The ethnography of language and language documentation

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Introduction

Documentary linguistics takes up a vision of the integration of the study of language structure, language, use, and the culture of language. Documentary linguistics demands integration. If we are to succeed in sensitive documentation, which by definition requires the deep involvement of communities, we must incorporate a cultural and ethnographic understanding of language into the very foundations of our research. Indeed, documentary linguistics, because of practical necessity, may have a better chance of sustaining such an integrated project than did its predecessors.¹

This chapter focuses on three requirements for the integration of the study of the culture of language into documentary linguistics that have an immediate practical relevance for this new discipline. The first is to move forward with the foundational idea from Hymes’ (1971) formulation of the ethnography of speaking, as the study of the way that language structures and uses are diversely and locally organized in the cultures of local speech communities. Documentary linguists need to be ethnographers, because they venture into communities that may have very different forms of language use from those of the communities in which they were socialized as human beings or trained as scholars.

The second requirement is to attend to the cultural foundations of elicitation and second language learning specifically. Documentary linguists undertake to inhabit a very peculiar role, that of adult second language learner in communities that almost never encounter such a creature. Similarly, their consultants enter into relationships that are without precedent in their communities. Together, they constitute so-called communities of practice, local micro-societies that are very likely to produce emergent forms of language and interaction that evolve very rapidly. Recent work on communities of practice, specifically learning communities, provide very useful theoretical foundations for understanding what is likely to go on in these
most dynamic of local systems, where goals and routines are negotiated at the level of distinct individuals.

The last requirement is attention to language ideology. One of the reasons history speeds up at the margins is that oppression and marginalization – and minority and indigenous language communities are almost by definition oppressed and marginalized – produces a special intensification of language-ideological projects. These can silence the voices of speakers, render untenable the presence of a researcher, or impede the distribution and implementation of the products of research, even within the community. Recent advances in our understanding of the semiotics of language ideologies provide very useful tools for documentary linguists, who must be able not only to identify and work among clashing ideological discourses, but assist communities with what Nora and Richard Dauenhauer (1998) have called “ideological clarification” to bring these discourses into line with what a community truly desires for endangered-language resources.

1. The ethnography of language: Relativity and the organization of diversity

Most linguists attend almost exclusively to what Michael Silverstein (1979 and elsewhere) calls “denotational text.” We can state the formal properties of declarative vs. interrogative vs. imperative sentences, for instance, without really paying much attention to the well-known fact that both assertions and questions can function as commands, or that commands can be made only under certain social conditions. But documentary linguistics on languages that are no longer taken for granted, where every construction carries a heavy political burden, really does not permit us the luxury of this particular reduction. We can find practical help in some of the foundational principles of the ethnography-of-language tradition.

The first of these principles is that speech communities will differ not only in manifesting different kinds of language structures, but in manifesting different patterns of use. An ethnography of the distribution of registers, speech-act types, and the like across the contextual landscape is critical to linguistic documentation. For instance, certain kinds of syntactic constructions may occur only in certain registers, so that even basic elicitation strategies will require ethnographic preparation. Hymes’ well-known SPEAKING heuristic provides a rule of thumb to help us notice patterns of usage. The acronym “SPEAKING” abbreviates some of the major components of the
speech situation: Setting/Scene, Participants, Ends, Act Sequence, Key, Instrumentalities, Norms, Genre (Hymes 1971; Saville-Troike 2003 offers a more comprehensive compilation of analytic units in the ethnography of communication). We need such heuristics, because patterns of usage are not always noticeable or easily interpretable. While we encounter some patterns as weird and jarring, others are so easily naturalized that they become invisible before we ever notice them. I have two rules I share with my own students: The first is to always assume that a difference is meaningful, not natural. The second is never to assume that a difference is due to inadequacy on the part of speakers. Indeed, for the ethnographer, the feeling that your interlocutors are rude, or stupid, or crazy, is an extremely useful signal that you have probably bumped into a very interesting difference.

Let me give an example of a mistake of my own, where I assumed that a difference was natural instead of meaningful. When I was working in central Mexico and would visit my Nahuatl-speaking friends in their homes, they would greet me with a peculiar intonation contour that starts in falsetto and terminates in creaky voice. Women do a particularly exaggerated version of this “squeak-creak” contour. I simply did not pick up on this as the highly formal politeness that it was. Why? I think the reason is that most people in this population are physically rather small. It is not uncommon for older women especially to be less than 150 centimeters tall, and I often felt like Gulliver among the Lilliputians. The falsetto voice of the squeak-creak contour seemed a perfectly reasonable sound to emanate from these tiny little women, and I never stopped to think that in fact on other occasions they spoke in perfectly normal voices. I had been in and out of the field in Tlaxcala for four or five years when the Mayanist linguist Louanna Furbee asked me at a conference party if Nahuatl speakers used the same polite falsetto that she had heard among the Tojolobales, a Mayan community of the Mexican state of Chiapas. I had the sort of experience that cartoonists represent by showing a lightbulb going on in the balloon above the character’s head; suddenly I could hear my friends saying, “*Comalehtzi:n! Ximopano:litzi:no! Ximotla:li:tzii:ni!”* and realized that what I had been hearing was not a natural index of how small they were, but a highly meaningful message expressing social distance and hierarchical order. They meant not just “Comadrita! Come in! Sit down!” They also meant, “We are greatly honored by your presence.” Fortunately my failure to understand exactly what they were doing did not, I think, have much impact on my work. But other cases of “naturalization” might have precisely such consequences. It is for this reason that one of the ethnographic arts is to “make
strange,” always to ask, “Why did that just happen? How might it have been different? Does it mean what I think it means? Can I find evidence in favor or to the contrary?” Staying for months on end in the hypothesis-testing mode of “making strange,” rather than simply “being there,” is exhausting, and we will always slip, but training in this ethnographic attitude and how to sustain it is essential for documentary linguists. And the rule of thumb – “Assume difference is meaningful, not natural” – is very helpful.

In contrast to differences in usage that are easily naturalized, some differences in usage are highly salient and even startling. These are the kinds of differences that are categorized under “cross-cultural miscommunication,” that lead people from one community to conclude that those in another are uncivilized or stupid. I want to give an example that will not only show how such differences are some of the most interesting for the ethnographer, but also to show how deeply embodied in speaker habitus the differential patterns of language use are, and how departures from them will seem almost physically uncomfortable. One extremely annoying feature of my fieldwork in Mexico was working with people who treated appointments – compromisos – as less than fixed. When I tried to make appointments for interviews, people would smile happily and tell me to come “a una buena hora” (literally, ‘at a good hour’, which turns out to mean “early”), and assure me that “primero Dios” (‘if God wills it’), they would be pleased to be available to help me. About 60% of the time people in fact kept such appointments. But on more than a few occasions I arrived for the appointment only to learn that the intended interviewee was far away on some errand that could have been easily predicted, such as a pilgrimage to a saint’s festival that was fixed on the annual calendar or attendance at a market that occurred on the same day every week without fail. I knew better than to think of them as rude or insincere, and began to think about why this happened. Eventually I developed an account of it in terms of the theory of types of “face” from politeness theory (Lakoff 1973; Brown and Levinson 1987), which was very helpful in understanding other communicative problems as well. Put briefly, these communities were heavily biased toward attention to so-called “positive face,” everybody’s right to feel wanted and liked. In local terms, to make a social commitment that you could not keep was a fairly minor white lie, while to say “No” to someone’s face, even very politely and with elaborate excuses, was a major threat, a threat to positive face. The threat to my negative face (the right to the autonomy that would permit me to avoid inconvenience) was practically irrelevant. I would be annoyed when I found myself 50 kilometers from my home base
in front of a house compound that was deserted and locked up tight, but nobody would be there to notice. In fact, I learned that in general threats to negative face hardly counted at all in the Mexicano communities. I learned as well that when there is any possibility of a “No” in a matter where an insincerely-uttered “Yes” would create inconvenience of a kind intolerable even for these people, that intermediaries were sent to pose the question. So I did have a reasonable understanding about what was going on, and even published an article on the local culture of politeness (Hill 1980). I didn’t make the mistake of thinking of local people as rude and inconsiderate. But now comes the tough part – I found it practically impossible to tell the little white lies about keeping appointments that everybody else used. If someone said, “Next week, let’s go and visit the church at Ocotlan, my daughter needs a ritual cleansing and you can take us there in your pick-up truck,” and I knew that next week I was expected in Mexico City at a professional conference, I would carefully – politely, in my terms, incredibly rudely in theirs – explain that I had a previous engagement but might be able to visit the Virgin of Ocotlan another time. I knew the “Primero Dios” routine perfectly well, understood its deep cultural foundations, and simply could not do it. In my cultural calculus, which I could not seem to set aside, the threat to negative face – the idea that someone might be inconvenienced if I didn’t show up – was truly dire, while saying “No” politely to someone’s face was a very minor matter. Although I attempted the Primero Dios routine occasionally when I thought the matter at hand was a fairly light one, I suspect that I acquired a reputation as a rather rude, stuck-up, and negative person, but I simply couldn’t help it. The American linguist Doris Bartholomew, who worked for 40 years with Otomi speakers in a part of Mexico near my own field site, told me that she finally learned to accomplish this particular flavor of social lie with a straight face, but that it pained her every time. The lesson of this case is that diversity in usage is not merely colorful, or interesting, but that it can be very, very hard to live with, even for a person with extensive anthropological training.

A second foundational presumption of the ethnography of language is, of course, that speech communities are not linguistically homogeneous, but are “organizations of diversity.” The idea of the speech community as an “organization of diversity” is a very useful one for students of minority languages who encounter communities that are at the very least bilingual. Especially important, of course, is the distribution of the linguistic resources of the minority language versus the other language or languages across the repertoire of possible speech events and acts, across genres, across the kinds
of speakers and addressees, across channels, across affective keys, and the like.

This organization of diversity has very practical consequences for our work. Again, we can note the problem of “naturalization” of difference. I never really learned Nahuatl very well when I was working in Tlaxcala, the reason being that hardly anybody ever spoke it to me until I had been returning to the communities off and on for almost a decade. This seemed reasonable; I speak halfway decent Spanish, and so do they, so it was just easier for everybody to use that language and that was how I initially thought about what was going on. But in fact this was much more than just a matter of “least effort.” People spoke Spanish to any stranger or outsider, no matter what their native language might be. It was quite astonishing to go to a public market and hear obviously indigenous sellers speaking heavily accented and even ungrammatical Spanish to equally obviously indigenous buyers throughout all the stages of the bargaining process until the very end of the event, when the deal was clinched and a few words of Nahuatl would be exchanged to express the solidarity that came in the moment of a successful transaction.

The sociolinguistic conventions that distributed Nahuatl and Spanish across the local contextual landscape would have had the most profound effect on my fieldwork had I been documenting grammar rather than language shift, since they would have made it very difficult for me to hear certain kinds of constructions or access certain lexical domains. I think it has been shown that gaining a speaking competence in a language under investigation is a prerequisite to truly sensitive description and analysis. But it was very difficult to do that in the Nahuatl communities. I did try, but without much success. I had the opportunity once to talk to a local veterinarian who had learned to speak Nahuatl, not only to facilitate his work, but because he was deeply interested in the language and its history. He discovered, however, that people did not respond well to him when he spoke it to them. He said, “When I speak it, they don’t respect me.” He had unwittingly run afoul of a convention of metaphorical switching that involves the use of Spanish even by Nahuatl speakers when they discuss technical topics, and, unfortunately, also of linguistic insecurity associated with Nahuatl, the idea that people who speak it are not as good as people who speak Spanish. If his interlocutors were relative strangers, he was probably even insulting them by suggesting that they did not know Spanish. Finding contexts for speaking the language in such circumstances requires the most careful analysis of how the various languages in a community are
deployed, so that the face and reputation of all interlocutors are properly attended to. Indeed, any community may have certain kinds of speech events in which outsiders simply cannot successfully participate. For this reason, and also because it is both ethical and sensible to build local capacity, it is generally preferable to train local native speakers in recording techniques and have them do most of the basic recording themselves.

2. Documenting languages in a community of practice

The kinds of diversity in patterns of usage studied by ethnographers of language have often been treated as relatively stable in communities. But documentary linguists must also attend to contexts in which new conventions and forms of diversity can emerge very fast: the contexts of elicitation and adult second-language acquisition that are at the center of their work. Linguists who do field work have understood for many years that elicitation is a collaborative process that requires mutual adaptation on the part of researcher and consultant. Early attention to the problem of what happens in elicitation and in the kind of adult second-language learning that documentary linguists undertake focused mainly on problems that would emerge from different patterns about matters like asking questions. Charles Briggs’ Learning how to ask (1986), where he argues that the acquisition of new information must be embedded in local social understandings of who is permitted to ask what kinds of questions to whom, is a classic discussion of this issue. Some anthropologists, including Briggs himself, have found that the best way to work is to undertake what is locally understood as an apprentice role. I don’t think this approach is a solution to the problems faced by documentary linguists. Communities may have well-established institutions for apprenticeship in wood-carving or divination. They will certainly have very well-established patterns for first-language socialization. But it is highly unlikely that they will have well-established patterns for adult second-language learning or elicitation. And certain local patterns for adult learning may be quite inappropriate to the documentary linguist’s task. A very good example is the routine of adult acquisition of ceremonial orations and creation accounts among the Tohono O’odham of Arizona described by Ruth Underhill (1946). A man (it was always a man) who wished to learn a particular oration would approach someone who knew it and present a very important gift, consistent with the significance of the target text – blankets, a rifle, a horse. If the source accepted the gift, he
would then recite the oration: once. The job of the apprentice was to listen with the most intense focus, to try to master as much of the oration as possible from this single recitation. Because if he needed to hear it again, another expensive gift would be required. This particular method really would not work for most documentary linguistics—indeed, it has been tried. The linguist Bill Graves described in his dissertation (Graves 1988) encountering a Pima speaker, an immensely knowledgeable elder who had been very highly recommended by everyone, who chose to organize his role as linguistic consultant along the lines of the traditional model for learning that Underhill had described. Graves had to arrive early, because if he was even five minutes late for an appointment Mr. Brown would refuse to talk to him. Graves had to listen with the most extreme care, because Mr. Brown spoke very quietly, did not like repeating things, and refused to explain things. Mr. Brown would occasionally rise abruptly and terminate a meeting if he was annoyed. Finally, Mr. Brown required cash up front at every meeting. After a summer of this sort of thing, Graves reluctantly concluded that Mr. Brown was a bit too traditional and sought a consultant who was willing to compromise.

The absence of established routines for adult second-language learning and linguistic elicitation in most minority-language communities makes it obvious that elicitation will produce some kind of new system that emerges in collaboration. New theory in “learning how to learn” shows that such emergent systems are always produced in learning communities, even in ones that seem well-established and stable. Learning communities belong to the category of social organizations that have come to be called “communities of practice.” Eckert and McConnell-Ginet (1992: 464) provided a founding definition of this entity: “A community of practice is an aggregate of people who come together around mutual engagement in an endeavor … practices emerge in the course of this mutual endeavor.” Meyerhoff (2002) has usefully summarized the theory of communities of practice, which have become an important unit of analysis in recent variationist sociolinguistics.

The key elements of Eckert and McConnell-Ginet’s definition are mutual engagement—which may be “harmonious or conflictual,” and the endeavor, which Meyerhoff defines as a jointly negotiated enterprise, which must be reasonably specific. Finally, a community of practice will develop a shared repertoire of normative practices and interactional resources that are “the cumulative result of internal negotiations” (Meyerhoff 2002: 528). These subcomponents are in dialectical relationship: mutual engagement both makes possible, and is made possible by, the negotiation of a joint
enterprise, and normative practices are negotiated and in turn facilitate negotiation and mutuality. The "communities of practice" in which documentary linguists work are, then, different from the "speech communities" of the classic ethnography of language. They may be constituted only for particular purposes, they may be ephemeral, and they can form and reform, being salient at certain times and places and irrelevant in others. Furthermore, single individuals may belong to several of these, and their practices and routines may overlap to some degree.

Wenger (1998) found that successful communities of practice exhibit certain properties that are highly relevant to the documentary linguistic enterprise. These include

1. rapid propagation of innovation;
2. jargon and shortcuts to communication;
3. the development of a certain very local insider perspective on the world;
4. a repertoire of insider resources and identifying markers such as jokes, stories, and specific tools and representations.

Specifically linguistic variables such as phonological elements, lexical items, and routinized phrases are a very important part of the emerging normative order within communities of practice. That is, linguistic resources evolve within communities of practice and may be quite specific to these.

The problem for the documentary linguist is to be aware of these emergent properties, and to try to remain conscious not only of her own role in such emergence, but of what consultants are doing as well. To think through thoroughly the implications of the evolving theory of the community of practice for the documentary project lies beyond the scope of this chapter. But I will advance a couple of simple and suggestive examples from my fieldwork with Cupeño, undertaken more than 40 years ago when not even the tiniest ray of social-constructionist light had yet penetrated my American structuralist training. I spent nearly all of my time working with a single consultant, Roscinda Nolasquez, who was then in her mid-sixties—about the age I am as I write this. I thought of her as very old. We spent hundreds of hours together, and became very intimate, a classic community of practice of two, in which marginal members occasionally participated for brief periods.

My first example of an emergent property within our community is the fact that my fieldnotes, to my extreme embarrassment today, are very messy
and often do not have glosses, in spite of the fact that I had some training in field methods. This is an excellent example of a “rapidly innovated shortcut.” In 1962 I was immersed in the language and had no trouble understanding anything in the notes, and really didn’t need to systematically gloss everything, and could use ellipses for predictable (to me, then) parts of utterances. And of course this was also fine with Roscinda Nolasquez, who was very quick-witted and did not enjoy waiting while I carefully wrote things down and glossed them. We had developed a sort of rapid work rhythm and my sloppy note-taking was one of its dimensions. And I note that I’m not the only person who ever did this. Shortly before his untimely death in 2001, Ken Hale turned over his field notes on Mountain Pima from the late 1950s and early 1960s to my graduate student, Luis Barragan, who works on the language. Luis was very moved when Ken offered him the notes, and awed when he discovered that only six pages into the notes Hale, who of course was famous as a linguistic savant, stopped writing glosses. I assure you that my glosses for Cupeño are fairly dense for many more pages than six, but after two or three weeks of work they became scantier and scantier. This is exactly what we would expect from findings about communities of practice, where shortcuts emerge very rapidly, but of course what it means is that my notes (and Ken Hale’s) are now very difficult to use. I was so immersed in my local formation of community in the summers of 1962 and 1963 that I did not think about how, forty years down the line, there would be nobody alive to check the odd form that I really am not sure about any more. So one of the lessons is that documentary linguists really do need to keep in mind, in the face of the profound force of local social construction in the linguist-consultant relationship, that they belong to a larger community with its own needs.

And of course consultants are contributing to the emerging structuration of the community of practice and its products. To discuss one of these contributions by Roscinda Nolasquez, I need to give you some background on Cupeño demonstratives. Cupeño has three demonstratives: *i’i*, a clear proximal, *axweš⁷* a clear distal, and a mystery demonstrative *et*. In writing my reference grammar (Hill, to appear) over the last few years, I had to figure out what on earth the mystery demonstrative meant. What I determined was that *et* and *axweš⁷* are contrasted as distal-proximal and distal-obviative. Part of the evidence was that only *axweš⁷* appeared in narrative, except for passages of reported speech, in which *et* could appear. The other bit of evidence was that *et* was absolutely ubiquitous in elicited sentences, where *axweš⁷* never appeared. For instance, in one section of field notes I
was investigating which noun stems would accept locative suffixes directly, and which required relational noun constructions. I figured a fly could sit on just about anything, and put a fly in all sorts of absurd places – on the basket, on the acorns, on the string, on the berries, on the cow, etc., in sentences for Roscinda to translate. She always translated English “a fly,” as et ku’al “that fly” – the distal-proximal (virtual) fly to which we were both paying attention. The combination of the presence of et in elicitation and in reported speech in narrative suggested that its function was “distal, but within the zone of attention of discourse participants.” On the other hand, axwesh meant “distal, but not available to discourse participants.” Hence, et ku’al, the mutually-imagined fly of the context of elicitation, but axwesh isily “that coyote,” a character of the mythic time who appears in narrative.

With my new-found understanding of the demonstratives, I am now able to more fully understand Roscinda Nolasquez’s goals, and why she was willing to spend so much time with me. At the time I had completely naturalized the idea that an American Indian community should include only a few elderly speakers of a heritage language. As far as I could tell there was almost no interest in the language; Roscinda never mentioned any regrets about being one of the last speakers, and handled most of her life in English. Indeed, she positively avoided talking to a couple of other women of her age who were speakers, because she didn’t like them. She called what she did with me “teaching.” But, looking at my notes forty years later, I could see that she was trying to accomplish much more: She was documenting, recording an archive, although she never said as much. And the distribution of the demonstratives became one of the key pieces of evidence for this.

Roscinda really liked best of all to record stories and histories. After a couple of months of work, she said that she wanted to tell about how the Cupeño had moved from their original homeland at Kupa, Pal Atingve, to their reservation at Pala. This is a dreadful story, of legal machinations by greedy Whites and a desperate battle by the Cupeño to keep their lands, which included valuable hot and cold springs in an arid region of San Diego County in southern California. Roscinda was nine years old in 1903 when she and all her relatives were packed into wagons and moved out of their beautiful village with its sturdy adobe houses and inviting pools of hot and cold water and moved to Pala, to live in tents in the flea-ridden willow thickets along the San Luis Rey river designated as their place of exile. She told the story of the removal on three separate days. On the first day, she narrated almost entirely from her own point of view, using almost no reportative evidentials. When she resumed again on the second day, she began
by labelling her talk as *a'alxi* “reciting history.” In this section and in the third section, the reportative evidential appears frequently, even where she is describing scenes in which she played a role (such as the rescue of her pet cats). On the first day, narrating as a sort of conversational account of a personal experience, she uses the base *eve-* , the inflectional base of *et*, almost exclusively for the locatives. That is, even though the places being referred to are not “in the immediate discourse context,” she refers to them in the voice of an interlocutor in dialogue with the listener (in this case, me, Jane Hill), who has been initiated into the world of the narrative and is taken to share her point of view. But in the second and third telling, the base *eve-* is entirely absent, and all references to place are with the base *axwa-*, *a-*, the locative bases of the obviative demonstrative *axwesh*. That is, in her second and third telling, Roscinda Nolasquez speaks in the voice of an “historian”; she animates a tradition, rather than engaging directly with me as her interlocutor. And it is clear that her descendants recognized what she was doing. One of the ways that Cupéeño have always used their oral tradition is to borrow lines from it to make songs. And singers today have taken lines from my recordings of Roscinda Nolasquez’s account of the removal. When I returned to the community a year and a half ago, I was treated to a performance of men singing to rattles, and was very moved to encounter a beautiful new song, composed for the 2003 centennial of the removal, that used a line that appears in her telling: *Peta'amay che'mixani chemewa$h Kupangax* “We lost everything from Kupa.”

In summary, the moral here is that what Roscinda Nolasquez took to be the mutual goal of the community of practice that we formed in the summers of 1962 and 1963, to document her language and its traditions, shaped even very fine details of her speech. In elicitation, where the sentences would have no historic significance, her demonstrative was *et*. In reciting texts where the sentences would have historic significance, she used obviative *axwesh*. So the notion of the community of practice teaches us that the ethnography of language in documentary linguistics must take as its site for study not only the organization of diversity in the speech community, but also organization and patterning that is emergent, including emergent in the context of elicitation and language learning itself.
3. Language ideology and documentary linguistics

The last set of ideas to be presented here involve how we can attend to the very fast-moving dynamics of language ideology in endangered language communities. Something of the significance of language ideologies has been recognized for a very long time. For instance, the early ethnographers and linguists working in indigenous North America discovered that accounts of the creation were fully performed only in the winter, and so they could not be elicited in the summertime; indeed, people thought it was dangerous to do so.

But the early ethnographers thought of this kind of ideologically-driven pattern as simply one more stable difference between them and their consultants. Today we are finding, though, that these ideological systems can evolve and spread in communities with astonishing rapidity. I will discuss an example that unfortunately I had to observe at immediate second hand – the contretemps around the publication of the Hopi Dictionary, for which my husband Kenneth C. Hill was project director. The Hopi, who live in northeastern Arizona, are the western-most of the Puebloan societies. Paul Kroskrity (1998) has shown how in the Puebloan communities of the U.S. Southwest, all indigenous language tends to be ideologically assimilated to the prototype of ritual language, the language of the kivas. Kiva knowledge is not shared with people who have not been initiated into the relevant ritual societies, and many of the pueblos have decided that their language is strictly for insiders. Indeed, one Hopi linguist, briefly employed at the University of Arizona about 30 years ago, refused to teach the language to non-Hopi students. A second point is important in understanding the dictionary controversy: During the period when public ceremonies are underway, the Hopi villages construct a sort of “anti-market” economy that extends the practice of the kiva to the entire village: nothing is sold, everything that one might need is given as a gift.

This was the background ideological context in which my husband worked for more than a decade with colleagues Emory Sekaquaptewa, Ekkehart Malotki, Mary Black, and others to compile the great dictionary of Third Mesa Hopi (Hopi Dictionary Project 1998). During the period of the research only the most minor difficulties appeared; all tribal officials were involved and participating. They all knew that the project was the brainchild and dream of a senior Hopi, Emory Sekaquaptewa. The dictionary research group was extremely careful of Hopi ritual sensitivities, and a committee of Hopi elders made sure that the dictionary would not contain
anything that would be in violation of ritual prohibitions. Arrangements were made to distribute dictionaries free to schools and at a greatly reduced price to Hopis, and all royalties were to be paid to the Hopi Foundation, a non-profit foundation dedicated to Hopi education. However, when the publication date of the dictionary neared, the University of Arizona Press proudly published a handsome full-color brochure as an announcement of this major work, in which a price of $80.00 for the volume was mentioned. This announcement finally made public and unavoidable what everyone had managed to keep in the background – that the dictionary, which had been largely funded by money from the U.S. government’s National Endowment for the Humanities, would be available to non-Hopis, and that it would be sold. This precipitated a difficult year during which the Hopi Director of Cultural Affairs, Lee Kuwanwisiwma, supported by many other Hopis, argued that the dictionary should not be published at all because the Hopi language should not be bought and sold, and certainly not for the benefit of non-Hopis. Eventually the political faction that supported the dictionary prevailed and it was published, but this result was by no means guaranteed (Hill 2002 discusses this episode).

Recent theoretical work on linguistic ideologies can help us to understand this sort of episode, and perhaps to work better and more comprehendingly with community members who support documentation of their heritage language in dictionaries and development projects like language classes. Susan Gal and Judith Irvine (1995) showed that language ideologies nearly always invoke three major semiotic principles. These are “iconization,” “recursiveness,” and “erasure.” In “iconization,” elements of language are shaped to match elements “in the world” – and by erasure, any dimension of language that does not conform is ignored. By “recursiveness,” “iconization” operates throughout the system, bringing elements at every level into line. Michael Silverstein (1996, 2003) has pointed out the operation of what he calls the “dialectic of indexicality,” by which indexicality is reshaped as reference. Miyako Inoue (2004) has shown how certain kinds of social circumstances – episodes of rapid political economic change, in which identities are being rapidly restructured – heighten the rapidity and strength of these processes.

Using these theoretical tools, we can say something about the Hopi case, in which a language and an associated way of life that had always been taken for granted becomes the object of the most acute attention and reflection. Such attention and reflection, and the iconization principle, yields an exaggerated purism. In the Hopi case, by iconization the Hopi community
itself is assimilated to the prototype of the kiva, and the language is assimilated to the language of the kiva. The words of the language become like kiva objects, which should never be seen by non-initiates. Just as ritual practice and ritual talk that occurs in the kiva is never shared with outsiders, the language should not be shared with outsiders. Just as the kiva and even public ritual is a site where nothing is bought and sold, and everything is generously shared, no price can be put on the language, so it cannot appear in artifacts that bear a price. In this case we can see the dialect of indexicality: the language, which indexes Hopi identity, must be shaped so that it refers perfectly to that identity: it must be ritually normalized, just as the identity itself becomes the identity of a ritual participant. Thus a Hopi word in an $80.00 dictionary published by a White institution, truly makes no sense; it is, in the words of the anthropologist Mary Douglas (1966), “matter out of place,” a form of pollution, and incites profound reactions in those who are offended.

Anyone who works in indigenous North America, where communities are only a few generations removed from a true genocide and continue to confront severe economic marginalization as well as racism, will be able to recount many examples like the case of the Hopi dictionary. The logic of language ideology outlined above predicts that documentary linguists will encounter similar episodes in communities that thus far have been reasonably receptive to documentary projects. The theory also predicts the general shape that such ideological projects are likely to take: they will assimilate the resources of language to some image of purity and essence, ritually validated, and will attempt to remove the language forever from history. Needless to say, such ideological projects happen everywhere. However, the community of speakers of Norwegian, or French, or German is robust enough to support the occasional outburst of purism without catastrophic results. Indeed, purism can be a positive asset if the community has the resources to do something about it; the examples of Israeli Hebrew and Catalan come to mind. But small minority-language and indigenous communities may not have such resources, and the state of the language may not give such communities time to work through such episodes and achieve positive and durable syntheses. So research specifically on such episodes, and how to handle and understand them, should be a part of our work. Leanne Hinton’s work on vernacular orthographies (Hinton 2003), a focus of ideological construction that has stymied language development in some American Indian communities for decades, seems to me a perfect example of the combination of theoretical penetration and practical recommendation that we require.
4. Conclusion

Training in documentary linguistics is very demanding, requiring as it does expertise in linguistics, in anthropology, in recording technologies and data management, and in a myriad other ancillary sub-fields. What I hope to have made clear, though, is that its anthropological component needs to include training not only in the foundations of ethnographic practice – in “making strange,” and in learning to notice and manage sites of miscommunication – but also in such arcana as the emergent formation of norms within a community of practice, and in the semiotics of ideology formation. The problem for us is to make these insights as straightforward for our students as is their training in phonology, morphology, and syntax. I hope that we will succeed in doing this. Just as recent advances in linguistic typology have immensely facilitated the recognition of the linguistic structures that we encounter in field work, advances in the study of cultural processes can help us organize our work and function more successfully, both as linguists and as friends, colleagues, and advocates for minority-language communities.

Note

1. Boas’ (1911a) great programmatic statement in the “Introduction” to the Handbook of North American Indian Languages was followed by scattered work by Boas, Sapir, Whorf, and a few others on cultural dimensions of language use. But this work is barely integrated with their extensive work on the description and documentation of grammar. In the 1960s Dell Hymes, John Gumperz, and their colleagues tried to reopen the Boasian project, proposing what Hymes called an “ethnography of speaking,” a “sociolinguistics” that took grammar and phonology to be simply one dimension of a pragmatics, one way that speakers actually use the material stuff of language. The diverse lines of work that Hymes enumerated as the foundations of a unified discipline exist today in over a dozen fragmented subspecialties with only occasional communication between them. Furthermore, very few people who emerged from the ethnography of speaking tradition, even those who have worked on indigenous and other minority linguistic communities, have made substantial contributions to linguistic description and documentation. Although it is a bit early to tell, the European “pragmatics” movement exhibits the same kinds of tendencies toward subspecialization, and its adherents, as far as I can tell, do not seem to be much involved in documentation of language organization at levels other than that of rhetoric and discourse.