



J. N. Hall

Looking Back, Looking Ahead

Jane H. Hill

School of Anthropology, University of Arizona, Tucson, Arizona 85721-0030;
email: jhill@email.arizona.edu

Annu. Rev. Anthropol. 2014. 43:1–14

The *Annual Review of Anthropology* is online at
anthro.annualreviews.org

This article's doi:
[10.1146/annurev-anthro-102313-025834](https://doi.org/10.1146/annurev-anthro-102313-025834)

Copyright © 2014 by Annual Reviews.
All rights reserved

Keywords

indigenous knowledge, linguistic anthropology, Uto-Aztecan, structure, agency

Abstract

Much of my career has involved the reshaping and transformation of knowledge of language and history shared with me by indigenous people who had made it a life's project to acquire, remember, and pass on this heritage. A consideration of the meaning of these encounters is interwoven with an autobiography of my scholarly formation and research career, the role of generationally shaped good luck in this career, and what it taught me about how speakers in diverse circumstances exploit the structural resources of their languages to protect and advance their interests.

REFLECTING ON DOROTHY RAMON'S KNOWLEDGE

Now that I am retired, people often ask me what I am doing. Alongside writing this essay, I am parsing sentences in an enormous collection of texts in the Serrano language by Dorothy Ramón & Eric Elliot (2000).

You can learn a lot from this book. It occurs to me that some reader might find these examples quaint; I recite them because they provoked me to reflect on what the world should mean to us. We live on an earth formed when Coyote smoked the first pipe with the first fire, and the smoke spread out and made the land. Fly drilled that fire, and you can still see flies rubbing their legs together, drilling. So creation was a collaboration, not the act of a singular agent, and made already worshipful by the tobacco and pipe. The creators brought people onto a bare new land, so some of them agreed to transform into deer, and pine trees, and medicinal herbs, and other useful shapes. The sacrifice was not an easy one: You can hear the tree people crying when their wood cracks and hisses in the flames. There are special wood songs to honor their transformation, but perhaps no one knows them today.

I never met Dorothy Ramón, but I know that she was honored for her knowledge (the Dorothy Ramón Learning Center in Banning, California, bears her name). She did not share it lightly. Her voice in the texts often seems angry and sad about what is lost: songs, names, water, land, children starved in hideous residential schools, kin who pursued justice shot or hanged. She recounts the greed of the ranchers who ran their cattle on Indian lands even after these spaces were designated as reservations and of the business folk in Banning who piped all the water from the springs on the Morongo Reservation into town to flush the toilets and sprinkle the lawns of white people (when my husband Ken Hill was doing his fieldwork at Morongo in 1962 and 1963, you could still see the hundreds of acres of dead orchards that the springs had once irrigated).

I am working on these texts because I need a deeper understanding of the Serran languages (Serrano, Kitanemuk, and Vanyume, for which we have almost no trace), after decades of work on the closely related Cupan languages, mainly Cupeño (Hill & Nolasquez 1973, Hill 2005). The job is part of work on the history of Uto-Aztecan, the great family to which these languages belong. I wonder how it was that a few speakers of their ancestral languages were able to spread these into the complex social systems of pre-Uto-Aztecan Southern California, and I hope that a better understanding of the languages will help me understand how this spread took place. And the job is also part of work to reassert the place of close attention to linguistic structure in linguistic anthropology, since I have a sense that it has often been displaced as we have pursued ever more sophisticated cultural interpretation. These interpretations need to be regrounded in the material signals of language to see what these may constrain and reveal.

I love discovering these linguistic structures and the cultural content of these texts. I value knowledge immensely and feel like a better person when I have learned something new. Almost everything in the Ramón & Elliot texts is new for me, and I am very grateful for that collection. But of its 731 texts, I would guess that a good third of them end with expressions of loss and grief. So I want this essay in part to engage the paradox of my own empowerment through work on these languages through words that are saturated with loss. I feel inadequate to work this out in a general way, without vulgarizing and simplifying the human relationships, the feats of memory, and the products of scholarship that are part of the appropriations and transformations of knowledge in which I have participated, so I focus mainly on specific examples that I encountered in my research career. In concentrating on research, I must neglect the teaching that was often foundational for it (and the students who have given me so much pleasure and pride) and neglect as well what I learned in professional service (and the many colleagues who enriched that service).

LUCK

The circumstances of my professional formation were fortunate. Today the mass media in the United States, trying to manipulate opinion in the face of an economy that has devastated the lives of many people, are full of anecdotes about how my generation of Americans rode the post–World War II economic boom to prosperity. In promoting a story of intergenerational injustice, the media nearly always utterly neglect the fact that my generational good luck was, specifically, the good luck of white people with certain kinds of class and cultural capital, that was by no means universally shared (a lesson driven home in my retirement from my volunteer work with less fortunate white people who are in my generational cohort).

I was born in a very favorable year, 1939, when hardly anybody was having babies. And I was born on academic third base, into the guild, with all four grandparents former schoolteachers and both of my parents holding doctoral degrees at a time when this was very unusual. The model of my mother’s career as a botanist was a priceless resource. My parents enjoyed an income that mostly kept up with the Los Angeles real-estate market [I grew up within walking distance of the University of California, Los Angeles (UCLA), where my mother was on the faculty for many years]. When my father’s forays into forming new businesses ran into trouble, he easily found teaching at the UCLA Engineering College or salaried positions in research and development firms. The public schools on the west side of Los Angeles were very good, and college educations for me and my sister and two brothers were cheap relative to my family’s financial resources. The numbers involved are astonishing today. I do not remember the figures exactly, and I am not sure how to verify them. Rather than preface every number below with “I think” or “as I recall,” I just give them as I remember them. My two years at Reed College in 1956–1957 and 1957–1958 cost about \$3,000 per year (~\$500 a semester for tuition and fees and another ~\$1,000 for the year’s room and board, and maybe another \$500 for other expenses). This was a significant expense for my family but not a source of serious financial stress. When I transferred to the University of California, Berkeley, in the fall of 1958, my summer wages of ~\$2 per hour covered my fees of \$45 per semester with plenty left over. It was years before I heard about students taking out loans for college; friends I knew who attended without family support “worked their way through,” paying their way on incomes from part-time jobs. This was not easy, but it certainly was not impossible. Graduate fellowship opportunities were plentiful, and my National Science Foundation fellowship of \$2,500 annually was adequate for my needs. Although fees did increase during our graduate years, UCLA remained affordable; my recollection is that in the last year my husband was enrolled, in 1967, just before Ronald Reagan as the governor of California took the axe to the university system, his per-semester costs were ~\$400.

My first job search produced a story I have often shared with astonished students. At the meetings of the American Anthropological Association in 1967, I decided to “go on the job market.” Ken was teaching at the University of Michigan in Ann Arbor, so when the AAA job-market form asked for a geographical preference, I wrote down southeastern Michigan. And I got an interview with the learned and witty Len Moss from Wayne State University. The anthropologists at Wayne offered me summer teaching, followed by a couple of years of temporary positions, and eventually a tenure-track job. My new colleagues were relieved that I already held the doctorate; most of Wayne’s new hires at that period were still working on their degrees. But the pressure of the waves of baby boomer students on the universities were such that my little cohort of white ‘39ers (and ‘40ers and ‘41ers too) pretty much had its pick of academic jobs even if its applicants had not created much of a dossier.

In these early years, I was oblivious to my class position (in fact, growing up on the west side of Los Angeles, many of my friends came from families much wealthier than my own). Even race

was background noise as far as my own life was concerned. Since then, I have seen how tough it is for working-class students to make it through the financial and ideological obstacle course of American academia. But it did not take long for me to realize that I had, indeed, enjoyed generational good luck. As early as the mid-1970s, the academic job market that made it not ridiculous to write southeastern Michigan on my application form in 1967 began to collapse, and I knew of brilliant students from top programs who fell by the wayside, making real the jokes that had begun to circulate about people with PhDs driving taxicabs. And much worse fates than driving taxis were soon obvious, as desperate homeless men and women became a common sight in the hearts of the big cities where we attended meetings of the American Anthropological Association. I emphasize that for my generation this was something new. We knew about skid rows and hobo jungles, but the newly conspicuous presence of homeless people was profoundly shocking. I got tenure in 1974, and within two or three years I felt like a guilty survivor. Before then I knew I had to make an extra effort to succeed as a woman; after the late 1970s I felt like I owed the world hard work to deserve the opportunities that were denied to so many others, male and female.

Not only did jobs disappear. Fellowships dried up, and the teaching-assistant salaries that permitted my own grad-school friends to live decently, if modestly, in the early 1960s did not even begin to keep up with the inflation of the late 1970s and early 1980s. For most of my students, their assistantships have functioned as supplemental fractions in complicated portfolios of small scholarships, multiple loans, and income from temporary jobs that usually did not advance their studies. My degree took six years, but the 8-, 10-, 12-year degree began to seem quite ordinary as students marked time on those meager gleanings, hoping that the job market would open up a bit. By the 1990s, even state universities were becoming increasingly unaffordable. The summer jobs that paid my Berkeley fees and rent would today, even with salaries adjusted for inflation (and of course that kind of work-study-level salary has not even begun to adjust for inflation), cover only a small fraction of the in-state tuition and fees at that institution. My own recent students struggle with debt and face years of temporary positions (and they are very glad to get those) and the daunting possibility that a tenure-track job, let alone one in a productive doctoral program such as Wayne State's, may never materialize. To pursue an academic career is more and more a choice to make sacrifices. I remember joking with my colleagues that we would never get rich, but we never joked that we were going to be poor. Families and friends thought we had good prospects; nobody ever told me I was foolish to pursue graduate work in anthropology. What all this leads up to is not only that this situation needs to change, but also that members of my senior generation of anthropologists may be very different people from their own students and are perhaps barely qualified to advise them.

TRAINING

During my scholarly formation I especially remember my two undergraduate years at Berkeley. The faculty were simply extraordinary; I had courses with Theodore McQuown, Sherwood Washburn (the teaching assistant was Irven DeVore, just back from Kenya and Tanzania with the first cuts of his baboon films), Robert Heizer, and John Rowe, and among the more junior luminaries Richard Salisbury (from whom I acquired a lifelong interest in New Guinea anthropology, which I have never been able to do anything about), Robert Murphy, and the newly fledged assistant professor Clifford Geertz for a semester on Malaysia and Indonesia (he would get so excited while lecturing that he would leave his cigarette balanced on the chalk tray and try to inhale the chalk; those were the days when you could smoke in the classroom—and of course there was still chalk).

The physical facilities at Berkeley were, at first, terrible; we were in a temporary two-story building accessed by rickety wooden stairs. I remember watching an octogenarian A. L. Kroeber

being helped up those stairs on his crutches. If all of us in the building's single large classroom had arranged to sway back and forth in synchrony, we probably could have flipped the building on its side. In my senior year, 1959–1960, we moved into the new Kroeber Hall with its beautiful research facilities, well-equipped classrooms, and faculty offices that had not had time to acquire much clutter.

The unembarrassed sexism of the period was part of what made me a linguistic anthropologist instead of an archaeologist. When I went to see Professor John Rowe about attending the field school, he informed me matter-of-factly that women were not admitted (he joked that they would have had to dig two sets of latrines). When I shared this experience with my realistic botanist mother (who had herself been denied graduate education in mathematics, her first choice of profession), she opined that the only way for a woman to have a career in archaeology was to marry a male archaeologist. Since by this time my relationship with my then-boyfriend had begun to go sour, such a marriage did not seem like an immediate prospect. My interest in linguistics, and a feeling of competence I had enjoyed in classes in that field, pushed me in that direction. So in 1960, I applied, very late (in June, as I recall), and was admitted to a new MA program in Linguistics at UCLA and was permitted to shift my National Science Foundation fellowship to the new program.

My undergraduate training had all been within the framework of American structuralism, studying with specialists in the indigenous languages of the Americas. At UCLA William Bright and Harry Hoijer were anthropological linguists working within that tradition. But other UCLA faculty were early adopters of the new “transformational-generative” direction in linguistics, which had no links to anthropology. I do not think that the new approaches were at all well taught at that time; syntax, for instance, was a journal-club-like seminar, when we needed fundamental foundations. Both I and new boyfriend and soon husband Ken Hill, who had come out of a very strong undergraduate background in structuralist linguistics at Georgetown, found the new models confusing, the advocacy for them heavy-handed, and the intense focus on the structures of English irrelevant to our dissertation research on languages of indigenous California. I did write my dissertation on Cupeño within the general model of Chomsky's *Aspects of the Theory of Syntax* (1967), to which I had access in the form of a purple “ditto” copy, but I felt that my understanding of it was superficial. When Robert Stockwell asked me, in my dissertation defense, to come up with examples of structural ambiguity in Cupeño (he had in mind the “Flying planes can be dangerous” type), I was utterly stymied; Cupeño syntax is almost all inside the verb, and the nominalizations for the event of making something fly, versus characterizing an object as capable of flight, are utterly distinct.

The experience of living through what many people saw as a genuine “scientific revolution” (Kuhn 1962) during the UCLA years did not make me an accomplished grammarian within the generativist framework. It did, however, provide some important lessons in academic politics. Many senior people felt that the worth of their entire professional lives had been dismissed, and were deeply bitter, and some young up-and-comers were notably unfeeling in deploying against them the aggressive new styles of argumentation, combined with a dismissive attitude toward the concerns of elders and outsiders, that characterized many generativist linguists of that era.

One of the most threatening claims of the new theoretical movement was that scholars could not conduct meaningful work on languages that they did not speak natively. This opinion dismissed almost all Americanist linguistic scholarship and must have been felt as a deep insult by my mentors. It made me anxious and uncomfortable because I wanted to do what they were doing. I did not feel this discomfort at the time as the embodiment of a challenge to my participation in structures of power over the transformation of knowledge, but that is what it was. I am not sure that the claim was intended as a political gesture (beyond the obvious academic politics), but it had profound

implications. In one direction, it produced efforts, led especially by Kenneth L. Hale, to bring into the linguistic profession members of what, sadly, was usually the very last generation of speakers who had acquired indigenous American languages in childhood. This project was taken especially seriously at the University of Arizona, where Hale had taught and where I eventually came to work. When I arrived there, the University of Arizona Department of Linguistics (in which I had a courtesy appointment; I was even acting head for two years in the 1980s) did not teach classes in “field methods” because the faculty members who would have done this considered such courses to be unacceptable forms of exploitation. Today University of Arizona linguists, both students and faculty, are engaged in long-term and productive collaborations with members of indigenous communities, continue to train students of indigenous heritage, and have created a methodological hotbed of new statistical and experimental techniques, but they still do not teach field methods as traditionally understood.

Chomsky is often accused of tolerating a disconnect between his supposedly apolitical theoretical work in linguistics and his political activism. But the proposal that only native speakers could have the deep insights into the workings of languages that can test foundational theories was a deeply radical move, challenging at its roots the presumption that Western scholars are competent to define the nature of the languages and histories of other traditions. I am not sure that this challenge is still felt by most nonindigenous linguists working today. The anxieties that I remember have been replaced by a near panic about language endangerment. The general tone of the linguistic discipline today implies that we need “all hands on deck.” The resources that are being shifted to documentation programs do provide some priority to the involvement of members of heritage communities. But advances in language typology have partially removed the veil of exoticism from linguistic structures that baffled the structuralists and the early generativists as well, increasing optimism about our capacity to understand these languages even when they are not our own. Although linguists I know universally acknowledge the need for work by native-speaker scholars, the radical intuition that some kinds of knowledge may be inaccessible, in principle, to nonnative speakers is no longer widely shared. I am also not sure that many nonindigenous linguists still feel in their guts the challenge to the validity of our knowledge that was briefly experienced by Americanists in the 1960s and 1970s.

WORKING

Fall 1964 found Ken teaching for a year at Berkeley. Spring 1965 was the Free Speech Movement, absorbing nearly all energy even for people such as ourselves who had nothing to do with it at all [although I did wheel the boys (Eric, then three years old, and Harold, eighteen months old) in their stroller down to campus to check out the doings and from a distance actually saw Mario Savio standing on the car in front of Sproul Hall]. Miraculously, that spring in Berkeley I managed to write my prelim papers (competent) and get back to UCLA for the orals (disastrous, but I squeaked through, probably because of Bill Bright’s skills as an advocate). That summer we moved to Ann Arbor, where Ken joined the Department of Linguistics at the University of Michigan. I spent a few years caring for the kids (Amy was born in July 1967), finishing my dissertation, and working on a couple of papers before joining the Wayne State Department of Anthropology in fall 1968.

Wayne State University was an astonishingly interesting place to work, with a richly diverse faculty and student population. Sadly, though, the decline of Detroit was under way even in 1968 when I first joined the faculty. The burned-over areas just west of the campus, site of some of the worst damage from the so-called Detroit Riots of summer 1967, were a devastating scar on the city. The enormous Dodge Main plant in Hamtramck just north of the campus and the Jefferson Avenue Chrysler plant on the east side were empty shells. The famous sign at the gate of the

parking lot of the downtown United Auto Workers headquarters, “Park your Toyota in Tokyo,” had already gone up. I remember when Carolyn Campbell (later head of African American Studies at the University of Michigan, Flint) had to cancel an appointment one day after a heavy snow because the city no longer plowed residential streets, and her house was several blocks from a main boulevard. More and more businesses up and down Woodward and Cass Avenues were boarded up; the big new Renaissance Center, on the riverfront behind moats of curving access roads and formidable concrete barricades, was a study in how not to revitalize a city. But the Tigers were doing pretty well during that period; they won the World Series in 1968, and I typed the Cupeño texts in Hill & Nolasquez (1973) during the summer of 1972, listening to the great Ernie Harwell call the games as the Tigers battled for their division title. Some of the many typographical errors in that work probably index tense moments in those contests. And from my office I could walk across Woodward Avenue to the Detroit Institute of Arts and hang out in front of Frederic Church’s *Cotopaxi* any time I wanted to. (There were other masterpieces, but this is the one I remember best. I may have a low taste, but I can see why people in the nineteenth century would pay a dollar to see a new painting by Church, rather like going to the movies today.) I would never in my bleakest nightmares have envisioned a world in which serious people would suggest that the collections of the Institute be auctioned to the highest bidder to make a small dent in the city’s debts. But this last year that world has arrived, and I can barely imagine the pain and frustration that must be felt by my colleagues in Detroit.

In 1971 Dale Valory, archivist at the Robert H. Lowie Museum of Anthropology in Berkeley (now the Phoebe Apperson Hearst Museum; I would like to have been a fly on the wall during the meetings that led to the name change), found a substantial body of material in Cupeño, the language of my dissertation, and let me know about it. The materials were collected by Paul-Louis Faye in 1919–1921, working as a graduate student under Kroeber. Unfortunately, by the time I located Faye (it was much harder to find people before the Internet), he had died after a long career as a professor of French at the University of Denver. So I was never able to discuss the materials with him. I got a small grant from the Phillips Foundation of the American Philosophical Society and went to Berkeley and photocopied (then a fairly new technology) as many of the materials as I could in the few days that I could be away from home. Among the several speakers Faye documented was Salvadora Valenzuela, mentioned often by Roscinda Nolasquez, my main teacher during my dissertation fieldwork (I had better mention that my two summers of funding came from the Survey of California Indian Languages). Miss Nolasquez was an expressive storyteller (I treasure her renditions of how Coyote looks back and forth, and how his ears twitch, and his funny toothless lisp when he sings), but her tales employed a narrow range of structures appropriate to the genre, intended for children. The occasional construction outside that range usually remained mysterious because I had only one or two examples and could not figure out how to elicit more [Roderick Jacobs (1975) did a much better job and made significant discoveries about the internal syntax of Cupeño verbs]. But “that Salvadora!” (Roscinda’s expression) and the other speakers Faye recorded had shared with him a diverse range of genres and a great diversity of complex constructions. These speakers had grown up before the forced exile, in 1903, of the Cupeño people from their homes at Kupa (Warner’s Hot Springs). Salvadora Valenzuela and most of her peers were still fully multilingual in indigenous languages (often more than one), English, and usually Spanish as well. Roscinda Nolasquez was only nine years old when she and her people were expelled from Kupa, and she had been subjected to several years of aggressively monolingual English education at the Sherman Institute. By the time I met her, in 1961, she was one of perhaps a dozen elders who spoke her language. Only three or four lived at Pala and she was not particularly friendly with any of these neighbors, so she had little opportunity to converse. Nearly all her opportunities to speak Cupeño, and to share the knowledge that she had

worked hard to acquire and remember, were found in teaching me, and later other linguists and classes with students of all ages from the community, until her advancing age made teaching too demanding.

Reflecting on insights about language function as a site for linguistic relativity (Hymes 1966), I thought it probable that the differences between the ways that Roscinda Nolasquez used the language in the early 1960s and the ways that Faye's consultants had used it in the early 1920s reflected the changing functions of the language within the speech community. I published a few papers on how function might select for form (Hill 1973, 1979, 1989) in moribund Cupeño. But I was frustrated by the very small sample of speakers (only three from my own fieldwork and a half-dozen from Faye's) and by the fact that my data consisted of a closed set of texts and elicitation results. Although I had some sense of the place of the language at Pala in the 1960s, I had not done any focused ethnographic work on this question, and of course I could only speculate about the place of the language in the early 1920s. I needed a new field site, with more speakers, to explore the relationship between language form and language function.

NAHUATL AND NEW IDEAS

During the 1970s Ken and I regularly attended the meetings of the Chicago Linguistic Society, an easy trip from Ann Arbor, where we could get a look at the bleeding edge of theoretical linguistics and hear the entertaining shouting matches between proponents of the various schools. At Chicago I met Jane Rosenthal, who described a situation in Mexico where she was working that seemed ideal for my project: a large number of communities, in different stages of language shift from Nahuatl to Spanish, scattered along the flanks of the Malinche Volcano in Tlaxcala and Puebla. Five hundred years of scholarship on Nahuatl meant that I would not have to worry about untangling grammatical and phonological fundamentals. Like Cupeño, it was a Uto-Aztecan language, so some of its structure and lexicon were already familiar. Ken and I found funding from the National Endowment for the Humanities for a year of fieldwork in the region in 1974–1975. Hugo Nutini, long-established in the region, met us in the field and generously spent several days introducing us to important dignitaries and warning us which towns should be avoided (shooting wars over land claims were going on in a couple of places) and which towns seemed the most likely sites. Because the kids needed to be in school, we lived that year in the city of Puebla and commuted out every day to the various communities, learning about the contexts that we eventually described in *Speaking Mexicano* (Hill & Hill 1986).

I do not recall that people were talking much about “agency” in the mid-1970s, and somehow I had never thought of Roscinda Nolasquez as an “agent.” Only years later did I gain some insights into what it might have meant to her to remember so much in a language that she hardly ever had an opportunity to speak and to share her memories through teaching about her mother tongue. But on the Malinche it was impossible to ignore how people were very actively manipulating linguistic resources in Mexicano (Nahuatl) and Spanish. Influenced by variation theory in sociolinguistics, I began to see the forms of language in multilingual contexts as a vast field of semiotic resources that could be selectively reshaped and resignified, in several dimensions of meaning and practice, through the work of speakers, work that I came to see as one element of a balancing act best understood in political-economic perspective. I have always been something of a lefty (although my level of commitment is low; in 1958 I missed going to jail for picketing the meetings of the House Un-American Activities Committee in San Francisco because the day all the arrests were made, I had a midterm). To understand the situation on the Malinche Volcano better, I began reading more intensively in Marx-influenced anthropology and linguistics (this shows up, for instance, in Hill 1985). Its title had attracted me to *Marxism and the Philosophy of Language* by

Vološinov [1973 (1930)]. One day, walking home from the Food and Drug Mart on the corner of Packard and Stadium in Ann Arbor with my Sunday *New York Times*, I met Aram Yengoyan. After chastising me for wasting my time with the *New York Times* (of course Aram was absolutely right about this), he asked me what I was up to, and I told him I was reading the Vološinov book. Aram took a few minutes standing on the sidewalk to bring me up to date on the possibility (which I now rule out) that the book had really been written by Vološinov's friend Mikhail Bakhtin, who had written other books that I might like. So I started reading Bakhtin and found especially in his great essay on discourse in the novel *The Dialogic Imagination* ([1935] 1980) and in *Problems of Dostoevsky's Poetics* ([1929] 1984) conceptual tools, outlined in Hill (1986), that led me to a new understanding of what Nahuatl bilinguals were doing on the Malinche Volcano. This work shaped a chapter in *Speaking Mexicano* and the essay "The Voices of Don Gabriel" (Hill 1995) and continues to frame my understanding of the shapes of language as profoundly embedded in social histories and as "filled with the voices of others" that sometimes become comfortably our own, but which must at other times become objects of struggle and resistance.

Not only did the Malinche communities teach us about the ways speakers invested linguistic forms with local meanings. The politics of knowledge appropriation and transformation also became too obvious for us to ignore. At the same time that the fluent bilingual talk we heard in the Malinche towns was challenged on every hand as backward, corrupt, and inauthentic, even a smattering of knowledge of the forms of Nahuatl documented from the sixteenth and seventeenth centuries, recorded in scholarly editions that were completely inaccessible to nearly all contemporary speakers, was a source of high prestige among mainstream Mexicans. In *Speaking Mexicano* we tried to represent the local forms of talk as the result of centuries of practical and positive work by speakers. When the book was translated into Spanish, by José Antonio Flores and Gerardo López Cruz (Hill & Hill 1999), Jacqueline Messing, then a University of Arizona graduate student, was in the field in Tlaxcala and organized for the new translation a *presentación del libro* (a book presentation, with speeches by scholars and dignitaries and drinks—in this case the excellent local *pulque*—and snacks for the public). José Antonio, Ken, and I attended and were moved to learn that some local people felt newly validated and empowered by the message of the book. Others, however, found the publication of Mexicano speech with Spanish loans included to be "wrong" and "disrespectful" (Messing & Rosales Flores 2013). Fortunately, today in Mexico, young scholars from indigenous communities, including many Nahuatl speakers, are increasingly prominent and will be able to work these questions out for themselves.

ARIZONA

In the early 1980s the economic situation in Michigan became increasingly dire. Both Ken and I had become department heads, and our jobs seemed to consist mainly of attending meetings about budget cuts and then going back to our departments and cutting our budgets, under attack both from colleagues who thought that we should have somehow defended them more effectively and from deans who thought we should be miraculously doing more with less. Faculty attrition and collapse of morale left the Department of Linguistics at the University of Michigan, where Ken taught, so vulnerable that in 1982 it was closed (it was reconstituted several years later with an entirely new faculty). Providentially, in 1983 I was offered a position in Anthropology at the University of Arizona. Ken decided to stay in Ann Arbor until Amy graduated from high school. Harold was studying at the School of Music at Michigan, so Eric and I moved to Tucson for the fall 1983 semester. Eric soon got very bored (as I had hoped) and enrolled at the University of Arizona, majoring in math and physics. In the fall of 1984, Ken took the satisfying step of (somewhat aggressively) submitting his resignation to his least favorite dean. He ended up with a

terrible severance package compared with that given to some of his colleagues, but he has always thought it was worth it. He joined me in Arizona in 1985.

In accepting the position at Arizona, I had made a commitment to work in the linguistic anthropology of the US Southwest. My first step was to sit in on a year's course with Ofelia Zepeda on her first language, Tohono O'odham, yet another Uto-Aztecan language. Ofelia and I were funded by the National Science Foundation in 1989 to work on O'odham dialectology. Assisted by Molly DuFort and Bernice Seraficio Belin, we collected huge amounts of material and produced some publications (e.g., Hill & Zepeda 1992, 1993, 1998a,b, 1999) and several conference papers, but a good deal remains to be done. Ofelia was becoming increasingly very well known as a poet, much in demand for readings, and as a major leader in the community-based revitalization of Native American languages, especially through the annual American Indian Language Development Institutes held in Tucson in June, which she has run, at first nearly single-handedly, for many years. (Ofelia's work won her a MacArthur "Genius" award in 1999.) So a monograph write-up of our project had to be put aside as of lower priority.

Although I continued to publish on Nahuatl, I could not conduct new fieldwork in Mexico for a decade. In 1990, Amy's shaky mental health finally focused into full-blown schizophrenia, complicated by alcoholism. After a long decline, Amy died in July 1997. Somehow, while trying in vain to help Amy manage her illness, Ken and I continued to do academic work. Fortunately, the work on O'odham dialects that Ofelia and I were conducting was local. But I never felt that I acquired much depth of ethnographic understanding from day trips to the reservation, in comparison with what I had gained from being actually in residence in San Miguel Canoa, the Malinche Volcano community where we had spent several multiweek summer visits as well as our fellowship year. And it was both instructive and intimidating to work with Ofelia, who knew all the nuances of how to behave and what was going on in Tohono O'odham country (my failed efforts at blending in inconspicuously are documented in Hill & Zepeda 1998b). I spent hours in O'odham households, attended many social occasions, and twice (with Bernice Seraficio Belin) taught literacy in the language in adult evening classes in Sells, the reservation capitol, yet this did not substitute for experiencing the rhythms of everyday life and the observation of everyday language use in a wide variety of contexts over many weeks of 24-7 presence in the community that I had been able to undertake in Mexico.

Perhaps because of feelings of inadequacy in my understanding of the O'odham community, and the impossibility of fieldwork in Mexico, I began to turn more and more toward reflection on "my own" people, motivated by developing a new course with the jaw-breaking title of "The Political Economy of Language in the Southwest." In this course, I had to address a classic finding in southwestern ethnolinguistics dating at least from the 1920s: the presence of an ethnically stratified system of multilingualism, with Native Americans often very multilingual, people of Spanish-language heritage usually bilingual, and so-called Anglos, people of non-Hispanic European descent, usually rigidly monolingual in English. As I began gathering materials for the class, I noticed an obvious complication of this simplified picture: uses of Spanish, and even fragments of Native American languages, to express a "Southwestern" identity among whites, which I eventually labeled "Mock Spanish." Working in the Malinche Volcano region, I had come to see in bilingualism the active manipulation of linguistic resources, as a form of strategic voicing in the Bakhtinian sense, and found in this understanding a useful approach to the bits and pieces of Spanish employed by largely monolingual whites in Arizona (and elsewhere—it turns out you can find bits of this everywhere in the English-speaking world and even beyond). I came to see this voicing as one of the tactics of white domination, which worked to turn Spanish into a useful resource for the management of white identities at the same time that explicit political efforts to deny use of the language to historically Spanish-speaking communities gained new strength. It

has been humbling to discover that my hard-won understandings are old stuff in many subaltern communities. I will never forget an African American woman who told me, after a talk at Wayne State, “I didn’t know that any white people *knew* any of that stuff!” So my scholarly efforts have brought me back simply to an approximation of understandings widely shared by those who suffer white racism.

My first paper on this work (Hill 1993) appeared in the journal *Critique of Anthropology*, where my colleague Dan Nugent was an editor. Dan insisted that I use the label “racism” in talking about how Mock Spanish works to reinforce negative stereotypes of Latinos at the same time that it elevates and authenticates white identities. Just as work on the Malinche pointed me in the direction of political economy, work on Mock Spanish, and Dan’s prodding, turned me toward literature in critical race studies and new work on racism within anthropology by such scholars as Audrey Smedley (1993), Faye Harrison (1995), and Enoch H. Page (Page & Thomas 1994). I also began to look more broadly at the many subtle ways that racism could be expressed in talk and text in the varieties of American English that I knew, as part of cultural systems that played out not only in face-to-face interactions, but also in discourse events that played out over weeks in the distributed spaces of national media. These events, media firestorms over so-called “gaffes,” racist utterances by important public figures, provided an opportunity for challenge and reinscription of ideologies. A fellowship at the Center for Advanced Study in the Behavioral Sciences gave me time to pull these ideas together in *The Everyday Language of White Racism* (Hill 2008b).

At the Center I also finished the grammar of Cupeño (Hill 2005), submitting the manuscript to the University of California Publications in Linguistics early in the fall, after spending August in the Bancroft Library checking details in the Paul Faye materials, which had by then been microfilmed and relocated to the Bancroft collections. I had gotten the long-delayed grammar under way in the Australian Spring (July–December) of 2000, when Alexandra Aikenvold and R. M. W. Dixon invited me to spend a term at the Center for Linguistic Typology at LaTrobe University in Bundoora, near Melbourne. In the Center’s extraordinarily productive atmosphere I had to present four two-hour seminars in as many months, so I emerged with four key chapters of the eventual twelve.

RETHINKING SPEAKER AGENCY

Returning to Cupeño, after reflecting on agency, power, and knowledge in the work on the Mexican of the Malinche and on Mock Spanish in the United States, I was moved to look again at what kinds of motives and tactics on the part of speakers might be retrieved from the Cupeño materials. Most of my work with Miss Nolasquez, after the first few days, had involved her telling me stories. Of course I hardly understood these at all the first time around, but Roscinda could see in the mechanical revolution of the reels of recording tape the eager listeners in her community’s future and ignored my clumsy efforts to act like a comprehending audience as she told her stories with fully performed special effects. She enjoyed telling stories, but she also told about personal experiences, the most important being her participation, as a small girl, in the forced removal in 1903 of her community from its ancestral lands at Kupa (Warner’s Hot Springs in English). Probably my most important discovery as I revisited my recordings was that she had quite consciously reshaped her own recollections, combined with what she had learned from elders of her youth, into history. In “history” (the Cupeño verb that labels it is *a’alxi*), every sentence that is not in reported speech contains a clitic construction, *ku’ut*, which indicates that the sentence carries the authority of a previous speaker. The animal stories that Roscinda told also use the clitic because technically they are also history, part of the creation accounts [she told them as “bedtime stories” (*silyichi*)], short snippets designed to entertain and teach children, which can be told at

any time of year]. The clitic is not found in first-person tales of experience. But, after starting her account of the removal without using it, Roscinda began her second day of telling by redefining her text with a new genre-designating verb, *a'alxi*:

Tuku'ep iviy ne'a'alxi ("Yesterday I told this history.")

Within a few sentences of this declaration, she began to incorporate the *ku'ut* clitic even in accounts of things that she herself had done. At the time, I missed the significance of this practice completely. Roscinda was definitively adding the account of the removal to the canon of authorized history (she was probably very conscious of the fact that she was one of the last people who would ever be able to do this), which included the great accounts of the creation itself and of the founding of Cupeño-speaking society. The significance of her text was not missed by members of the community; in 2004, before anyone had seen my new work, I learned that a group of bird singers led by Leroy Miranda had taken a line from her telling to make a new song. The bird songs tell of moments during the events of creation.

My new intuitions about what Roscinda was up to suggested the obvious point (one that was, as far as I can recall, never suggested to me as a student) that the indigenous people who shared their knowledge with me may have been involved in projects of inscription to which my own academic efforts were merely tangential. Kroskrity (2009) has recently begun deeper work in this direction, exploring the life and purposes of Rosalie Bethel, a speaker of Western Mono. My favorite telling by Rosalie Bethel is her "Coyote and Mole" (which appears on the CD compiled by Kroskrity et al. (2002). The gist of the story is that Mole wins a race with Coyote by having one mole duck into the underground mole-way at the starting line and another one pop up into a cheering crowd of mole fans at the finish line. The moles' secret was never explained when it is told to Mono audiences (a version of this type can be seen in Loether (1991), which ends with Coyote wondering, "Now how did he beat me?"). Kroskrity (2009) explains that children were supposed to ask their elders for help in answering Coyote's question. But for new audiences, who had no elders to turn to, Ms. Bethel added, in the voice of the triumphant lead mole, this coda:

Taaqwabo' nasimitU miyu simi'inisU sunawidi ("We moles all look alike.") (Kroskrity 2009, p. 203)

Some implications of Ms. Bethel's fondness for this story, with its universal trope of the homogenizing (and foolish) dominant gaze, should be obvious.

My most recent work, on historical Uto-Aztecan, has also depended heavily on the work of an indigenous scholar, Emory Sekaquaptewa of the Bureau of Applied Research in Anthropology at the University of Arizona. Emory had initiated a major dictionary of the Hopi language that would advance his vision of universal Hopi-language literacy. Emory, his student and editor Mary Black, and Ekkehart Malotki of Northern Arizona University, with my husband Ken serving as the project's lead editor, compiled an unmatched collection of lexical material on Hopi (Hopi Dict. Proj. 1998). When Jonathan Mabry, a University of Arizona PhD who had suffered through my core seminar, asked me to look into Uto-Aztecan maize vocabulary [Jonathan's excavations were uncovering unprecedented early dates for maize in the Tucson Basin (e.g., Mabry 1998)], I asked Ken to search the databases of the dictionary project for maize lexicon. We immediately found Hopi maize vocabulary that had not been previously available to scholars. Some of this material turned out to be linked to maize vocabulary in other Uto-Aztecan languages, probably by common descent rather than by language contact. This finding pushed me to advance a controversial argument, that the Proto-Uto-Aztecan speech community had included cultivators located on the edges of Mesoamerica and that the Uto-Aztecan corridor represented a northward expansion of

this community (Hill 2001). Although I am convinced that the spread of Uto-Aztecan, especially into southern California and probably onto the Colorado Plateau as well (Hill 2008a), included episodes of language shift as well as the movement of people, I have continued to work on Uto-Aztecan linguistic history within this general framework of a southern location for the proto-language (Hill 2012). My current work on California languages continues this project, with the goal of understanding how these (and their non-Uto-Aztecan neighbor tongues) functioned anciently as a shared pool of resources for the political-economic projects of speakers. This project is often threatening to local ideas about history in those communities. However, I have met people from these areas who find these hypotheses about Uto-Aztecan origins to be exciting; I hope that this work will be understood as an effort to open up rich new possibilities and not as a betrayal of those who have shared knowledge with me.

I need to finish this essay. I want to get back to the last 200 pages of Dorothy Ramón's texts. Native American stories often have very nice stock phrases for endings, but Roscinda Nolasquez would just say "That's all" in English. Ken, who is not very reverent about these things, suggested the Bugs Bunny closing line, "That's all, folks." Somehow, though, a slightly more dignified rhetorical structure seems appropriate. Looking to the future, my goal is to move toward a more fruitful integration of interpretations of linguistic structure with interpretations of the culture and ideology of language and the ways these are embedded in political economies, using the Uto-Aztecan languages of California as a site for that effort. This is going to be hard, and I may not get very far. But I hope that whatever transformations of knowledge I may achieve will honor what has been left to me by the elders who spoke these languages, and by the anthropologists and linguists who came before me in documenting their knowledge, and be useful to those who will follow.

DISCLOSURE STATEMENT

The author is not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

LITERATURE CITED

- Bakhtin M. 1984 (1929). *Problems of Dostoevsky's Poetics*, transl. C Emerson. Minneapolis: Univ. Minn. Press
- Bakhtin MM. 1980 (1935). *The Dialogic Imagination*, transl. M Holquist. Austin: Univ. Tex. Press
- Chomsky N. 1967. *Aspects of the Theory of Syntax*. Cambridge, MA: MIT Press
- Harrison FV. 1995. The persistent power of "race" in the cultural and political economy of racism. *Annu. Rev. Anthropol.* 24:47–74
- Hill JH. 1973. Subordinate clauses and language function. In *You Take the High Node and I'll Take the Low Node*, ed. CW Corum, TC Smith-Stark, A Weiser, pp. 33–52. Chicago, IL: Chicago Linguist. Soc.
- Hill JH. 1979. Language death, language contact, and language evolution. In *Approaches to Language*, ed. WC McCormack, SA Wurm, pp. 45–78. The Hague: Mouton
- Hill JH. 1985. The grammar of consciousness and the consciousness of grammar. *Am. Ethnol.* 12:725–37
- Hill JH. 1986. The refiguration of the anthropology of language. *Cult. Anthropol.* 1(1):89–102
- Hill JH. 1989. Relativization in obsolescent and non-obsolescent languages. In *Investigating Obsolescence*, ed. N Dorian, pp. 149–64. Cambridge, UK: Cambridge Univ. Press
- Hill JH. 1993. Hasta la vista, baby: Anglo Spanish in the American Southwest. *Crit. Anthropol.* 13:145–76
- Hill JH. 1995. The voices of Don Gabriel: responsibility and self in a modern Mexicano narrative. In *The Dialogic Emergence of Culture*, ed. D Tedlock, B Mannheim, pp. 97–147. Urbana: Univ. Ill. Press
- Hill JH. 2001. Proto-Uto-Aztecan: a community of cultivators in central Mexico? *Am. Anthropol.* 103(4):913–34
- Hill JH. 2005. *A Grammar of Cupeño*. Univ. Calif. Publ. Linguist. Vol. 136. Berkeley: Univ. Calif. Press

- Hill JH. 2008a. Northern Uto-Aztecan and Kiowa-Tanoan: evidence of contact between the proto-languages? *Int. J. Am. Linguist.* 74:155–88
- Hill JH. 2008b. *The Everyday Language of White Racism*. Malden, MA/Oxford, UK: Wiley-Blackwell
- Hill JH. 2012. Proto-Uto-Aztecan as a Mesoamerican language. *Anc. Mesoam.* 23:57–68
- Hill JH, Hill KC. 1986. *Speaking Mexicano: The Dynamics of Syncretic Language in Central Mexico*. Tucson: Univ. Ariz. Press
- Hill JH, Hill KC. 1999. *Hablando Mexicano*, transl. JA Flores Farfán, G López Cruz. México, DF: CIESAS/INI
- Hill JH, Nolasquez R. 1973. *Mulu'wetam: The First People*. Banning, CA: Malki Mus. Press
- Hill JH, Zepeda O. 1992. Derived words in Tohono O'odham. *Int. J. Am. Linguist.* 59:355–404
- Hill JH, Zepeda O. 1993. Mrs. Patricio's trouble: the distribution of responsibility in an account of personal experience. In *Responsibility and Evidence in Oral Discourse*, ed. JH Hill, JT Irvine, pp. 197–225. Cambridge, UK: Cambridge Univ. Press
- Hill JH, Zepeda O. 1998a. Tohono O'odham (Papago) plurals. *Anthropol. Linguist.* 40:1–42
- Hill JH, Zepeda O. 1998b. Collaborative sociolinguistic research among the Tohono O'odham. In Special Issue: *Native American Oral Traditions: Collaboration and Interpretation*, ed. L Evers, B Toelken. *Oral Tradit.* 13(1):1130–56
- Hill JH, Zepeda O. 1999. Language, gender, and biology: pulmonic ingressive airstream in Tohono O'odham women's speech. *Southwest J. Linguist.* 18:15–40
- Hopi Dict. Proj. 1998. *Hopi Dictionary/Hopiikwa Lavàytutuveni: A Hopi-English Dictionary of the Third Mesa Dialect*, ed. KC Hill. Tucson: Univ. Ariz. Press
- Hymes DH. 1966. Two types of linguistic relativity (with examples from Amerindian ethnography). In *Sociolinguistics: Proceedings of the 1964 UCLA Sociolinguistics Conference*, ed. W Bright, pp. 114–57. The Hague: Mouton
- Jacobs RA. 1975. *Syntactic Change: A Cupan (Uto-Aztecan) Case Study*. Univ. Calif. Publ. Linguist. 79. Berkeley: Univ. Calif. Press
- Kroskrity PV. 2009. Embodying the reversal of language shift: agency, incorporation, and language ideological change in the Western Mono Community of Central California. In *Native American Language Ideologies: Beliefs, Practices, and Struggles in Indian Country*, ed. PV Kroskrity, MC Field, pp. 190–210. Tucson: Univ. Ariz. Press
- Kroskrity PV, Bethel R, Reynolds JF. 2002. *Taitadubaan: Western Mono Ways of Speaking* (CD-ROM). Norman: Univ. Okla. Press
- Kuhn TS. 1962. *The Structure of Scientific Revolutions*. Chicago, IL: Univ. Chicago Press
- Loether CP. 1991. *Verbal art among the Western Mono*. PhD Thesis, Univ. Calif., Los Angeles
- Mabry J. 1998. *Archaeological Investigations of Early Village Sites in the Middle Santa Cruz Valley*. Anthropol. Pap. No. 19. Tucson, AZ: Cent. Desert Archaeol.
- Messing J, Rosales Flores R. 2013. Syncretic speech, linguistic ideology, and intertextuality: (re)presenting the Spanish translation of “Speaking Mexicano” in Tlaxcala, Mexico. In *The Persistence of Language*, ed. ST Bischoff, D Cole, AV Fountain, M Miyashita, pp. 291–320. Amsterdam/Philadelphia, PA: John Benjamins
- Page H, Thomas RB. 1994. White public space and the construction of white privilege in U.S. health care: fresh concepts and a new model of analysis. *Med. Anthropol. Q.* 8:109–16
- Ramón D, Elliot E. 2000. *Wayta' Yawa': “Always Believe”*. Banning, CA: Malki Mus. Press
- Smedley A. 1993. *Race in North America: Origin and Evolution of a Worldview*. Boulder, CO: Westview
- Vološinov VN. 1973 (1930). *Marxism and the Philosophy of Language*, transl. P Lamplugh. New York: Seminar