore of diverse and heterogeneous narratives of kinds of empowerment, but their voices in my own accounts, as well as my part in their stories, was already present before anything was ever written down. The experience in Nicaragua led me to take on and accept new and very different challenges in my next effort at collaborative production of anthropological analysis, which in some but not all cases involved elicitation of my interlocutors’ written texts.

In this project, I worked for about five years with individuals and tribes located from the San Francisco Bay Area northward almost to the Oregon border to produce a book that focuses on the polyvalent significance of the abalone mollusk in California Indian diets, symbolism, ritual, and mythic narrative. I should say first that for a decade before initiating this project I had been staff ethnologist for an unrecognized tribe in the Bay
Area, which had charged me with helping to produce their petition for federal recognition. I still work with this tribe, the Muwekma Ohlone, in this capacity, which means that I take direction, methodologically and thematically, from the tribe’s chair and its council with respect to the kinds of texts I research and write about Muwekma culture, society, and history. These underpinnings have not undermined the objectivity of the work I have done for the Muwekmas. In other words, I have not been obliged to bend data to fit the Muwekmas’ a priori conception of their cultural and social history. Much to the contrary, the task of confronting federal acknowledgment regulations in my work for them has obliged contestations with the power to bend data of a priori anthropological conceptions about identity and cultural continuity that are stubbornly ensconced at the Bureau of Indian Affairs.

Be that as it may, my relationship with the Muwekmas around formulating research agenda goals and methods prepared me for collaborating with a much broader range of Native Californian peoples in pursuit of the abalone research project, although in this case, I was not strictly directed by any tribe. Halfway into that project, I realized that the research would never have progressed, whatever my own intentions, if it had not coincided with the ongoing research interests of intellectuals and leaders within the Native communities I ended up working with, among whom there were several agendas and goals that were compatible with my own, which were and remained very open. From the beginning, I aimed to investigate whether abalone for California Indians was, like buffalo or salmon elsewhere in Native North America, an iconic animal whose significance congealed multiple layers of daily utilitarian and sacred ritual life. Such an open-ended inquiry, and its congruence with a number of tribal and individual interests and goals, formed the basic precondition of our collaboration, a fact of which I tried not to lose sight as the research progressed. In other words, I understood that I collaborated with these intellectuals, leaders, and tribes not because doing so made it possible for me to pursue my project. Rather, the project was at each step of planning and execution moving forward because it already reflected already agreed upon, although always dynamic, agendas with particular individuals in specific tribes. These agendas derived from and reflected ongoing struggles over Native identity and sovereignty within the participating Native communities. Within those contexts, different kinds of collab-
orations around research and writing advanced the specific individual and tribal agendas at hand. I was obliged not to fetishize one kind of collaborative process—say, for example, the elicitation of texts from my interlocutors—over any other.

Each collaborative process, and the role of writing texts within that process, was therefore distinct and even unique. On the one hand, for two cases I did most if not all the writing, which was oriented around collaboratively negotiated and derived research agenda and goals. In the case of the Muwekma Ohlones, their interest in the project reflected the tribal council’s and chair’s desire to document diet and subsistence practices of the past as well as their growing concern to understand the late eighteenth- and early nineteenth-century material culture in which abalone plays an important role. That material culture, most of which resides in either European museums or in collections in California to which tribal members are not given access, constitutes lost cultural patrimony without which it is very difficult to substantiate the cultural continuities needed to achieve federal recognition, but which without federal recognition will remain inaccessible to the tribe. The tribal chair and council gave me permission to pursue the current locations of several very important artifacts from the aboriginal Ohlone homelands around the San Francisco Bay area, and using funds from a National Endowment for the Humanities research grant, I traveled around California and to Europe to look at, describe, and photograph these objects. Following established practice in my work with this tribe, I then produced text which was then subject to their critical review and over which they exercised final editorial power.

In the second instance, a much more emotionally and historically wrought process hinged upon the reconsideration of a coastal Pomo elder’s family history and that family’s long-term relationship to anthropology. The elder and I agreed to look over and then critically discuss decades-old (forty- to seventy-year-old) anthropological texts that described life histories of members of her family along with their religious beliefs. My intention in this process was to attempt to rectify inaccuracies about this elder’s family history and present a more respectful and humanistic portrayal of her ancestors. Her intentions, if I may presume to summarize them, were to become more familiar with what was written about her family and her tribal region, and to help me understand her own views on these matters. Her family history was
not the only focus of our collaboration, which in fact had been initiated around and continues to center on the uses and significance of abalone for coastal Pomo groups. I would describe the chapter in *Abalone Tales* exploring these themes as “co-produced” through extended discussions and redrafts of manuscripts over whose editing the elder exerted final decision-making power.

In other collaborative work explored in *Abalone Tales*, tribal leaders and scholars wrote text that either accompanied mine, in one case, or dominated an entire chapter of the final volume, in another case. In working with the Wiyot Tribe, the resulting material became the former type, as tribal chair Cheryl Seidner, whom I described in the introduction to this article, had been actively engaged in addressing the legacies of social traumas from the genocidal ethnic-cleansing campaigns of the mid-nineteenth century that her tribe had never recovered from. Because Seidner considered the work of early twentieth-century anthropologists who had recorded extensive mythic narratives about an abalone spirit-being among the Wiyot part of her peoples’ cultural patrimony, she put a high value on continuing discourse about that spirit-being, and doing so through writing. She therefore undertook to explore the writing of a three-part prose poem. Similarly, in the chapter that explores the complex ritual relationships between the Hupa people and abalone, one individual, a well-known storyteller, produced a textualization of the particular telling of the abalone spirit-being narrative in her family, which was included in the overall text. Other Hupa people who are deeply involved in the tribe’s ceremonialism coproduced different sorts of texts based on conversations and interview transcriptions that formed other sections of the chapter.

One chapter of *Abalone Tales* is entirely written by someone other than myself. For Julian Lang—Karuk linguist, published intellectual (see Lang 1994), and performance artist—talking about abalone provided him with the opportunity to advance his own long-term ongoing grand goal: the reconstruction of the Karuk mythic narrative cycle. Long before I met Lang, he had been wondering about the abalone spirit-being in his own people’s narratives and had taken action as an artist and a researcher. He had assembled and shown an innovative installation about the abalone spirit-being for a gallery in San Francisco in the early 1990s, and had also undertaken extensive archival research about the abalone story. He took the opportunity afforded by our interaction to
write a chapter-length exploration of this work. The scholarship in that chapter straddles and combines academic and tribal notions of expertise, knowledge, and methodology.

Concluding Thoughts

As I reflect on the specificities of the collaborative projects I have described, I recall Bruce Mannheim and Dennis Tedlock’s discussion of what they call “the dialogic emergence of culture,” by which they referred to the strange intertwining between ethnography and ethnography’s object “culture” (1995). The way I read their analysis, anthropology’s interventions in the histories of the peoples that anthropologists have spent their time studying have become part of those histories, and the result is an intimacy between those peoples and anthropological work. (See Hanson 1989 for a more strictly “invention of culture” view.) This is extremely evident for the Muwekma Ohlones, for whom the ethnographic notebooks of John Harrington have played a central role in their federal recognition work. Julian Lang has on more than one occasion mused about a revision of Alfred Kroeber’s 1925 tome *The Handbook of the Indians of California*, rewritten by anthropologists from each tribe. Such musings derive from both an acknowledgment of the importance of Kroeber’s work for the California Indians of today, as well as a desire to place the multiple heterogeneous voices, perspectives, and analyses of California Indians into the forefront of the dialogue between Indians and anthropologists.

I am therefore of two minds as I think of these collaborations. On the one hand, I know that the nitty-gritty of fieldwork has always involved a collaborative intimacy conjured by the image of Lönnrot’s research: “The singers sit either side by side or facing one another, close enough to bring in contact their right hands and also their knees (the right knee, of course, of one, the left of the other), on which they prop their clasped hands.” On the other hand, anthropologists’ willingness to fully acknowledge and theorize the significance of their collaborative methods and epistemologies has varied quite widely. It is about the specific outcomes of that full acknowledgment that this article has been directed.

With respect to the production of *Abalone Tales*, I maintained my concept of writing as both a means and ends with respect to individual and
social empowerment, the concept that had inspired my earlier collaborative project in Nicaragua. In the case of collaborative work in Native California, collaborative research and writing ultimately intersect with my overall intention of directing anthropological tools toward the pursuit of tribal goals. Ultimately, and in the near future I hope, what we read about indigenous and aboriginal communities, should be, in the main, what they write, as Julian Lang also intends. That will not necessarily shut non-Native anthropologists out of the conversation, but it would change everything with respect to methods, agendas, and goals. I hope my collaborative research and writing efforts play a role in that change.

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Notes

1. A cycle of ceremonial dances including the White Deerskin Dance, Jump Dance, Boat Dance, and others have historically been conducted by a number of different California Indian peoples, including the Wiyots, Hupas, Yuroks, Karuks, and Tolowas. The intention of these dances is to simultaneously give thanks for food and good fortune and to try to recalibrate social harmony so as to reproduce social and environmental benefits. Anthropologists and Natives alike refer to the ceremonial cycle and the philosophy behind it as the World Renewal religion.

2. Geertz, the famous pioneer of interpretive anthropology, did not always let individual people with whom he worked have much say in these interpretations. In many essays, including his most famous, “The Balinese Cockfight,” he had successfully gotten “inside the heads” of his informants, at least to his own satisfaction, and spoke (in other words, wrote) almost entirely on their behalf.

3. The edition of The Kalevala that was available at my university bookstore was Francis Magoun’s 1963 translation published by Harvard University Press. It was a fortuitous purchase because this edition features quite a few different essays written by authors, including Lönnrot himself, from both the nineteenth and twentieth centuries.

4. I went to Finland in the fall of 2007 to participate in a traveling exhibition of Nicaraguan pottery from the village of San Juan de Oriente, where I have worked since 1982. The exhibition was organized by Ricardo Alvarado, the Nicaraguan ambassador to Denmark and Finland, and his wife Paula Blomster, a Finnish anthropologist. In addition to giving a paper about San Juanense pottery at the University of Helsinki, I prepared for
this trip by reading The Kalevala, and during my stay in Helsinki, spent time investigating and talking about the artistic and literary legacies of the “Finnish national epic.”

5. Karelia is a historically Finnish-speaking region that has also been central to the development of Finnish nationalism and cultural awareness. Remote from urban areas and influences, the inhabitants and their traditional songs, stories, crafts, and sociality persisted long after industrialization had stimulated social and economic transformations across much of Finland. Karelia has been in contention between Russia and Finland both before the latter gained its independence from the former in 1917, as well as after Finnish independence, particularly during World Wars I and II. Currently Russia controls the greater part of Karelia.

6. Ernesto Cardenal—poet, activist, artist, Catholic priest, and Marxist philosopher—became the minister of culture under the revolutionary Sandinista government of 1979–90. The implementation of his ideas had significant consequences for artisans such as the San Juanense potters.

References


Tedlock, Dennis, and Bruce Mannheim, eds. 1995. The Dialogic Emergence of Culture. Urbana: University of Illinois Press.


