PART III

Biographies of Archaeologies of Listening
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I arrived on the Montana Blackfeet Reservation with a fresh B.A. from Barnard College as an anthropology major, a ticket for graduate work at Harvard, and three summers of archaeological fieldwork behind me. I was to be assistant curator, for three and a half months, at the Museum of the Plains Indian in Browning, the reservation agency town. The museum had been a Works Progress Administration (WPA) project, with a regular staff of two, the director and the maintenance man, who would be Blackfeet. What I found was an acting director, a young man named Tom Kehoe, and the maintenance man, Joe Schildt. The director, Claude E. Schaeffer, was on medical leave.

Tom had been hired by Schaeffer two years earlier to conduct an archaeological survey of the reservation. He had just completed course work for a master’s degree in anthropology at the University of Washington (Seattle), following an anthropology B.A. from Beloit College, and had worked on River Basin Surveys crews along the Missouri River, excavating Indian villages that would be flooded by dams. Like me, he had been admitted to Harvard for doctoral work, in his case pending the completion of his M.A. His M.A. thesis drew on his survey of the reservation, focusing on the question of the function of rings of stones commonly termed tipi rings (Figure 12.1). Dr. Schaeffer had suggested the topic and informally directed Tom’s research. The thesis was completed and submitted a few months after I had decided to remain on the reservation to marry Tom and participate in his research.

Tom’s thesis featured interviews with older Indian people on the reservation. They told him where there had been camps and explained that, before the extinction of the bison herds in the 1880s, stones had been used to hold down the bottoms of their tipis. When the tipi was lifted, the stones rolled off a little and formed a ring. Hearths were only sometimes built inside tipis, as people preferred to cook outside if weather permitted. All the older Indians, whose grandparents
had been the generation forced to settle on the reservation, concurred that, except for a few unusually large rings, the stone rings had secured tipis. The common designation “tipi rings” was correct. Thanks to Mary Ground, a Blackfoot born in 1883, we even ground-truthed this claim, when she pulled up her tipi after the reservation’s 1956 powwow and revealed the tipi ring it had left (Figure 12.2).

This might seem like a routine, straightforward archaeological interpretation for the millions of such rings once found all over the prairies, if not for professors who scorned Indians’ histories. William Mulloy (1952) had argued that “tipi ring” is a folk term, to be rejected in favor of the noncommittal phrase “manifestation of unknown relationship.” In support of his skepticism, he cited Lewis (1889), a report on Lewis’s explorations and recording of stone constructions on the plains. Lewis, too, had urged scientists to be wary of common labels for precontact phenomena. A member of the audience who heard his report politely told him that tipi rings are indeed tipi rings, stones that once weighted down hide tipis and were left as rings when the tipis were pulled up. Dr. Washington Matthews had served as a surgeon in Dakota Territory/the Great Sioux Reservation and per-
personally observed this many times (Lewis 1889:164–165). Lewis apologized and accepted Dr. Matthews’s empirical knowledge of tipis and camps. Mulloy apparently had not read the smaller-print “Discussion” following Lewis’s paper.

Thus, at the beginning of my professional research, I saw the gap between empirically based inductive scientific work and its opposite, academic arrogance privileging formal Western training over any other means of interpreting archaeological data (Figure 12.3).

Over the years, I have seen and pondered the contrast. Spending some weeks each summer in Blackfoot territory, visiting with friends and collaborators there, I comprehend something of Blackfoot reality, the world in which these people live. It is as if I take off the formal tailored jacket I wear in the city; I experience the prairie wind and sun and cold rain and what distances are on that landscape, how the mountains are alive and other species of people speak—ground squirrels and birds and horses and deer and plants. Archaeological data lie in this world. They are not lab specimens to be measured and tagged with academic labels.
Frank Speck and Bedside Ethnology

The history of archaeology (and of anthropology, which to me encompasses archaeology) reveals a persisting tension between rational theories and discomfiting data. Private interests and ambitions, often masked, are considerations not easily remarked. Social class was once a criterion for recognizing a scientist
A Lineage of Listening (Shapin 1994) and remains, along with “race,” a condition affecting opportunities for doing science. All these factors come into play in considering the work and teaching of Frank G. Speck, Claude Schaeffer’s professor (Figure 12.4).

Speck (1881–1950) was an outsider, literally and professionally. From boyhood he roamed the woods and marshes of New England and New York, learning from local people, Indians, and books the habits of fauna and flora. Speck was also a sociable person who made friends easily and enjoyed eating and traveling with companions. At the same time, he meticulously and faithfully documented every specimen he acquired, whether animal, plant, or handicraft, and contributed substantial collections to a number of museums, large and small (Medoff 1991). He earned his living as a professor of anthropology at the University of Pennsylvania, taking students with him on field trips; among his students were William Fenton, A. I. Hallowell, Anthony F. C. Wallace, Edmund Carpenter, and John Witthoft (Blankenship 1991; Darnell 2006). All of these anthropologists developed close relationships with Indian people, listening and expanding their universes with other-than-human beings (Figure 12.5).

The Speck family descended from Dutch settlers in the Hudson Valley, including, they said, some Mahican ancestry. At 18, Frank finished high school in Hackensack, New Jersey, and enrolled in Columbia University. Spending a few days at Fort Shantock, Connecticut, during his holidays, Speck fell in with three Mohegan youths who took him to their community, introducing him to older relatives who spoke Pequot/Mohegan (Bruchac 2018:140–175). During his junior year at Columbia, Speck took a course in comparative philology. His professor was interested in Algonkian. When Speck revealed that there was at least one community where Pequot/Mohegan was the daily language, the professor teamed with him in collating and analyzing it. Their work was nearly ready for publication when the professor’s home, with the study materials in his office, burned. All the manuscripts, including extensive journals in Pequot/Mohegan lent by matriarch Fidelia Fielding, were lost. The professor also introduced Speck to Franz Boas, with whom Speck began graduate work in anthropology, writing a dissertation on the Yuchi, whom he visited in their exile home in Oklahoma. Speck was awarded the Ph.D. by Pennsylvania, with Boas as his dissertation director. He then commenced his lifelong employment at Penn.

Speck’s devotion to the tiny marginalized remnant Indian communities east of or within the Appalachians made him an anomaly in American anthropology. His position on the faculty at Penn, and Boas’s approval, kept him respectable. His indefatigable and superbly documented collecting made him welcome
in museums, and his encyclopedic knowledge and rich fieldwork won him first-rank students, without fully negating the unease with which most anthropologists viewed his recognition of apparent “Negroes” and people who looked white as authentically of Indian descent and tradition. Speck ignored the color and class lines of American society. He recorded and published observational

Figure 12.4. Frank G. Speck (right) and Chief Jasper Blowsnake at Winnebago Camp, Elk River Reservation, Minnesota, 1936. Photo: University of Pennsylvania Museum UPM Neg. #148615. Jasper Blowsnake was the elder brother of Sam Blowsnake, narrator of *Crashing Thunder: The Autobiography of an American Indian* (1926).
data that supported Indian affiliations. That, like his natural history papers on amphibians drawn from field data, was solid science.

Fogelson (2016) quotes Anthony Wallace on Speck’s method, expounded to his students:

There are three kinds of ethnologists; the doorstep ethnologists, the kitchen table ethnologists, and the bedside ethnologists.
The doorstep ethnologist takes a room in town at a good hotel; he comes out to the reservation and interviews people on their doorstep. [Here he fixed an imaginary monocle in his eye and scowled] “Would you marry your sister?” Then he goes back and writes a book telling all about the Indians. Then there is the kitchen table ethnologist. He comes in the morning and stays all day. He sits at the kitchen table and takes notes and asks people questions. But he wouldn’t stay the night there! [Here Speck straightened up haughtily] It might be dirty upstairs! The bedside ethnologist is the only one who really gets to know the people and their culture. He stays all day; he eats with the family; he learns the language; and he sleeps in the same house. He never learns everything; but he learns a lot more than the doorstep ethnologist or the kitchen table ethnologist.

I want all of you to be bedside ethnologists.

The present book’s “archaeologies of listening” approximates Speck’s bedside ethnology. Niceties of terms aside, we who advocate listening are in the Frank Speck mode, never learning everything but learning a lot more than the archaeologist who only talks to community representatives or does not talk to anyone.

Whom Should We Listen To?

Frank Speck was notorious for fraternizing with communities marginalized by the United States and Canadian governments—neither recognized as Indian nor accepted as white. Their ways of speaking were judged to be uneducated dialects, their subsistence from woods, waters, and marshes to be primitive. Speck heard words and phrases that he recognized as Algonkian or another American Indian language stock and saw technologies and field knowledge that he could link to early historic descriptions of Indians. East of the Appalachians, Indian nations had been forced to adapt to European colonizations for three centuries before Speck’s fieldwork: Frank knew that some of Fidelia Fielding’s fellow Mohegans “looked Indian” as she did, while others, equally kin, appeared phenotypically white or black. A single family can exhibit the range, as parents’ genetics sort out. In the heyday of White Anglo-Saxon Protestant eugenicists for whom Franz Boas was the nemesis (Spiro 2009:317–319), Speck’s deep history for unrecognized tribes cast him as a radical.

With his broad range of friendships built up since youth, Speck apparently listened to hundreds of Algonkians, Iroquoians, Cherokee, and craftspeople...
who, by implication although not always stated, were of Indian descent. See, for example, his masterly study of *Eastern Algonkian Block-Stamp Decoration* (1947), in which he quotes a number of basketmakers and their immediate descendants, including the wonderfully named Miss Pocahontas Pharaoh, a Montauk descended from their hereditary chief Wyandank (Speck 1947:11). Using the scientific method, Speck recorded a range of variation rather than demanding the “true” or ur-text sought by most of his contemporaries; one of these, Clark Wissler, instructed his interpreter to find the “most common arrangement of incidents” in recording Blackfoot myths, until “a venerable old man pulled up a common ragweed, saying ‘The parts of this weed all branch off from the stem. They go different ways, but all come from the same root. So it is with the different versions of a myth”’ (Wissler and Duvall 1908:5).

The communities where we work may be municipalities, tribes, or bands—units in bureaucratic structures of modern nation-states, whatever the legal form. Governments require the units to elect or appoint (or accept) officers who will pass down decrees and speak for the community to the government agents; these may be selected by the actual most influential persons in the community to fulfill the government’s position. When I lived in an Aymara village in Bolivia, to learn how a project to restore Tiwanaku-period raised fields met villagers’ needs, I was told that the official *alcalde* (mayor) had no power. I observed that the real headman of the community was the appropriately named Don Plácido, who would speak only Aymara. It is normal for principal investigators or their field directors in an archaeological project to go to the local community, ask to speak with its officers, likely men whose names have been provided by the district governor, and formally inform them about the project. Casting a project in this colonial structure may be necessary. It is after this step, if required, that an archaeologist can hang out in the community, sit in kitchens listening to women, walk along when food or medicines are collected: brush past the ragweed, as it were, picking up its sticking seeds. One never gets the full range of variation; on the other hand, one can begin to develop a feel for the landscape, social relations, weather, and other-than-human beings including animals. Archaeological data begin to have dimensions outside conventional experience.

Boas, with his historical particularism (every society has its particular history), and Speck, with his naturalist orientation, opposed the fundamental Western Enlightenment supposition that science can discover universal regularities, “laws” to all intents and purposes. At its worst, the Western premise
of universal laws led to twentieth-century fascism, not only in Adolf Hitler’s Germany but also in the Rockefeller Foundation’s bankrolling of projects in “[t]he social sciences, [that] like all science, are primarily concerned for analysis, prediction, and control of behavior and values” (Barber 1952:259; emphasis added). Less ambitious but still pernicious was, and is, conventional Western science including archaeology that “has nomothetic, generalizing, interests . . . [a] responsibility to” render non-Western peoples’ experience and ideas “into the language of modern scientific and historiographic discourse” (Mason 2006:241). Such hegemony attempted by an invasive, conquering power over American Indians was consistently resisted by Fidelia Fielding. Her foster son carried on the struggle, teaching a cohort of outstanding scholars to eschew reductionism and racist hierarchies. Not incidentally, Speck recruited young American Indians to assist him in the field and to come to Penn for a university degree. They had little success there, given the impediments set before them at that time: only Gladys Tantaquidgeon, a Mohegan, is generally known today as an Indian historian trained by Speck.

Claude Schaeffer was reserved and scholarly where Speck was extroverted and an outdoorsman. Schaeffer taught Tom Kehoe how to proceed systematically through a research agenda, checking sources against sources—both oral and published—as one moves along. The published thesis (Kehoe 1960) is organized according to a Western rational schema: Historical Evidence, excerpting nineteenth-century explorers; Ethnological Considerations (Kehoe was advised to say “considerations” rather than “evidence”) from Blackfoot informants and informants of other tribes; Archaeological Considerations, including environmental factors, ethnological background, and archaeological fieldwork; and, finally, “Discussion, Conclusions, and Appendix: A Modern Blackfoot Camp” (the 1956 North American Indian Days powwow in Browning, Montana, where Mary Ground put up her tipi with its ring of stones). Schaeffer liked to talk with Indians in his office in the Museum of the Plains Indian in Browning, a place filled with their families’ beautiful craftwork, where they felt welcome. He worked with older Blackfoot men and middle-aged interpreters to create what he hoped would be definitive lists of Blackfoot (or at least Amskapi Pikuni, the Montana Blackfeet) bands, their leaders, and households. To do this, he laid large sheets of paper on the desk and drew up lists and genealogical connections with the men’s help. All this was significant knowledge to the Pikuni men, the bones of their recent history and with real-world utility in land claims (Figure 12.6).
Schaeffer advised Tom Kehoe to go out in the museum van to the homes of many respected elders, listening to them tell where there were tipi rings, why (in many instances) the camp had been there, and perhaps what family had camped there and then eventually asking whether all the rings called tipi rings were indeed rocks that had held down tipis. This was, in Speck’s terms, kitchen table ethnology. The second phase of the research was Listening Part 2, taking the Blackfoot to sites that they had told him about and talking about the observed phenomena on site. After months of listening and tying verbal information to actual places and observed data, Tom hit the libraries to search explorers’ journals and classic ethnographies for mentions of rocks and tipis.
Working for the People of Saskatchewan

I lived with Tom on the Montana Blackfeet Reservation for three years. He then successfully applied for the new position of provincial archaeologist for the Canadian province of Saskatchewan, and we moved to its capital, Regina. At that time (the 1960s), middle-aged residents of the province included a high proportion of people who had grown up during the Dirty Thirties, the droughts and erosion in the 1930s when few farms got any crops. There was no money. Almost no one could afford college. They stayed on the land, walking the dry, blowing fields. These were perfect conditions for noticing sites, with intelligent literate people having time to collect, read what they could find, and document their artifacts. The Saskatchewan Museum of Natural History, where Tom was based, had homegrown self-taught naturalists (especially the incomparable Fred Lahrman) and a geologist, Bruce McCorquodale, who worked at a professional level in the province. They knew hundreds of farmers, knew the landscapes, and knew the local collections. Tom drove the grid roads to the farms that his museum colleagues recommended, sat at the kitchen tables with hundreds of artifacts laid out before him, walked out with the farm people to their sites, and recorded what they told and what he saw. Many of the blowouts of the Dirty Thirties were by then blown over; the local collectors provided evidence of occupations since the Terminal Pleistocene.

There had been a little professional archaeology before Tom, mostly relatively recent salvage work by a Saskatchewan man who had archaeological training but not at the doctoral level. With so many data available and the eager cooperation of museum staff and residents of the province, Tom could construct a plan for excavating a few key sites that promised critical material for radiocarbon and stratigraphic chronologies and artifact diagnostics. We and several leading nonprofessional archaeologists who lived in the Regina area organized the Saskatchewan Archaeological Society in 1963 to link all those collectors into a democratic science for the province—as a century earlier, Joseph Henry and Spencer Baird had worked to make the Smithsonian a central node for the widely scattered self-taught naturalists of the young United States (Hinsley 1981:34).

We decided that two projects would be crucial to the program to map and manage Saskatchewan archaeological sites. A site with deep and clear stratigraphy was needed to discover a chronology of artifact types and occupations, and a site from the first European traders in the province was needed to establish
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both indigenous and imported artifact types at that time of historic contact. Avocational archaeologists John and Jean Hodges told us that the Gull Lake bison drive, in the southwest of the province, had indicated deep, clear stratigraphy when the avocationalists had tested it. Fur trade histories described a 1768 independent traders’ post on the Saskatchewan River as the first successful post in the province. Tom decided to undertake Gull Lake, which more than fulfilled its promise, yielding a sequence of bison pounds from 200 CE to 1876, near the extinction of wild bison herds (Kehoe 1973). I took the fur trade post, called François’ House after the French-Canadian voyageur who was a partner in it with the Scots immigrant merchant James Finlay (Kehoe 1978).

Neither Gull Lake nor Nipawin, where François’ House was located, had First Nations communities in their areas. Gull Lake may have been used by the Blackfoot before they retreated in the early nineteenth century and may have been used by Cree during the nineteenth century, and possibly by Assiniboine. Nipawin was Cree territory historically. For the two seasons of excavation at François’ House, I hired a crew of local laborers, middle-aged men who had worked on farms when they were younger, before machines made day labor redundant. Nipawin was homesteaded at the turn of the twentieth century, and some of these men had grown up in cabins not much different from the François and Finlay post cabins. They knew the history of the river running beside the terrace on which the post was built, the timber and carpentry, the soils, and how to deal with the bear and her cubs eating chokecherries for three days on the edge of our clearing. One day Reinhardt Lehne looked up from troweling and said to me, “I remember this floor. It has the fine black dust that my mother’s floor had from her sweeping the cabin every day with her twig broom.” Prompted, I realized that the floor of what was probably Finlay’s cabin was a finer texture than the floor of another cabin likely to have been the room for the men of their brigade. Checking texts later on eighteenth-century vernacular construction methods, sedimentology, Nipawin ecology, and so on showed me how much the crew’s knowledge from experience had facilitated recognition of the site’s data.

Lehne’s explanation of the quality of the floor of Finlay’s cabin made me think about the trader’s companion, mentioned in fur-trade histories as “a Saulteau woman” (Kehoe 2000:174). She bore Finlay a son, Jacques Raphael Finlay, called Jaco, conceived during the time they lived at Nipawin. Finlay had a legal, white wife back home in Montreal, whose son, James Jr., was a fur trader like his father and half-brother. The Saulteau woman was a “country wife,” an
Indian woman married à la façon du pays (country custom, as the traders and historians term it). François LeBlanc, the other partner in the post, had married a young woman, perhaps the daughter of a country wife, in a Catholic ceremony at Michilimackinac, the principal depot for the inland fur trade. She and their young son accompanied François. Rival Hudson’s Bay Company traders reported that François and Finlay casually permitted Indians to enter the post and kept no night guard, “even when the Natives are lying on their plantation” (Matthew Cocking quoted in Morton 1939:286).

I noted that indigenous artifacts—some small pots, stone knife and scraper blades, bone harpoon, awls, and thong softener, antler flaker, bone and shell beads—lay immediately outside the walls of the post, testimony to the “Natives lying on the plantation,” particularly women, to judge by the number associated primarily with women’s activities. A few sherds found close to the wall seemed impressed with traders’ stroud cloth, in place of the usual twined bag fabric on Woodland sherds. I was not doing “feminist archaeology,” a standpoint just breaking through into the discipline when I analyzed Francois’ House. I was doing straightforward inductive inference to reach the best explanation for the range of artifacts and their positionings in the site (Kehoe 2000). Underlying my interpretations were my ethnographic experiences with Blackfoot, Cree, and Dakota women, watching and occasionally helping with their tasks, and listening to their reasons for allocations of activities to women and to men.

Wearing the Ethnographer’s Cap to See Conventional Archaeology

Political upheaval, precipitated when Saskatchewan’s agrarian socialist government instituted provincial health care that drew wrath and a million dollars from the American Medical Association to call for an election, cut out the province’s heritage programs and my expectation to teach anthropology in the University of Regina. We moved to Lincoln, Nebraska, where Tom became director of the Nebraska Historical Society Museum and I started teaching in the university’s Anthropology Department. There, in the state capital, we both experienced, head-on, life in hierarchies. My department chair, Preston Holder, actively discouraged the five faculty members from talking with one another; he apparently feared that we would plot his ouster. Tom discovered that he was to operate a program given to him by his superior in the Historical Society, a patriotic Nebraskan who wanted monuments to Pony Express stations installed with ceremony, rather than research into the state’s history, postcontact or pre-
contact. We were saved by an opening for a curator of archaeology in Tom's home state's premier museum, the Milwaukee Public Museum. Tom got the appointment, and I was hired to teach at Marquette University, a Jesuit institution that realized it needed some anthropology to look fully credited. We were both free in summer to continue fieldwork, mostly projects in Saskatchewan. We could build on collegial relationships with archaeologists there and in Alberta, professional and nonprofessional, and with Indian people I knew from my dissertation research in the province. Once again, we could drive the grid roads to sites and reserves and listen to knowledgeable people.

Now we became tenured and published members of committees in our regional and national disciplinary organizations. These brought us into association with denizens of ivory-tower major research universities. It was the heyday of Lewis Binford. The only worthwhile archaeology would be “scientific” archaeology, which exclusively used hypothetico-deductive methodology and relied on statistics. “Culture history” was damned. Neither local people nor Indian people lived in that universe. Tom, employed in a civic museum with the excellent ethnographer Nancy O. Lurie, became more active in museology matters, stubbornly ignoring Binford as more and more midwestern archaeologists became his followers. I read seriously in history/philosophy of science, discovered the sociology of science, tried to publish papers showing how inappropriate Binford’s method was, and accumulated a pile of rejected papers. Only by clapping on my ethnographer’s cap could I make sense of the national meetings. I observed a phenomenon that resonated with my experience in high school: my suburban school had one (officially not permitted) boys’ fraternity and one girls’ sorority. Most of the girls who were excluded by the sorority sisters sat around sad. We certainly could have created our own sorority, but no one did. The excluded girls were disdained and that was that. The same thing happened, I saw, with so many marginalized by announced “theorists” in anthropology and by Binford and other self-appointed “scientists” in American archaeology. Out on the margins, we talked to one another without mounting any concerted challenge to the dominant cliques. Only a sociology of science can explain such failure of rational behavior.

Frank Speck had that experience of being marginalized, alongside his “Negro-looking” or Indian or “river people” research communities. Overt racism had a lot to do with that marginalization, yet there was more: dominance and marginalization are the means supporting ideologies. Those in power are the elect; those marginalized bear irredeemable defects. I found it curious that
very few of the purportedly very rational scientific practitioners would read history, philosophy, or sociology of science, although they routinely cited Binford’s consultant Merrilee Salmon and her husband, Wesley, whose field was philosophy of physics (see Turner 2007 for crucial differences not noted by the Salmons). Two exceptions were my friends out on the margins, Jane Kelley and Guy Gibbon.

Kelley from childhood accompanied her father, William Curry Holden, on his naturalist excursions in southern New Mexico and Arizona and adjacent Mexico. They listened to several generations of Yaqui, in Mexico and in Arizona, coming to know the land through Yaqui eyes. Jane recorded harrowing histories of Yaqui women during the Mexican war against the nation, when thousands of Yaqui were sent as slaves to plantations far away to the east (Kelley 1978). Disturbed by the “New Archaeology” subverting work like hers, Kelley teamed with Marsha Hanen, whose doctorate was in philosophy of science, to publish *Archaeology and the Methodology of Science* (Kelley and Hanen 1988). Guy Gibbon, a University of Minnesota faculty member specializing in northern Midwest archaeology, used a sabbatical to spend a year studying at the London School of Economics, at that time the center of philosophy of science debates. He then published *Explanation in Archaeology* (Gibbon 1989). No matter how sound their foundations and demonstrations of scientific reasoning appropriate to American archaeology were, Kelley and Gibbon did not inspire followers.4

Something much greater than reason organizes mainstream and marginal archaeology in North America (Mirowski 2005). We are all immersed in American culture. From early childhood, we listened to, saw, and moved in a society where white Christian men rule. Our school textbooks and school holiday celebrations steadily reinforced Anglo-Christian white male dominant status as the natural order of the world. *Natural Order* is the book that cracked open that ideological hegemony for me in 1981 (Barnes and Shapin 1979). It anthropologizes science, describing the cultural contexts of scientific researches. World War II shattered the centers-and-colonies structure of the “long century” from the early nineteenth to mid-twentieth century. The story of America, its frontiersmen with their axes and rifles creating civilization out of the wilderness, was being undermined as the disenfranchised struggled for rights. In reaction came a resurrection of nineteenth-century unilinear cultural evolution, constructed to scientifically prove the natural superiority of white men (Kehoe 1998:172–187). Archaeologists were welcomed to demonstrate the “law” (in quotation marks, to be sure) or regularities of cultural evolution, posed as working
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hypotheses to be validated by deductively sought data analyzed by means of statistical formulae. Was it just coincidence that the leader of the New Archaeology professing this science was a big blond man from the heartland of Christian Fundamentalism?

Now I was in the level of verstehen, a deeper, empathetic understanding. I was also engaged in anthropological efforts to contest creationism, doing ethnography with local fundamentalist churches and creationist groups and publishing on creationist culture and its contexts in American society. Parallels between evangelical fundamentalists and mainstream American archaeology, from the dominance of white men to the tautologies of proving hypotheses by deductively selecting supportive data, were all too apparent. The ethnographer’s cap filtered out superficial differences. From the 1493 papal bull Doctrine of Discovery, through Supreme Court Justice John Marshall’s trilogy of court decisions (1823–1832, 1840s), Manifest Destiny propaganda, Frederick Turner’s 1893 frontier hypothesis, and the entrenched paternalism of the Bureau of Indian Affairs, Anglo disposessions of American First Nations are legitimated. Challenges evoke uneasy emotions of insecurity. Acknowledging that racism continues to bedevil descendants of slaves and of “primitive” tribes is easy charity; acknowledging five centuries of deliberate deceits perpetrated by ironically revered men such as Thomas Jefferson subverts our whole edifice of common knowledge.⁵

Listening as Radical Critique

A “Fact” printed in 1776, to be read by “the candid world” concerned over 13 American colonies’ Act of Rebellion, is that King George “has excited domestic insurrections amongst us, and has endeavoured to bring on the inhabitants of our frontiers, the merciless Indian Savages whose known rule of warfare, is an undistinguished destruction of all ages, sexes and conditions.”⁶

Who were the “Indian Savages” on the colonies’ frontiers? Primarily the Five Civilized Tribes—Cherokee, Choctaw, Chickasaw, Creek, and Seminole—whose successful plantations competed with those of the English settlers. Jefferson himself, in “Logan’s Lament,” which he published in 1782, acknowledged that the most mercilessly savage attacks were those of the white vigilantes upon Indian families such as Logan’s (Wallace 1999). Is there any more succinct example of social charter myth than the Declaration’s final “Fact” that every American schoolchild is expected to read?
Jefferson’s chicanery, continued during his presidency (Miller 2008:96; Onuf 1997), laid a pragmatic foundation for the United States’ inexorable expansion across the continent. Indians were to be a vanishing people, physically surviving only as assimilated persons of color in the lowest stratum of society. Frank Speck’s uncovering of communities that against all odds had persisted through three centuries of persecution was a radical challenge to American hegemony. Furthermore, his bedside ethnology deliberately transgressed bedrock societal norms: *sleeping in the beds of the uncivilized*. Listening at the kitchen table is only somewhat less radical, for do we not often sip coffee from *their* cups? Our “archaeologies of listening” follow in this unfashionable practice that subverts the dominant role of “civilized scientists” in academic research.

Archaeology’s own philosopher of science, Alison Wylie (2015:208), explicates what Frank Speck knew well: “those who are socially marginal may be epistemically advantaged” not only to bring into view data overlooked or mis-identified by mainstream archaeologists but also “may catalyze counter-narratives and counter-norms that have the conceptual resources to capture forms of experience, dimensions of the world (social and natural) and ways of navigating it that are lacking in dominant culture.”

Wylie chooses her words with care. “Dominant culture” dominates: refusal to accept its decreed boundaries is a radical move. Both Wylie and her frequent interlocutor, Sandra Harding (2016), assert the value of recognizing standpoint (Wylie 2012), and both locate their work in postcolonial critique (Harding 2016; Wylie 2015). Speck antedates postcolonialism, yet his life and work take its standpoint.

Neither Tom nor I perceived an archaeology derived from listening to Indian people to be a radical move. As Claude Schaeffer laid it out, it was an obviously efficient method to get to the sites most likely to be significant in regional pre-contact history and gain information about them that would not be discovered by excavation. The method worked in Saskatchewan, too, although with a settler rather than indigenous populations. After we moved into academia, we put a name to the method: the direct ethnographic approach in archaeology (Kehoe and Kehoe 1985). In the heyday of hypothetico-deductive scientistic archaeology (e.g., Watson, LeBlanc, and Redman 1971) our opposing method was ignored by mainstream academic archaeologists, even as the profession was shifting to a majority of practitioners working through cultural resource management contracts.

The Native American Graves Protection and Repatriation Act (NAGPRA)
and the subsequent authorization of THPOs (tribal historic management officers) overturned the power hierarchy (Colwell 2017). With lawyers hovering in the background, museum officials and archaeologists opened doors to listen to Indian people examining collections and designating areas where archaeology might be permitted or would be out of bounds. Now listening may take place in tribal government offices more often than at kitchen tables. Archaeology has taken a historical turn (Sassaman 2010; Sassaman and Holly 2011). Even as academic subfields are broadened to include studies of the unenfranchised, the Doctrine of Discovery and Manifest Destiny still structure the way American history is taught. Seismic shifts take time.

In this chapter I have endeavored to highlight an anthropologist whose unusual naturalist bent provided him with an “epistemic advantage,” enabling him to conduct ethnographies of listening among socially marginalized communities. Frank Speck created a lineage of anthropologists who were comfortable listening at kitchen tables. It was my good fortune to take a summer job that led into this lineage.

Notes

1. We interviewed Mrs. Duck Chief, an aged Siksika Blackfoot who had lived in a bison-hide tipi as a child. She told us that hide stretches under rocks, while canvas tipis don’t stretch and rocks roll off. She said that people had to give up using rocks to anchor tipis when hides were no longer available. Canvas tipis are secured by pegs driven through loops on the tipis into the ground, like modern tents.

2. See, for example, John Wesley Powell’s description of American Indians in his report as director of the Bureau of Ethnology (Powell 1881:xxvii–xxx). He states unequivocally that before European invasions America was occupied only by “semi-nomadic . . . savage tribes” (Powell 1881:xxvii).

3. When we lived on the Blackfeet Reservation, we were befriended by Mae Aubrey Williamson (1892–1985), a leading citizen whose father was a white trader and whose mother was from a prominent Blackfeet family. Mrs. Williamson told me that her brother was blond and blue-eyed like their father; she and one sister looked “mixed-blood”; and the third sister looked Indian. All spoke fluent Blackfoot and knew the protocols of religion and custom.

4. Fogelin (2007) is a gentle, clearly written effort to defuse New Archaeology’s claim to scientific method. He explains how inference to the best explanation is generally followed by archaeologists, even those claiming to be more rigorously scientific, and emphasizes “empirical breadth” (encompassing “a wide variety of observations or evidence”) as a criterion of adequate explanation (Fogelin 2007:618). Fogelin specializes in the archaeology of South Asia Buddhism, outside American archaeology’s principal arena.

6. Declaration of Independence, “The unanimous Declaration of the thirteen united States of America,” signed July 4, 1776. The quotation is the last of “27 Facts” (the document’s phrase) presented to justify the colonies’ rebellion.

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