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How I Got Here and Where I'm Going Next

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Abstract

My career falls into two distinct periods. The first two decades featured insecurity combined with the luck of wandering into situations that ultimately helped me become a better linguist and a better teacher. I had the insecurity mostly under control by the watershed year of 1988, when I published a favorably reviewed coauthored book on language contact and also became editor of *Language*. Language contact has occupied most of my research time since then, but my first encounter with Séliš-Ql'ispé (a.k.a. Montana Salish), in 1981, led to a 40-year dedication to finding out more about the language and its history.

FIFTY YEARS AGO

Fifty years ago I was thrilled with my personal life (husband, toddler, new baby, amiable dog, and a nice big faculty apartment in one of Yale's residential colleges), but my career prospects were bleak. In 1966, when many universities were adding linguistics jobs, my Yale professors had tapped the men in my graduate-school cohort one by one when jobs popped up on the old-boys' network—UCLA, Penn, Wisconsin, Toronto—leaving only me jobless. "How about me?" I asked. "Sorry, no one else has asked us for a recommendation, so we can't do anything for you." I wasn't especially resentful; I thought three of those men were better qualified than I was, and all four of them were more efficient at completing their degrees: them, 1967; me, 1968. But I noticed, even in those days before feminism burst onto the national scene, that nobody seemed to have considered the possibility of recommending me before any of the men. The old-boys' network did eventually come through for me, however. I became an Instructor of Linguistics at the University of Pittsburgh (Pitt) in the fall of 1967, a tenure-track job that would have been an assistant professorship if I'd finished my dissertation before starting to teach.

Back then it wasn't expected that a newly hired linguist would have either publications or teaching experience—luckily for me, since I had neither. I got married in the middle of that academic year, and my husband was teaching logic in Yale's philosophy department; in the spring I moved back to New Haven, defended my dissertation, and settled in. By 1971 I had held a precarious position as a Lecturer in Yale's Slavic department for several years, earning less than a teaching assistant. I taught beginning Russian and beginning Serbo-Croatian for one year and, after that, graduate Slavic linguistics courses: Old Church Slavic and History of Russian. My colleagues viewed me as a dilettante, and my unfortunate tendency to assume that other people's views of me were accurate led me to see myself that way too: No interesting research ideas occurred to me, and I had no reason to think that any ideas would come to me in the future either. I enjoyed half of my teaching duties, though. I discovered that I had no talent as a language teacher, but teaching linguistics was fun, especially the challenge of convincing literature specialists that historical Slavic linguistics was worth learning about.

Then the Slavic department hired a senior Slavic linguist, Edward Stankiewicz, and the department chair told me he could no longer justify paying me to teach his, the chair's, regular courses. Bleak: no teaching job and no research ideas. I didn't expect things to improve, but that turned out to be the nadir of my career (if you don't count my PhD dissertation, which would have been a strong candidate if anyone had awarded a prize for Worst Linguistics Dissertation Ever).

BEFORE GRADUATE LINGUISTICS

But to go back to the beginning: I was born in late 1939 in Evanston, Illinois, on the northern outskirts of Chicago, and I grew up 20 miles farther north, in Highland Park. English was the only language taught at my elementary school, but in high school I discovered the joy of learning languages, taking three years each of Latin and French. In college (Stanford University, class of 1961) I took German, Russian, and Ancient Greek. But the animal kingdom was my enduring passion, so my first choice of a college major was biology. I had to give up on that plan: A toxic high-school chemistry teacher had left me with a powerful aversion to chemistry, which was required for biology majors. And fear of math (justified in my case) ruled out a geology major, my second choice. I settled for German, an undemanding major that left me plenty of time for elective courses in exciting topics like evolution, invertebrate paleontology, comparative vertebrate anatomy, and plant ecology.

One summer during my college years I glimpsed what seemed like the ideal career when my mother, an ichthyologist, was helping to organize the 1960 annual meeting of the American

Society of Ichthyologists and Herpetologists ("Ichs and Herps," they called themselves) at Chicago's Field Museum, where she was a long-time Associate in the Division of Fishes. They needed someone to work at the registration desk, and she drafted me. When I wasn't on duty I went to talks, some of which were fascinating, and I also attended the smoker. Back then professional conferences typically featured social events called smokers, gatherings where people mingled and ate and drank and, yes, smoked. At this particular conference the smoker was held in the Shedd Aquarium, which is next to the Field Museum. We attendees were allowed to prowl behind the scenes, peering into tanks from above and getting close-up looks at the inhabitants and learning about them from hovering aquarium staff members. I thought this must be the life, traveling to conferences in interesting places, hearing interesting talks, meeting interesting people from all over the world, and touring aquariums. I had no idea how to go about achieving such a career, but that conference planted the seed. (It did occur to me at the time that a German major wasn't likely to get me to conferences that held smokers in places as interesting as the aquarium. I graduated from college with the German major, though, excluded from the professional delights of ichs, herps, and other zoological territory by my chemistry phobia. But many years later, as a member of the Fachbeirat of the Max-Planck-Institut für evolutionäre Anthropologie in Leipzig, I was delighted when our site visit included a tour of Michael Tomasello's primatology lab.)

I enjoyed studying languages, so I eventually found my way to three (the only three) linguistics courses offered at Stanford at the time. One was history of English, taught by Herbert Dean Meritt; I no longer remember any details about the course, but I do remember that one spectacular lecture earned Mr. Meritt a standing ovation—a demonstration that was as unheard-of then as it is now. The second linguistics course, taught by a Mr. Meadows, consisted mainly of the professor's tales of his brilliant dog and rants about the university's offensive parking policies, punctuated occasionally by denunciations of his colleagues in Romance languages. We learned almost nothing about linguistics that term, and much too much about Mr. Meadows. The third linguistics course was taught by a prominent Slavic linguist, Cornelis Hendrik van Schooneveld, who had been a student of Roman Jakobson's and was developing some of Jakobson's ideas further in his own semantic framework. I remember two things about that course: First, it made linguistics exciting; and second, I failed to understand most of what Professor van Schooneveld told us, including Jakobson's famous "case cube" depiction of the Russian case system. (All these years later, I still don't understand Jakobson's case cube. My fault, not Professor van Schooneveld's.)

I had no plan for life after college, so I was relieved as well as flattered when my German professors nominated me for a Woodrow Wilson Fellowship, which at that time provided funding for the first year of a Fellow's graduate education. I'd enjoyed studying German literature, but I couldn't imagine spending a lifetime with it. In one class my term paper was an elaborate analysis of layers of tragedy in Friedrich Hebbel's nineteenth-century play *Maria Magdalena*, and I was quite proud of my ingenuity, but there was nothing real in the analysis: It was adequately supported by the text, and it was clever, but it was all artificial imagining, not anything the author had put into the text. Linguistics was different: You were dealing with real facts about real languages, and you could construct hypotheses and test them. So in my application for the Woodrow Wilson Fellowship, I wrote that I wanted to study linguistics in graduate school. (At that point, judging by their horrified reaction, I'm afraid my German professors regretted nominating me.)

I was bothered by the fact that, after four years of studying German, I could read it well but had essentially zero speaking ability. So instead of applying to graduate schools immediately after graduation, I spent the 1961–1962 academic year at the University of Freiburg in Germany, funded by a small inheritance from my grandfather. I took classes in Gothic, Swedish, Arabic, and Hindi, and I took a few trips—including a visit to Berlin three weeks after the Berlin Wall went up, when there were still black wreaths on neighboring West Berlin sidewalks where people had died trying to escape from East Berlin over the Wall—but mostly I spent my time learning to speak German. It wasn't as easy as I'd hoped, because nearly everyone I met spoke English fluently, and I didn't want to insist that they listen to my stumbling German when it was so much easier for all of us to speak English. Eventually, though, I found a group of people who weren't very fluent in English: the director and members of the Russian Chorus of the University of Freiburg. I'm an enthusiastic singer, but a very bad one; they let me join them because they were nice people and because I knew more Russian than most of them did. They put me in the front row at concerts so that audiences could see that I had the words right, and they tactfully suggested that I not sing very loudly.

By the end of the year my spoken German was good enough to fool all of the people some of the time and maybe some of the people all of the time into thinking I was a native speaker. I was also very sick. I'd been so worried about running out of money before I could book a trip home that I got into the habit of saving my limited financial resources by eating Eintopf every day in the university's Mensa. It cost about five cents, and it tasted pretty good. Eintopf (literally "one pot," a hearty one-dish meal) was always cooked for so many hours that it had little nutritional value, so after several months of it I collapsed. I was also almost 20 pounds overweight, which delayed diagnosis of the problem: I was suffering from malnutrition. Once diagnosed and treated, I recovered enough to travel home, satisfied that I had achieved my main goal for that year.

GRADUATE SCHOOL

Meanwhile, my Woodrow Wilson Fellowship had been reawarded. Being ignorant and with no one to advise me, and wishing to be within easy traveling distance of my sister and her family, I wrote to several East Coast universities more or less at random: "Do you have a linguistics department? If so, do you admit women?" (This was in 1962, so although both questions were dumb, the second one wasn't necessarily irrelevant.) I ended up at Yale. This was a good thing, because I realized early on that my main interest was historical linguistics, which at that time mostly meant Indo-European linguistics—it was essential training for any budding historical linguist, although by then first-rate historical work had been done on other language families too. Yale was an excellent place to study Indo-European linguistics in the early 1960s. Bernard Bloch taught the introductory historical linguistics course, and then there were comparative grammar of Latin and Greek (actually an introduction to Indo-European linguistics, taught by Warren Cowgill), two yearlong Sanskrit courses taught by Paul Tedesco and Stanley Insler, Old Irish (Cowgill), Lithuanian (Cowgill), and Old Church Slavic (Michael Samilov).

Historical linguistics wasn't the only focus of Yale's graduate program in those years, however. We were also taught (pregenerative) structural linguistics and field methods for phonology and morphosyntax, together with analytical techniques for processing the data. Conducting fieldwork seemed integral to being a linguist, and I wanted to learn how to do it outside a classroom.

But where to go? I'd have liked to go to Africa, but I was too timid to undertake a year there, with no prior background in any African language and no established contacts with Africans and/or Africanists. In the end I decided that Eastern Europe was about as far from familiar territory as I could venture. I knew some Russian, but in the Soviet Union of that era, by all accounts, I wouldn't be allowed to travel around the countryside gathering data; I'd have to live in a university dormitory in Moscow or Leningrad, and I'd be restricted to interviewing people living at or near the university. That wasn't the kind of fieldwork I wanted to learn, so I settled on Yugoslavia (at the time a politically unified country under Marshal Tito) and its major language, Serbo-Croatian.

In order to become a Slavist I studied Old Church Slavic and Serbo-Croatian, and I began consulting with my advisor-to-be, Alexander Schenker. I spent the summer of 1964 at the Linguistic Society of America's (LSA's) Linguistic Institute at Indiana University, where I took a course in

Balkan linguistics from Pavle Ivić, the premier Serbo-Croatian dialectologist. (I also took my first and only syntax class at that Institute—a memorable course on transformational grammar taught by Paul Postal—plus a course on Mongolian.)

That fall, Mr. Schenker arranged for me to spend a whole year (1965–1966) based in Novi Sad, Yugoslavia, working under Ivić's direction on a dialectology project. I was assigned a word-formation topic, specifically noun suffixation in the various Serbo-Croatian dialects. It wasn't until I had spent several months in Novi Sad that I realized that I was not going to have anything significant to say on the subject, beyond a catalog of forms and a few minor generalizations. Another student might have come up with ideas to pursue, hypotheses to test, something of interest; I did not. Not while I was in Yugoslavia, and not when I was back home trying to transform my stack of closely written notebooks into an acceptable dissertation.

I did, however, learn quite a bit about doing fieldwork. I spent the first few months in Novi Sad, reading everything I could find in the imposing Matica Srpska Library on relevant aspects of word formation in many Serbo-Croatian dialects. Then I set out to collect original data in the villages Ivić had recommended. I had devised a long questionnaire, with questions like "What do you call a man with a big nose if you don't like him?" (The answer to that one, depending on the dialect, was *nosonja* or *nosac* or some other form I've forgotten now.) The first thing I learned was that I couldn't question men in any of the villages. Men were happy to talk to me, but it was inappropriate for me, as a young woman, to ask them questions from a prepared questionnaire. They chatted amiably but ignored the questions. I found that many villagers over the age of 60 had never attended school, so it was easy to find elderly women who spoke only a local dialect—except that the questioning had to be done in the absence of men, because it was also inappropriate for women to talk much (at least to a foreigner) when men were present.

In the first village I visited, between Niš and nearby Macedonia, I learned that I was too scrawny to be at all useful for village life, so they fed me and fed me—six meals in one noteworthy day, accompanied by large quantities of new wine. By midafternoon I was too stuffed with food and too tipsy to see my questionnaire clearly, much less work through it with my generous hosts.

In the second village, a touristy place in Montenegro by the Skadarsko Jezero and within sight of Albania (across the lake), I learned that some residents were nervous about politics. I found a woman herding sheep on a hillside above the lake and persuaded her to talk to me, but she refused to say much. If a man had a big nose it didn't matter whether she liked him or not, she told me, because God gave him that nose and it wasn't for her to judge. My questions and my scribbling made her so uneasy (someone told me later that she no doubt assumed I was from the secret police) that she asked me to erase everything I'd written down during our meeting. I'm ashamed to say that my ethical sense wasn't yet fully developed: I did erase it all as she watched, but it wasn't a high-quality eraser, so the pencil marks were still faintly visible and I was able to recover all the data afterward.

In the third village, on the island Krk in Croatia, I found a wonderful consultant, an elderly woman who was a superb speaker of the local dialect. But her husband had been in the merchant marine in America and he was eager to tell me all about it, in English. So each morning I would leave the inn in the village, walk the mile or two to their house, and hide behind the low stone wall across the road until I saw the husband come out and head to town. Then I'd dash into the house to interview his wife.

And so it went: The fieldwork was both stressful and fun, in about equal measure. At the end of the year, I took my data home and began the uninspired and uninspiring process of analyzing it, hoping to coax a solid work of scholarship from it. I failed. And in fact, I spent almost an entire year procrastinating, alternately playing chess and watching sitcoms on the TV that my landlord had kindly provided in my little apartment.

In the spring of 1967 I met Richmond Thomason, my husband-to-be, a logician who had graduated from college in 1961, as I had, and who had completed his PhD in lightning-quick time and was already an Assistant Professor of philosophy at Yale. The contrast was demoralizing. That fall, when I went off to teach at Pitt, he called me from New Haven every night, and every night he asked how many pages I'd written that day. Rather than lie or admit my paralysis, and anxious to prevent him from discovering my incompetence, I finally started writing. I defended the dissertation in the spring of 1968. Some unkind person told me after the defense that Warren Cowgill, the most prominent faculty member present, had commented, during the private faculty deliberations, that he didn't think anyone had to go through three years of graduate school to write **that**. I had to agree. But they awarded me the PhD anyway. Probably they felt sorry for me.

RESEARCH CAREER

Then came the years at Yale as a Slavic linguist. By 1971 Rich had been promoted to Associate Professor with tenure. When my tenuous position at Yale vanished that year, we started job-hunting. Or rather, he put out the word that he wanted to move, and when philosophy departments showed an interest in hiring him, he'd mention that his wife also needed a job. Since said wife was several years beyond the PhD and still with no publications, our potential employers were understandably unenthusiastic. Possibly some of their lack of enthusiasm stemmed from the fact that the chair of Yale's Slavic department, who had also been my dissertation advisor and was therefore the most obvious person to ask for recommendation letters, was outraged that Rich was planning to leave his tenured position at Yale just because I had no job; he assured me that I could stay at Yale and use the library and maybe in five or ten years they'd find another course for me to teach. He was so outraged that—as his secretary, shuddering at her breach of confidentiality, eventually warned me—he was writing negative letters about me.

Pitt was the exception to our asymmetrical job search. Its philosophy department was eager to hire Rich, and its linguistics department was happy to welcome me back. We moved to Pittsburgh in 1972, and that's where my research career finally began. I had a few original ideas, and in the 1970s I published several papers on analogic changes in systems of noun declension in Serbo-Croatian and other Slavic languages (e.g., Thomason 1976a,b, 1977). But that was the end of the Slavic phase of my career, and of my exclusive focus on Indo-European languages.

Several things conspired to make me look elsewhere. One motivator was my impression that the long-term research prospects for Indo-European linguistics might be somewhat limited: So much of what was being written around 1970 seemed to me to involve very small insights into very small changes. I didn't find it exciting. This impression was profoundly mistaken, and I can't justify it; but it's true that most of the exciting developments in Indo-European linguistics since that period have been in syntax, and I was never going to be a syntactician. Other major advances in Indo-European have come from intensive studies of ancient languages that were still somewhat mysterious when I was a graduate student, among them Tocharian, Hittite and other Anatolian languages, and Continental Celtic. But in 1970 I didn't foresee those developments either, so I stopped being a specialist in Slavic linguistics. My slightly jaundiced view of the field no doubt owed something to my less than delightful experiences as a marginal member of Yale's Slavic department.

At this juncture I read "Empirical Foundations for a Theory of Language Change," the epochal 1968 article by Uriel Weinreich, William Labov, and Marvin Herzog (Weinreich et al. 1968). That article made me think for the first time about external causes of language change. It didn't motivate me to become a sociolinguist, but it did make me understand that social factors could be powerful causes of change. A few years later, I bought and read Dell Hymes's (1971) edited volume *Pidginization and Creolization of Languages*. That book was a revelation, and it changed my career path dramatically: I decided that to do historical linguistics right I had to know about pidgin and creole languages and, more generally, about contact-induced language change.

After I read the Hymes volume, my next goal was to get to Hawaii to attend the 1975 International Conference on Pidgins and Creoles so that I could learn more. To do that, I had to submit a paper. I read and thought and came up with the germ of an idea: Judging by the case studies I was reading, there seemed to be a sharp division between linguistic changes in contact situations in which there is no imperfect learning (because the people responsible for the changes are fluent bilinguals) and linguistic changes in contact situations that involve imperfect learning during language shift—that is, learners' errors made by a group of shifting speakers in the language they're shifting to, their target language. Specifically, in borrowing situations (without imperfect learning) nonbasic vocabulary is borrowed first, followed, if the intensity of contact increases, by structural features and perhaps some basic vocabulary as well. This generalization holds regardless of whether the people doing the borrowing are transferring features from their native language to their second language or vice versa; the crucial factor is lack of imperfect learning. By contrast, in shift situations that involve imperfect learning of their new language by the shifting speakers, structural interference—mainly in the phonology and the syntax—shows up first and most in the target language, though it is usually accompanied by some lexical transfer as well. This robust correlation between sociolinguistic context and linguistic outcome holds for the vast majority of two-language contact situations; the most common type of exception (and it's not particularly common) is that some cultures ban lexical borrowing and may therefore show structural interference without lexical transfer even when there has been no imperfect learning and no group shift.

I tried this idea out on my Pitt colleague Terrence (Terry) Kaufman, and it passed muster with him. He too had a serious interest in language contact (for instance, he contributed a short paper on Chinook Jargon to the Hymes volume; see Kaufman 1971), so we agreed to make the Hawaii conference paper a joint project (Thomason & Kaufman 1975). The following year our first coauthored publication, a short paper on contact-induced phonological changes (Thomason & Kaufman 1976), appeared in a proceedings volume from another conference.

Neither contact-induced language change nor pidgin/creole linguistics was a major research area before the 1970s, when interest in pidgin/creole studies, in particular, expanded rapidly. The 1971 Hymes volume emerged from a 1968 conference that was arguably the first highly visible modern exploration of pidgin/creole linguistics and sociolinguistics, and the 1975 Hawaii conference on pidgins and creoles was attended by a large number of enthusiastic and active scholars.

Scholarly interest in other language-contact phenomena grew slowly at first and then rapidly. For most of the twentieth century, language contact had a rather spotty record in historical linguistics. The most prominent early scholar who wrote extensively on the subject was Hugo Schuchardt (1842–1927), who has been called the "father of pidgin-creole studies" (DeCamp 1977, p. 9); but Schuchardt's anti-Neogrammarian stance helped to prevent his insights on language contact from entering the mainstream of historical linguistic research. Claims about contact-induced change due to imperfect learning during a group's shift to a second language—a process that Terry and I called shift-induced interference, in order to avoid the sociopolitical implications of the traditional term "substratum interference"—had acquired a bad reputation because of some fanciful late-nineteenth-century claims about long-vanished substrate languages somehow managing, centuries after their disappearance, to contribute features to the languages that had replaced them (e.g., Ascoli 1881). Of course there were occasional books and articles focusing on contact-induced change, but the work that set the stage for the explosion of research in the area, and that is still considered the foundation of modern language-contact investigations, was Uriel Weinreich's (1953) classic study *Languages in Contact*, a revised version of his PhD dissertation, originally published in 1953 and reprinted frequently since then.

Approaching the subject 20 years after the original publication of Weinreich's book, Terry and I had the benefit of a large body of newly available research on contact-induced language change, and we also took a deeper dive into pidgins and creoles than Weinreich had done. Using our 41-page Hawaii conference paper as an extended outline, we expanded it over the next decade into a 400-page book, *Language Contact, Creolization, and Genetic Linguistics* (Thomason & Kaufman 1988; the book was "in press" for three years because the original copyeditor first wrought havoc with our prose and then quit her job without correcting any of the astonishingly large number of mistakes she'd made). The starting point for the original paper, the finding that borrowing situations (in our narrow sense) and shift situations correlated robustly with strikingly different linguistic results, was not wholly original; our contribution consisted mainly of pulling together scattered suggestions in the literature into a more comprehensive model of contact-induced change. Nor were we the only ones to make such a contribution: In the same year our book appeared, the Germanist Frans van Coetsem (1988) published a similar analysis, specifically for contact-induced phonological changes.

As the title of Terry's and my book suggests, a major goal was to compare the linguistic outcomes of both ordinary and extraordinary contact situations with the standard framework of historical linguistics, according to which language history proceeds by descent with modification from a parent language as a result of gradually accumulating (and mostly internally motivated) changes. In particular, we wanted to determine whether the most extreme contact phenomena present a challenge to historical linguists' tried-and-true family tree model of genetic relationships. Our answer was yes, contra Greenberg (1953), Weinreich (1958), and others. We argued that to be genetically related to other languages, a language must be descended primarily from a single parent language. This formulation leaves room for considerable amounts of contact-induced change, and (with "descended primarily") it suggests that there can be a fuzzy boundary between "genetically related" and "not genetically related." That is, we also rejected the doomsday conclusion that "[i]f mixed languages could occur, then no language could be proved to be a descendant of an earlier stage of a single other language" (Darnell & Sherzer 1971, p. 26).

The vast majority of known languages can be shown to have descended primarily from single parent languages, and they therefore fit neatly (or at least fairly neatly) into family trees, even when they have undergone considerable amounts of contact-induced change; but pidgins, creoles, and bilingual mixed languages like Michif (French noun phrases, Cree verb phrases and sentential syntax), Media Lengua (Quichua grammar, 90% Spanish lexicon), and Mednyj Aleut (mostly Aleut lexicon and structure, but Russian finite verb inflection) have a dramatically different history, and they are not genetically related, in the historical linguist's sense of the term, to the languages spoken by their creators. Of course this does not mean that they have no historical connections to other languages. It's just that those connections result from developmental processes that differ from the kind of gradual developmental history that family trees represent. Some pidgins and probably most bilingual mixed languages are in fact deliberately created in order to serve particular social purposes—basic communication among groups that share no common language (pidgins), a means of keeping outsiders from learning too much of a group's language and culture (pidgins), or in-group languages to serve new ethnic or subethnic groups (bilingual mixed languages).

Terry and I collaborated on another major project too. In 1986 we organized a small workshop, the First Spring Workshop on Theory and Method in Linguistic Reconstruction, and invited seven historical linguists who worked on various language families (we wanted to avoid overloading the group with Indo-Europeanists). That turned out to be the beginning of a long-running series of biennial workshops, held first at Pitt and then at the University of Michigan; the most recent one, the Seventeenth Spring Workshop, was in 2018, with almost 20 invited speakers who talked about language families of North America, Mesoamerica, South America, Europe (including

Indo-European), Africa, the Caucasus, East Asia, and Australia. (Due to the COVID-19 pandemic, there was no workshop in 2020.) All of these informal workshops have featured lively discussions among historical linguists who enjoy debating theoretical and methodological issues.

Most of my publications since 1975 have explored various aspects of language contact—morphological diffusion, mechanisms of contact-induced change, case studies of pidgins with non-European lexicons, and case studies of bilingual mixed languages, among other things. But in the summer of 1980 something else caught and held my attention. The pivotal event arose from one of my language-contact interests, a desire to understand the dynamics of linguistic areas, Sprachbünde. One famous but (in 1980) understudied Sprachbund is the Pacific Northwest area, which contains three core families, Salishan (about 23 languages, depending on where one places the divide between language and dialect), Wakashan (about 6 languages), and Chimakuan (2 languages). The easternmost Salishan language is spoken on the Flathead Reservation of western Montana, one mountain range to the west of Condon, Montana, where Rich and I have spent every summer since 1976 (together with our daughters, in earlier years). In order to investigate the history of the Sprachbund, I figured I needed to understand at least one of the three core families well, and to do that I'd need to start with one language. The most convenient one was Séliš-Ql'ispé.[1] In 1981, Anthony Mattina, a University of Montana linguist I'd been in touch with, recommended me to the Séliš-Ql'ispé members of the (then) Flathead Culture Committee: They wanted a linguist to help them learn how to use their new IPA-based alphabet, and Tony wasn't available. I leapt at the opportunity, constructing spelling-and-pronunciation drills and working with elders and others at the Culture Center in St. Ignatius, Montana.

That was the beginning of a relationship of more than 40 years with what is now (after two name changes) the Séliš-Ql'ispé Culture Committee. In the early years I visited St. Ignatius only occasionally during the summer; starting in the early 1990s I began weekly trips to the reservation throughout each summer, sometimes staying over two days but usually making the whole trip in a single day, working with a group of elders for seven hours between the two 100-mile drives. I hoped to help the Culture Committee with its language revitalization goals while learning something (anything) about Séliš-Ql'ispé.

But my early efforts were clumsy. The elders and the Culture Committee members didn't make requests, so I had to guess what might be useful to them, and my first guesses were off the mark. I began by writing some grammar lessons, avoiding technical terminology in the hope of making them intelligible to nonlinguists. Those lessons sank like a stone: After I gave them to the Culture Committee, I never heard of them again. Eventually I began working on a dictionary, compiling words from my field notes and expanding the dictionary files greatly by reeliciting as many words as possible from the thousand-page nineteenth-century Jesuit dictionary of the language (Mengarini et al. 1877–1879). Thankfully, this project appealed to the elders and the Culture Committee. It appealed to me too, because I was able to use my philological skills—interpreting the Jesuits' orthography is far from simple—as well as my analytical skills. The Culture Committee

[1]The Séliš-Ql'ispé language has undergone several name changes in the fairly recent past. There are two Salishan tribes on the Flathead Reservation, the Séliš and the Ql'ispé. They speak the same language. In prereservation times these tribes probably spoke noticeably different dialects, since they lived in different homelands. Nowadays, as far as I have been able to discover, there are no significant dialect differences between the two. I've called the language Montana Salish in earlier publications. The other language names that have been used track with the changing names of the tribes' Culture Committee: First it was the Flathead Culture Committee, then the Salish-Pend d'Oreille Culture Committee, and now, with the two tribes' self-names, the Séliš-Ql'ispé Culture Committee. The Salishan language family got its name from the Séliš people, who encountered the Lewis & Clark expedition in 1805, sold them horses, and showed them how to find the pass over the mountains.

also seemed pleased to get the analyzed texts that I prepared as my understanding of the language improved.

I learned new lessons about how to do fieldwork. Unlike the people I interviewed for my dissertation project in Yugoslavia, the Séliš and Ql'ispé elders I've worked with have all been fully bilingual in English, and in fact, with one possible exception, dominant in English. Séliš-Ql'ispé is so gravely endangered that there were perhaps 60 or 70 native speakers when I first visited St. Ignatius, and now there are fewer than 20. I have almost always worked with a group of five to seven elders rather than in one-to-one sessions. In a group the elders jog each other's memory when someone has trouble retrieving a word or phrase, and I also get to hear table talk in Séliš-Ql'ispé. Except for John Peter Paul and Agnes Paul, who were, I believe, the last married couple for whom Séliš-Ql'ispé was the regular home language, most of the elders I've known reported that they spoke their native language only at the weekly sessions with me in the summer and, during the rest of the year, in elders' meetings at the Culture Center.

One similarity between fieldwork in Yugoslavia and in St. Ignatius is that it would not have been appropriate in either situation for me to try to speak the language I was investigating. In Yugoslavia, Standard Serbo-Croatian was the only dialect I knew, and it was the only dialect that a foreigner like me was expected to speak. If I had tried to imitate villagers' nonstandard dialect speech, it would probably have struck them as mockery, not solidarity. In St. Ignatius, because the elders had spoken their language only to each other for decades, it was in effect an in-group language, and an outsider's effort to speak it would have seemed presumptuous. When I had learned enough of the language to understand some of the jokes they made around the table, and laughed out loud at one joke, it made them uncomfortable. We'd better be careful what we say, they said. So I stopped doing that.

I also learned how helpful it is to work with a colleague in the field. At the beginning of this article I mentioned our toddler and our new baby. The baby, Jenny, grew up and went to the University of California, Santa Cruz, and wrote the following answer when she was asked, on the first day of an introductory linguistics class, why she was taking the course: "I'm taking this course because it will make my mother happy, and besides, I want to know what they've been talking about around the dinner table all these years." She majored in linguistics (and literature). The toddler, Lucy, became interested in linguistics, especially Séliš-Ql'ispé, years before she went off to college. When she was about 12 years old, she spent a whole summer typing up the entire nineteenth-century Jesuit dictionary, and she also began accompanying me on trips to the reservation. The elders always wanted to see their words spelled on the whiteboard, so while I wrote the words and discussed the proper spelling with the group, Lucy transcribed everything, including not only the data but elders' comments about the proceedings. Our collaboration continued until she shifted her main interest to Algonquian during her PhD studies in linguistics, but even after that we gave a few joint conference talks and she wrote a few Salish papers. She still knows more about the Jesuit dictionary than anyone in the world.

I have not been able to shake the feeling that I'll need to live for at least another 80 years in order to understand Séliš-Ql'ispé really well, and then maybe another hundred years will suffice to solve some of the historical mysteries of the Pacific Northwest Sprachbund that led me to the Salishan language family in the first place. I'm loving the journey, though. Séliš-Ql'ispé is the most wonderful language, especially for someone who, like me, loves consonants (vowels tend to leave me cold) and morphology: It has 38 consonant phonemes, including four resonant pharyngeals (whose morphophonemic behavior bears a striking resemblance to that of reconstructed Proto-Indo-European laryngeals), and it has morphology to die for. I've published a few low-profile papers on the language, with the Culture Committee's permission (e.g., Thomason & Thomason 2004, Flemming et al. 2008, Thomason 2016), and even a couple of papers about the Sprachbund

(Thomason 1983, 2015b). But I have not finished the dictionary, after all these years: I don't know how anyone ever finishes a dictionary. I have very large dictionary files full of data gathered over 40 summers; but summer is my only large block of time for working on Salish, and when I'm in Montana, I have had to choose between collecting more data and cleaning up the many, many errors and inconsistencies in my dictionary files. I have always chosen data collection over file repair. The pandemic closed down any chance of visits to St. Ignatius in the summer of 2020, however, and in recent years all but one of the elders I've worked with for so long have passed away. I can't express adequately how losing them has affected me personally; knowing them has enriched my life in ways I could never have predicted, and their absence is a great sorrow. From a professional perspective, my data-collection opportunities may be at an end. But if so, I can start my retirement by fixing those files, finally (I officially became an academic nonperson on June 1, 2021).

TEACHING AND EDITING

I have written quite a bit about my research career in this article, but little about my teaching career, and nothing at all about administrative and other professional activities. Teaching has been a constant and vital part of my life for more than 50 years. I love being in the classroom and interacting closely with students, especially in seminars and small classes. I enjoy performing in big (for linguistics) Introduction to Language classes of 200+ students, but I'm no superstar, and I'm not very good at energizing and engaging a large class. My own energy level has gradually decreased over the past decade or two, and I can no longer remember how on earth I ever managed to teach three courses every term. But that was the normal teaching load when I was an Assistant Professor at Pitt, so obviously I did manage it.

I've taught four quite different student populations. At Yale I had the strong impression that many undergraduates were challenging me, as in, "Show us why we should bother listening to you." (This happened only in the Russian and Serbo-Croatian language classes. The one undergraduate seminar I taught had a more hospitable climate, and so did the graduate Slavic linguistics courses.) At Pitt, a sizable number of undergraduates were the first in their families to go to college. Most of them were not children of privilege, and their attitude toward their studies and their professors, on average, was more appreciative and more dedicated to learning. University of Michigan undergraduates seem to include both extremes, along with every perspective in between.

The fourth group of students I've taught was wildly different from all the others, and in some ways the most satisfying. In the early 1990s, I received a letter from an inmate in Western Pen, the local nickname of the State Correctional Institution in Pittsburgh, asking if I'd be willing to teach a course he needed for his BS degree in psychology. I discovered that Pitt had a program at the prison through which inmates could earn bachelor's degrees in several disciplines; the instructors were all volunteers from the Pitt faculty. So I went to the prison for the required orientation, which mostly consisted of instructions about the panic button—an actual red button on the wall of each prison classroom. If there was a riot in class, I was told, I should show no fear but walk calmly to the button and press it. It didn't sound like a promising strategy, but in any case I received authorization to enter the prison once a week to teach Introduction to Linguistics. There were about 25 students in that first prison class, and they were the most enthusiastic and dedicated students I've ever taught. Getting their attention and respect was not a problem—they knew I had volunteered for the gig, and classes were by far the best part of their day. And many of them were talented students. I had enlisted a graduate student, Sasha Nikolic, as a coteacher, and he too was enthusiastic.

We did in fact have a near-riot that term. One evening, while teaching syntax, Sasha was explaining transitivity. One of the students offered the opinion that *find* could be used intransitively. Other students said loudly, "NO it can't!" "YES IT CAN!" the first student yelled: "Say we can't find the baby and we're looking for it, so we search, and we don't find in the house, but we find in the yard." Response from his fellow students: "You CAN'T SAY THAT!" By this time several of them were jumping up and down and shouting passionately about the immutable (or not) transitivity of *find*, and Sasha and I were eyeing that red panic button on the wall and envisioning the next day's newspaper headline, "Pitt Professors Injured at Western Pen in Melee Over Transitivity." It didn't come to that, but some months later I read that the intransitive-*find* proponent had been arrested for attempted murder when he dropped a heavy piece of cleaning equipment down a stairwell, aiming at the head of someone he didn't like.

I taught a few other courses in the prison, and enjoyed them all thoroughly. The last two classes had only a handful of students, though, because in 1994 the US government's policy changed to prevent Pell Grants from being awarded to prison inmates, and few of Western Pen's would-be college students could afford the tuition costs without those grants. The policy change was easy to understand—taking a privilege away from convicted murderers and other felons was a popular move—but the Pell Grants, in addition to their obvious benefits for the students themselves, benefited society, because education demonstrably reduces recidivism. The policy was finally reversed again in December 2020, when Pell Grant eligibility was restored to prison inmates as part of a federal stimulus package. This happened 21 years after I'd moved away from Pittsburgh, and too late for Western Pen inmates anyway: The prison was closed permanently in 2017, possibly as an indirect result of the fact that six inmates (not including any of my former students) had escaped by tunneling out of the place.

I never did find out why Pitt's director of the prison program (whom I didn't know) had urged the inmate to write to me in the first place, but in general I was the linguistics department's first choice of a target when unusual phone calls came in; the departmental staff knew that I found oddities intriguing. I got started on expert witnessing, for instance, when a local lawyer called trying to find a linguistics professor who could testify in court about the meaning of *cull*. After my appearance on the witness stand in that case, the word apparently spread, and several more cases landed on my desk, all of them offering fascinating glimpses of the legal system. They also offered me the opportunity to try my hand at designing and teaching a course on language and the law to a mixture of miscellaneous undergraduates, linguistics graduate students, and law students. Dipping a toe into the legal system never occupied much of my time, but another odd phone call did eventually lead to a continuing hobby: debunking linguistic pseudoscience.

It all started with a call from a local hypnotist. Most of his clients wanted his help in stopping smoking, but he got interested in efforts to age-regress hypnotic subjects to earlier lives and have them speak the languages they had spoken in one or more of those earlier lives. He called the linguistics department asking for a professor who could "verify" the languages his clients were speaking in their earlier lives. I couldn't verify them, of course, but I could and did test the claims for several of the purported past lives. All of the subjects failed the test, but the debunking method turned out to be interesting, so I wrote up the case for publication (Thomason 1984). From this beginning I became a fan of CSICOP, the Committee for the Scientific Investigation of Claims of the Paranormal, giving invited talks at two of their annual conferences and publishing two articles in their journal (Thomason 1987, an article that has been reprinted and translated more often than any of my serious research publications, and Thomason 1989). I've continued to collect examples of paranormal and other far-out linguistic claims, and recently Bill Poser and I published an article on the topic in this journal (Thomason & Poser 2020). We're hoping to use that article as a blueprint for the book on pseudolinguistics that we've been talking about writing for years.

Back to ordinary teaching. Too often, I've been unhappy with the available textbooks for introductory classes. At Yale I had that problem when I taught History of Russian, so I put together materials for a textbook on the subject; they worked well in my classes, but I never wrote them up formally. At Pitt, when I taught Introduction to Linguistics in the 1970s and 1980s, I didn't like the textbooks that were available then, so I began to write my own. It was to be a collaborative project, with my husband Rich and another colleague, Laura Knecht: They would write the syntax chapter, and I would write the other chapters. (As I have already suggested, syntax has never been my forte.) We even had a contract with Oxford University Press for the finished product. But there never was a finished product. My chapters were done; syntax was also done, but the floppy disk that chapter was saved on (ancient technology!) got lost, and the book project died. Too bad: We thought our chapters were well designed to teach students how to analyze linguistic data.

My favorite course at Pitt was Languages of the World, and I wrote several chapters of a textbook on the subject, *Language Classification and Language Structures*, focusing on the classification and typology of selected major language families—Indo-European, Uralic, Afro-Asiatic, and Dravidian—and one proposed family that remains highly controversial, Altaic. Then I stopped writing, because those chapters were enough to cover the 15 weeks of the course. I never got to the remaining chapters I'd planned (Austronesian, Sino-Tibetan, Niger-Congo, Algonquian), so that textbook project also remained unfinished.

I didn't manage to complete and publish a textbook until Rich and I moved to Michigan. By 1998 we had spent more than 25 years at Pitt, me in linguistics and him in both philosophy and linguistics. We enjoyed Pittsburgh for all those years and the university for many of them. But eventually the academic politics at Pitt went from difficult to unpleasant, both at the departmental levels and at the university level, and we put out the word to colleagues outside Pitt that we wanted to move. The University of Michigan's philosophy and linguistics departments approached us independently, and both departments offered us jobs. We moved at the end of 1998. My teaching repertoire changed at Michigan. No more Languages of the World; instead, I taught (actually team-taught, with a colleague each time) the big introductory class a few times, with limited success. I continued to teach historical linguistics once a year, with much better results, and I also fell into a pattern of regularly offering a senior undergraduate seminar for linguistics majors, a "capstone" course. Most of my colleagues apparently haven't been clamoring to teach these courses, so I haven't had to feel guilty about teaching them so often. I've rotated three topics—language contact, endangered languages, and Séliš-Ql'ispé (under the name Montana Salish)—and I ended up writing textbooks for the first two (Thomason 2001, 2015a). My textbook adventures are not accidental: My main talent as a teacher is explaining things clearly, and writing textbooks provides an extended opportunity to do that.

Administrative and other professional service activities have taken up an immense amount of my time over the years, but they needn't take up more than a few sentences of this retrospective. No one asked me to do any service activities at Yale. I was never even allowed, much less required, to attend a Slavic department faculty meeting, because the faculty meetings were held in Mory's, a private club that did not permit women on its premises. At Pitt I served on the usual sorts of committees until 1987, when I was nominated for the editorship of *Language*, the flagship (and until recently the only) journal of the LSA. The search committee selected me, and in 1988 I became the journal's fourth editor. One of my three predecessors in that position was Bernard Bloch, my revered graduate-school teacher, who edited the journal from 1940 until his death in 1965. With the transition from his successor to me, the LSA decided to change the editor's term of office from an indefinite duration to a seven-year appointment, and I decided I could handle the workload for that length of time.

I did manage it, barely, by giving up most of my research time (except for Séliš-Ql'ispé in the summer) and much of my teaching. Running the journal had been a one-man job throughout its history, and I didn't want to be seen as too feeble to carry on that tradition, so I worked an average of 40 hours a week on journal business. I had able assistants, from copyediting to the mechanics of managing incoming and outgoing mail—physical mail, because in those years, 1988–1994, journal submissions still traveled to and from authors and reviewers mostly by snailmail—but I was always the bottleneck in the operation. Choosing manuscript reviewers, nagging slow manuscript reviewers, making final decisions, classifying the review books that poured into the journal office, choosing book reviewers, reading and editing (often rather heavily) everything that appeared in the journal, checking the copyediting, dealing with lawsuit threats by disgruntled book authors—for excellent reasons, I was the last editor of *Language* to do all those tasks, but at least no one could say that the first woman to edit the journal wasn't able to handle the pressure and the workload. Unfortunately, I used up my entire lifetime supply of efficiency during those seven years, and I'm ashamed to say that ever since 1994 I've been late with almost every piece of writing I've promised to an editor. During my seven editing years, though, I published four journal issues on time each year, and I was proud of the relatively short average time (about three months) from the date of submission of a manuscript to the date of the decision on it.

WHAT'S NEXT

In Winter Term 2021 I taught my last course (a capstone seminar on endangered languages, via Zoom because of the pandemic) before retiring on June 1, so I'm now thinking ahead to projects that I suddenly have more time for. For the last 15 years or so I've been intrigued by deliberate language change—by the possibility that it is considerably more frequent than historical linguists have imagined. I'm skeptical of the traditional view that linguistic change is overwhelmingly unconscious, as expressed by (among other people) William Labov (1994):

> There is a part of language behavior that is subject to conscious control, to deliberate choice, to purposeful and reflective behavior. But as far as I can see, it is not a major part of the language faculty, and it has relatively little influence on the long-range development of language structure.

I've become more radical the older I get: I now believe that people can change anything in their language that they are aware of. All they need is a strong enough motive. I published one early paper on deliberate change (Thomason 2007), and I want to complete the paper on sound suppression that Ken Olson and I started working on some years ago (see Thomason & Olson 2016): In numerous contact situations around the world, and for various motives, people deliberately conceal particular speech sounds around uncongenial outsiders—especially sounds that are visible to the hearer, such as bilabial trills and the remarkable "voiced lateralized apical-alveolar/sublaminal-labial double flap with egressive lung air" of Pirahã (Everett 1982, p. 94). Then I want to explore further to see if I can find ways to assess the likelihood and frequency of deliberate changes. Another project is yet another textbook, to be coauthored with my colleague Marlyse Baptista, for a course that I designed years ago and that we've both taught since then: Language in a Multicultural World. Bill Poser and I still plan to write that book on debunking pseudolinguistics. And I want to know much more, and write more, about Séliš-Ql'ispé and its history. My most urgent goal, however, will be to repair my Séliš-Ql'ispé dictionary files at long last, and at long last (with the Culture Committee's permission) publish the dictionary.

Finally, a note about my nonlinguistic activities. I've never lost my fascination with the natural world, especially zoology. I've pursued that interest for many years in reading and in wildlife watching (bears by preference, but mostly birds). In 2019 I took a summer naturalist class and became the proud holder of a Montana Master Naturalist certificate. But my main expression of this interest is in the doodles, mainly of fanciful animals, that I've been drawing since my undergraduate days (see **Figure 1** for a few examples). These evolved from years of attempting realistic drawings of animals I found in exhibits at Chicago's Field Museum and Lincoln Park Zoo. The

Figure 1

Three of the author's doodles from different linguistics conferences.

doodling began in my afternoon Greek classes at Stanford: I loved studying Greek, but note-taking wasn't necessary, so I had a lot of trouble staying awake in class. I therefore developed a drawing style intricate enough to require exactly one hour, the length of the class period, to complete a single beast. Doodling worked so well for me that ever since then it's been my best way to stay awake, alert, and focused in committee meetings and in (other people's) conference presentations. It does help me concentrate. Doodling and research have always been in complementary distribution in my professional life, and I find the dialog satisfying. I've been lucky in my career and in my life, and I plan to keep doing both research and doodling for some time to come. If my luck holds, both will continue to evolve in new directions.

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