

**Secretsharers:
Intersecting Systems of Knowledge and the Politics of Documentation in
Southwesternist Anthropology, 1880-1930**

by

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Abstract

This dissertation examines shifting relationships between Anglo anthropologists and indigenous informants in the Southwestern United States, 1880–1930. Through an in-depth study of Southwesternist anthropological fieldwork, this dissertation explores the politics of ethnographic documentation, presenting anthropologists’ strategies and motivations for obtaining certain sorts of ethnographic data, and the management of ethnographic inquiry by indigenous communities that hosted (or tolerated) anthropologists. Southwesternist ethnographers pioneered fieldwork immersion in the 1880s and 1890s, but soon found that both Pueblo and Navajo social restrictions on the free flow of knowledge complicated attempts to produce ethnographic documentation of ceremonial practices. Ethnographers, in response to resistance to public documentation, forged more intimate, even clandestine, relationships with select informants to obtain novel and “secret” information. Despite the idea that modern anthropology is rooted in participant observation, *Secretsharers* reveals a turn away from it in the early twentieth century, toward tactics that isolated individual informants to provide in-depth cultural information on sensitive issues about which an Anglo (or any outsider) could not openly ask.

Southwesternist ethnographers grappled with the professional tensions of discretion and disclosure in their inquiries among Pueblo and Navajo communities. On the one hand, anthropologists needed to practice discretion regarding the sensitive components of sacred events and the identities of their informants. On the other hand, scientific standards demanded disclosure of ethnographic documentation to be considered a contribution to “scientific” knowledge. Even as anthropologists sought indigenous “secrets,” they worked to keep their ethnographic publications “secret” from the communities they presumed to describe. The disjunction between scientific epistemological standards and Pueblo and Navajo beliefs in the importance of contextualized, situated knowledge spotlights the unforeseen consequences of information accumulation and dissemination within scientific knowledge production—the presumption that the science of humankind has a “right to know,” regardless of risks to “secretsharing” informants or to the integrity of sacred, situated knowledge systems.

Introduction

Beginning in the late 1800s, Pueblo and Navajo Indian communities received a new sort of Anglo visitor, men and women calling themselves anthropologists, who came asking questions about their traditional beliefs, languages, and lifeways. These anthropologists came clutching pens and notepads; they asked different questions than other non-Indians, more familiar outsiders such as missionaries, government bureaucrats, traders, or local farmers and ranchers. For one, anthropologists were particularly interested in the language of each indigenous community, even though many Pueblos and Navajos could speak Spanish, the longstanding trade language of the region. They were also interested in aspects of local spiritual life. Whereas a history of spiritual repression from Catholic and later Protestant missionaries lived on in many an Indian community's memory, a history surely known to all Indian groups in the Southwest, the reasons for newcomers' interest could not be readily apprehended. At first, the outsiders asked basic questions of everyday life: how the people grew crops or raised their sheep, or what they called the seasons and the local flora and fauna in their native tongue. Anthropologists wrote down the answers provided by their interlocutors in their notebooks. Anthropologists also brought out their notebooks, to the dismay of many in Pueblo and Navajo settlements, in more intimate moments of community life, during ceremonies and storytelling.

Although Pueblo and Navajo groups differed vastly in their ways of living and understanding the world—and indeed were often at odds with one another and had been for centuries—they both found that these inquiring newcomers transgressed community propriety when they wrote or sketched at ceremonials. For both groups, documentation of ceremonials risked capturing a complex and sacred event involving stewarded knowledge. Captured on paper, the description of an event could travel elsewhere, yet a piece of paper scribbled with ink was not an adequate replication of a meaningful, contextual practice. In the years following the inception of Anglo anthropology in the Southwest, Pueblo and Navajo communities in the region came to understand ethnographic writing, sketching, and photographing as a form of knowledge sharing

that did not fit with their own systems of knowledge management. This dissertation explores the disjunction between situated knowledge systems and a universalistic one in Anglo human science, charting changes in Southwesternist ethnography's documentary tactics and investigating ethnographer's conflicting motivations regarding discretion and disclosure around ethnographic data. Ethnographer's promises of discretion, regarding the sensitive components of sacred events and the identities of their informants, were used as a means to quell the anxieties about the publication of sensitive information. I show, however, that the disciplinary motivation of disclosure, the publicizing of novel or sensitive ethnographic information, took precedence over an anthropologist's promises of discretion.

The tension between discretion and disclosure transformed as ethnographic documentation became more routine in the Southwest. While ethnographic fieldwork immersion arose in the 1880s and 1890s, establishing a standard of anthropological practice known as "participant observation," the respective responses to documentation by Pueblo and Navajo communities curbed community-wide studies. In response to resistance to public documentation, ethnographers forged more intimate, even clandestine, relationships with select informants to obtain novel and "secret" information. Despite the idea that modern anthropology is rooted in participant observation, I argue that Southwesternists studying indigenous ceremonialism—and its sacred constituent knowledges—ultimately shifted to tactics that isolated individual informants, leveraging goods and services for the individual rather than the community, to provide in-depth information on cultural "secrets."¹ Ethnographers thus forged relationships with informants that asked the indigenous party not only to risk social censure, but also to sanction the transformation of socially valuable, situated knowledge into written pages that could be circulated and widely shared.

When anthropologists arrived in the Southwest in the late nineteenth century, writing was not a common practice in Pueblo and Navajo settlements. These Indian communities were, however, familiar with literacy. Some Indian children had been to Anglo-run schools, locally or

¹ Secrets form a complicated linguistic-ontological terrain but have fairly straightforward characteristics that can be noted here (and will help circumvent an extended discussion of the ontology of the secret). At the most general level, secrets are hidden things. In this register, secrets can be places, persons, things, actions, feelings, thoughts, knowledge, etc.; the hidden or concealed status makes something secret. The thing's status—as hidden or not, as secret or not—is constituted by an informational relationship between groups of people, those who know and those that do not. There are some who are "in" on the secret, and others that are outside of it.

far afield, and could read and write in English. Both Pueblo and Navajo peoples understood writing as a processes of material replication and transformation, wherein speech or action became written words on paper. In the late nineteenth century, writing on paper was not integrated into long-standing social and ceremonial practices of Pueblo or Navajo communities. In both groups, social and ceremonial knowledge was gained through experience and/or personal tutoring at certain moment's in a person's life. Writing was, to be sure, predominantly associated with outsiders, with Spanish bureaucrats, with Catholic birth-records and Christian names given during baptism, with traders and land-dealers and their various systems of inventory—and, after 1880, with a new form of inquiring outsider, the ethnographer.

This is not to say that Pueblo and Navajos did not keep records or use symbols to denote ideas, events, and things. Rather, the different forms of Pueblo and Navajo methods of record-keeping were not transferable in the same forms as Anglo knowledge. The potential circulation of certain forms of knowledge, both to unsanctioned people inside a community or to outsiders, represented the chief issue of inscription practices. Pueblo memories stored in symbolic objects or Navajo reckoning through sandpainting stories captured certain events and messages, but such information could not be reproduced outside the context of the knowledge community and its stewards. Recorded knowledge that was special and enlivening required a steward, an interpreter. Unlike the record-keeping of bureaucrats, priests, traders, and anthropologists, these two distinct Indian peoples kept records solely for the benefit of, and comprehension within, their respective peoples.²

This dissertation shows that, in both Pueblo and Navajo communities, structural restrictions on the free flow of knowledge complicated ethnographers' attempts to capture it as data. Through a series of case studies, I explore the evolving conditions and rules for ethnographic fieldwork, presenting both anthropologists' strategies and motivations for obtaining ethnographic data, as well as the management of ethnographic inquiry by indigenous communities that hosted (or tolerated) anthropologists. Anthropologists were prevented from entering sacred ceremonial spaces with documentary technologies as simple as a paper notebook,

² The Pueblo and Navajo eschewal of outsider literary technologies and inscription practices is in many ways similar to resistance to documentation in the peoples of the Zomia region in Southeast Asia, as shown by James C. Scott. See James C. Scott, *The Art of Not Being Governed: An Anarchist History of Upland Southeast Asia* (Yale University Press, 2009). The work of the late Eric Michaels, which explored the complications of documentary practices among the Yuendumu community in the Northern Territory of Australia, also raises the question of whom documentation (specifically writing and literacy) is for. Eric Michaels, *Bad Aboriginal Art: Tradition, Media, and Technological Horizons* (University of Minnesota Press, 1994).

as when, in the early 1880s, John Gregory Bourke was excused from a sacred chamber in the Hopi (Pueblo) towns and Washington Matthews was compelled to put away his pocket writing pads in a Navajo medicine lodge. Sketching or writing during public events such as dances was strongly discouraged; Frank Hamilton Cushing, for instance, brought his sketchbook to a public ceremonial of the Zuni, a Pueblo Indian people, but the townspeople quickly prevented him from documenting the event. These early moments of resistance to ethnographic documentation in both Pueblo and Navajo contexts reveal the intersection of indigenous knowledge systems with the ideals of Anglo-American scientific knowledge production, with its emphases on credibility of unbiased observation, analysis, and democratic publication of data. The following cases show changing politics of documentation at the intersection of localized indigenous knowledge and (an aspirational, universal) Anglo knowledge. Anglo inquirers and their informants negotiated questions of knowledge replication, transferability, and responsibility. In this dialogical negotiation, anthropologists developed new strategies of documentation as Indians sought new ways to manage outsiders and their attempts to impinge into the private and personal elements of community life. Indigenous politics of documentation, I argue, unintentionally shaped ethnographic practices, as anthropologists in the region sought new avenues that would facilitate ethnographic documentation. This conflicted constitution of ethnographic practice reveals that anthropologists at the time believed they had a right and even obligation to document indigenous lifeways, even against the wishes of its subjects.

Both Pueblos and Navajos adopted a politics of documentation that emphasized the local contextualization and stewardship of sacred knowledge, and both resisted the inscription of their respective knowledge systems on paper by outsiders. But Pueblo and Navajo responses to documentation diverged as anthropologists continued to stream into the region after the turn of the twentieth century. Pueblos and Navajos were (and remain) distinct American Indian cultures, with different lifeways, beliefs, and languages. I offer case studies of these two groups because both became well-studied by anthropology as the discipline became professionalized at the turn of the twentieth century. The ethnographic notebooks of Southwesternists—including organized vocabulary notebooks, official memoranda, scribbled diary reflections, and notes scrawled in the moment, in conversation with informants—provide a glimpse at different qualitative research strategies beginning around 1880 and continuing into the 1930s. I focus on ethnographic documentation of Pueblo and Navajo communities because of the availability of source material

the strength of the “Southwesternist” anthropological scene in the late nineteenth century, which shared ethnographic strategies among themselves. By looking at the archival and published material of Southwesternist ethnographers, I assess how they often navigated and attempted to subvert Pueblo and Navajo politics of documentation, and how they transformed the information collected into ethnographic publications.

Although the Pueblo and Navajo placed similar emphases on locality and stewardship, their systems of knowledge were as distinct from one another as they were from Anglo epistemology. Pueblos, for instance, typically distributed specialized knowledge across their communities such that one person could never know “all things.” Fraternal societies and social cliques held certain forms of sacred knowledge that would be useful to the community as a whole. Leaders within these social groups were bound democratically by other members, and members mutually enforced the boundaries around the knowledge they stewarded. Because of this boundary-maintenance by Pueblo knowledge stewards, anthropologists increasingly had to find new ways to study the forms of ceremonial knowledge that was hidden from them. The clandestine tactics and secret informant-anthropologist relationships that emerged to work around the Pueblo system of knowledge maintenance, I demonstrate, caused internal strife within Pueblo communities, continuing to the present day.³

By contrast, Navajo sacred knowledge followed a more hierarchical system of knowledge maintenance. Navajo medicine practitioners, who had trained for many years in their craft, typically instructed individual students who showed interest and ability (and were willing to pay for the teaching). While lineage or birthplace might bring a Navajo student to a certain medicine singer, individual medicine singers had the authority to decide whom could be instructed and how the instruction was transmitted. In the Navajo case study, I show the evolving politics of documentation among a select number of medicine singers whom directly worked with Washington Matthews, an army surgeon and amateur anthropologist. While they did not speak for all of Navajoland, these singers came to tolerate certain forms of documentation in order to deepen the archive to ceremonial knowledge for themselves and their students, and they considered it their right to make this decision.

³ Joseph Suina, “Pueblo Secrecy: Result of Intrusions,” *New Mexico Magazine* 70, no. 1 (1992): 60–63. See also, Elizabeth Brandt, “Native American Attitudes towards Literacy and Recording in the Southwest,” *Journal of the Linguistic Association of the Southwest* 4, no. 2 (1981): 185–195.

The respective, historical nuances of each of these indigenous knowledge systems, and their interface with the documentary technologies of Anglo epistemological systems, charts changes in ethnographic fieldwork from before American anthropology's professionalization to the establishment of the Southwest as a well-trod ethnographic field. Notably, I locate early instances of intensive ethnographic study and language fluency, practices that came to be known as "participant observation," *avant la lettre*. From my point of view as a historian, an outsider to the discipline of anthropology, the story of the founding of American and European anthropology has taken on a mythic character in much anthropology graduate training; American anthropology, for instance, champions the cultural relativist (and four-field practitioner) Franz Boas as a founding father. I show that both contextualist narrative writing and "participant observation" can be seen in the history of Southwesternist anthropology, in fieldworkers such as Frank Hamilton Cushing, among the Zuni (Pueblo), Alexander Stephen among the Hopi (Pueblo), and Washington Matthews among the Navajo.⁴

From the perspective of anthropologists in the 1880s and 1890s, documentation in the "field" was a crucial constitutive element of scientific endeavors. Documentation of scientific explorations, including the gathering of ethnographic information, was a "literary technology" that narratively enrolled other scholars as witnesses to scientific knowledge production.⁵ For anthropologists in particular, in situ documentation was essential to scientific understanding of, and continued political engagement with, Native American communities: if documentation could not be produced in the presence of Native informants, the ethnographer may fall prey to faulty memory, romantic inclinations, or over-theorized conclusions. Anthropology, in other words, needed to document its subjects on the spot—or else find another way to extract ethnographic information from informants, away from the field.

⁴ This is not to discount "founding" narratives, but rather to add a bit more nuance to the development of American and European anthropology by recognizing a regional fieldwork tradition. Scholars have previously recognized the pioneering character Frank Hamilton Cushing's fieldwork. See Joan Mark, "Frank Hamilton Cushing and an American Science of Anthropology," *Perspectives in American History* 10 (1976): 449-86. Curtis Hinsley, "Life on the Margins: The Ethnographic Poetics of Frank Hamilton Cushing," *Journal of the Southwest* 41, no. 3 (1999): 371-382. For founding narratives, see George Stocking, *The Shaping of American Anthropology: A Franz Boas Reader* (University of Chicago Press, 1974); Regna Darnell, *And Along Came Boas: Continuity and Revolution in Americanist Anthropology* (J. Benjamins, 1998). The selective genealogical "origin" (and erasure of previous practitioners) makes sense in many respects. Boas was a remarkably prescient figure and serves as an exemplary figurehead for a discipline that presents itself as anti-racist, politically engaged, and critical of Euro-American modernizing practices. He is also an exemplary case for the establishment of anthropology in a mode of "normal science," following Thomas Kuhn. While the actors in my various case studies certainly are, or become, part of "normal science" after the rise of Boas after 1890, they draw from a larger trough of empiricist human and natural science fieldwork practice.

⁵ Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton University Press, 1985).

Given Native community management of ethnographic documentation as the century turned, Southwesternist anthropology expanded its strategies for data accumulation. In addition to the community-engaged “participant observation” of Southwesternists in the 1880s and 1890s, anthropologists in the early twentieth century included surreptitious documentation and isolated informant interviews in their ethnographic toolkit. This more covert style of ethnography prompted anthropologists to reconfigure their intellectual questions toward indigenous “inner life”—an area that was regarded as private and personal in both Pueblo and Navajo communities—and forge close relationships with informants of high social standing or, alternatively, without community members who were willing to provide information outside of the view of fellow community members.

Relationships with indigenous informants, in conjunction with a community’s politics of documentation, encouraged changing ethnographic documentary strategies. Two striking shifts can be located. First, anthropologists turned to qualitative writing and in-situ notetaking to document complex multi-sensory events, leveraging their relationships with select informants to obtain “unseen” or “secret” ethnographic data. However, resistance to documentation later prompted ethnographers in the 1920s and after to move *away* from the community to extract sensitive data, enticing informants to provide information away from their fellow community members. That Indian communities resisted documentation, even as anthropologists continually attempted to overcome such hurdles, illuminates an important question about ethnographic documentation: whom is documentation for, and what is its use? Southwesternists from 1880–1930, I show, perhaps conceptualized that their work would have value for the communities they studied as an archive for “disappearing” sacred knowledge, but the practices of isolating informants and withholding ethnographic articles and monographs from subject communities indicates that ethnographic documentation was primarily concerned with the development of their discipline and their own scholarly profiles.

In the end, Pueblo and Navajo politics of documentation reveal but never resolve the ineluctable consequences of cross-cultural interaction. Anthropological fieldwork, in time, did not even need to occur in the “field”—a hotel room or Anglo ranch nearby an Indian community was, at times, an even better venue for gathering ethnographic data. The isolation of informants was particularly pronounced in ethnographic projects on the constellation of communities collectively known as the Pueblo Indians because their “compartmentalized” knowledge system

policed anthropologist-informant interactions in their settlements. As anthropologist sought information on Pueblo “secrets” (largely having to do with ceremonialism), their relationships with Pueblo informants became “secret” as well. That these “secretsharer” relationships could emerge, we will see, shows the incommensurability of Pueblo epistemology and social practices with Anglo anthropological aims to document, even if ethnographers purported to be collecting information on behalf of Pueblos themselves.

This historical investigation of Southwesternist anthropology begins during Anglo-American development of the West, a period in which anthropology grew simultaneous to US national expansion wherein indigenous communities were subjected to assimilation measures enacted through educational, land-tenancy, and economic means. The 1880s saw a marked increase in Anglo-American ethnologists taking to the “field,” where they began to study human groups in situ and turned attention to their cultural practices and belief systems. The Southwest presented an ideal case for anthropological studies because of the density of Indian groups in the region, most of which lived in the same environments that they had for centuries.

For Pueblo and Navajo Indian communities in the nineteenth-century Southwest, relations with outsiders were not uncommon. The historical presence of hispanophone peoples, the Catholic church, and the Spanish/Mexican states stitched together a complex web of relationships across the Southwest.⁶ The Spanish had arrived in the region in the sixteenth century and had established permanent settlements by the seventeenth. Throughout the next two hundred years, relationships between the Spanish and regional indigenous communities vacillated. After continued spiritual oppression by Catholic missionaries, Pueblos revolted against the Spanish in 1680 and the newcomers retreated until 1692, when they returned to their settlements and began to practice more moderate conversion tactics and diplomatic relations with the Pueblos. While not always harmonious in their relations, Hispanos and Pueblos joined in common resistance to Navajo raiding throughout the eighteenth and nineteenth centuries.

Anglo-Americans arrived in the Southwest in the nineteenth century and, after the US annexation of New Mexico from Mexico in 1848, increased Anglo settlement deepened the high-

⁶ See Ramón A. Gutiérrez, *When Jesus Came, the Corn Mothers Went Away: Marriage, Sexuality, and Power in New Mexico, 1500-1846* (Stanford University Press, 1991); James Brooks, *Captives & Cousins: Slavery, Kinship, and Community in the Southwest Borderlands* (University of North Carolina Press, 2002).

desert exchange of goods, ideas, and kin. An Anglo-American campaign against the Navajo resulted in the relocation of a large amount of the Navajo population out of the region, an event known as the “Long Walk,” in 1864. The relocation was unsuccessful, however, and Navajos returned to their homelands in 1866, and relations between Navajo and others in the region pacified considerably in the following years.

The transformations of the Southwest’s major population centers into reflections of Anglo America were well underway in 1880s and 1890s. But the territories of New Mexico and Arizona were still tenuous spaces for the pursuits of Manifest Destiny, and the Anglo settlers were unsure of their futures in this region. For Indians of the Southwest, the persistence of Anglos and their increasing numbers in the region also made their own futures more uncertain. With the realization that the region’s Anglo-led development might displace or corrupt Indian culture, Anglo anthropologists saw a limited opportunity to gather ethnographic data in the region. By the turn of the century, anthropology had established the Southwest as a training ground for ethnography. Here, Anglo anthropologists sought to understand Indian communities and their lifeways, forming relationships with informants and accumulating data for their burgeoning science.

In the early days of ethnographic inquiry in the Southwest, visiting anthropologists made use of any available interlocutor—ideally a community leader who could speak Spanish or English, but traders, interpreters, or mixed-heritage people at the margins of a community were often enrolled as “informants,” too. In any given community, many local people could answer questions about the word “horse” or the time that corn was harvested. Ethnographic fieldworkers initially engaged in directed ethnographic inquiry, writing down vocabulary terms and basic information points on ordered, pre-formatted tables, but as the century wore on, in the face of problems of documentary complexity, they began to include nuanced descriptive accounts of Indian communities. This system of ethnographic documentation, organized via standardized data points, helped to establish a base-line of ethnological data that could be cross-culturally compared. The tables of “schedules” and the paper technology of the circulars, as well as the shift toward qualitative description, are discussed in Chapter 1. The rationalized tables of indigenous data, I show, limited ethnological study to comparative analysis between different Indian groups.

To understand the complicated intersection of diverse knowledge systems, I focus on formation of increasingly intimate, even secretive, relationships between anthropologists and Native informants, primarily Pueblo Indian communities and secondarily Navajo medicine men, in the Southwestern United States, from around 1880 into the 1930s.⁷ As ethnographic fieldworkers spent more time among peoples like the Pueblo and Navajo of the Southwest, they realized that the complexity of Indian life, including their intimate ceremonial practices and the contents of their “inner lives,” could not be rendered on pre-printed tables and linguistic schedules. Fieldworkers shifted toward narrative description and self-designed methods for organizing data collection. In Chapter 2, I present an exemplary case of a late-19th-century ethnographic project, Washington Matthews’s study of Navajo *chantway* ceremonialism under two medicine singers, Tall Chanter and Laughing Singer. Matthews’s sustained study of two Navajo ceremonials, the *Mountainway* and the *Nightway*, provided a proof-of-concept for in-depth ethnographic study of highly complex, multi-sensorial spiritual and aesthetic events conducted over nine days. In attempting to document Navajo chantways, Matthews had to make choices about what to document—and to whom it might be interesting—as he witnessed the Navajo singer’s enactment of interwoven songs, sandpaintings, fumigations, ritual paraphernalia, dancing, storytelling, and consumption. Even while Matthews ostensibly encountered the limits of ethnographic representation, he imagined his monographs as contributions to a “science of ceremony,” a method for studying the spiritual, inner lives of Indian peoples.

Matthews’s ethnography under the guidance of Navajo medicine singers progressed concurrently to more sustained anthropological study of the nearby Puebloan peoples. Though linguistically diverse, Pueblo groups were composed of nineteen distinct groups that shared common social and spiritual practices. Pueblos lived in similar permanent settlements (called pueblos, or “towns”), and recognized themselves as a part of an “ecumene,” a related group of people amidst a number of other indigenous groups, Hispano settlers, and Anglo newcomers. For anthropologists, Pueblo similarities and differences, as well as their settled agricultural lifeways and proximity to established infrastructures maintained by the Spanish and later Mexicans, made them easy subjects for sustained ethnographic study. But, as shown in Chapter 3, the specific

⁷ Foregrounding the histories of Indigenous and non-Indigenous relations has been demonstrated in indigenous studies literature. See Linda Tuhiwai Smith, *Decolonizing Methodologies: Research and Indigenous Peoples*. (Zed Books, 2013); Martin N. Nakata, *Disciplining the Savages, Savaging the Disciplines* (Aboriginal Studies Press, 2007).

system of Pueblo knowledge maintenance presented problems for ethnographic documentation. Across the Pueblo ecumene, individual communities distributed knowledge regarding ceremonial practices among a variety of social cliques, a decentralized organization of sacred knowledge stewarded and guarded against defilement. Pueblo politics of access restricted knowledge to “initiated” society members, each of whom was expected to guard their specialized knowledge (about foundational beliefs, histories, or ceremonial practices) from outsiders. This distributed character of sacred, stewarded knowledge—undergirding Pueblo beliefs and teachings—proved difficult for ethnographic study because informants were unwilling to share their “secrets.” Moreover, Pueblo social societies shared information about anthropologists’ persistent probing into sensitive matters and developed new tactics of resistance to documentation. Ethnographers soon found they were effectively shut out of public inquiry, their studies of indigenous social structure stymied by dissimulating (yet often still amiable) townspeople.

In response to Pueblo resistance to ethnographic inquiry, Anglo anthropologists like Elsie Clews Parsons and Leslie White turned toward developing informants in private, off-site spaces. Chapters 4 and 5 demonstrate the formation of “secretsharer” relationships, an entangled informant-anthropologist bond characterized by the clandestine exchange of private information and material goods, a compact solidified by an exchange that was not typically sanctioned by the indigenous community in question. Ethnographic documentation continued, but some information gathering as relegated to the shadows. Moreover, published ethnographic monographs were actively withheld from Pueblo communities by anthropologists, for fear that communities might bar them from future access.

Dueling notions of discretion and disclosure emblemized the secretsharer compact. On the one hand, Pueblo ceremonials were maintained as precious, esoteric knowledge, and anthropologists needed to practice discretion regarding these events and the informants who helped them obtain such information. On the other hand, scientific standards demanded disclosure of ethnographic data to be considered “scientific” in the first place. I illustrate that the tension between discretion and disclosure helped to maintain the clandestine exchange relationships and ethnographic access that Parsons and other prominent Southwesternists sought. Moreover, secretsharer relationships further developed the study of Indian mentality or inner life, including psychological and individual “life history” studies. While the shift toward inner life allowed anthropology to elaborate “secret” aspects of Indian culture through the study of an

individual (the informant), anonymity protections for indigenous informants lasted only as long as communities did not have access to publically-available ethnographic information about themselves. Ethnographic monographs, when they eventually ended up in Pueblo towns, revealed who was a “secretsharer”—who had, against the wishes of the community and intellectual leaders, shared sacred information with an outsider.

The consequences of secretsharer relationships haunt Indian communities, particularly the Pueblo, to this day. Anthropology long ago began to reconcile with dubious practices and practitioners in its disciplinary history. Still, the issues of cross-cultural knowledge sharing, especially in small and economically precarious communities such as Indian reservations, bring to the fore questions of the intellectual property of community-enlivening, sacred indigenous knowledge. The ineluctable problem of the materialization of knowledge through documentation and public circulation remains. A Pueblo question that might have been posed by a Pueblo informant in 1890 still applies today, in 2018: When an “origin story” or ceremonial is extracted from an indigenous community by a social scientist, who is it that becomes the steward of sacred knowledge?

Histories of anthropology in the US and Anglophone world have focused on institutional and funding histories, beginning with work in the 1980s on Smithsonian-based anthropologists from Joan Mark and Curtis Hinsley.⁸ Anthropology has long traced its intellectual legacy through generations of teachers and practitioners, and this genealogical interest has been reflected in biographical monographs on major figures and their intellectual and fieldwork legacies.⁹ While indigenous informants often appear in biographical works, the focus often rests clearly on the anthropologist.¹⁰ History of anthropology work broadened in 1990s and afterward

⁸ Joan T. Mark, *Four Anthropologists: An American Science in its Early Years* (Science History Publication, 1980); Curtis M. Hinsley, *Savages and Scientists: The Smithsonian Institution and the Development of American Anthropology, 1846-1910* (Smithsonian Institution Press, 1981). See also Thomas C. Patterson, *A Social History of Anthropology in the United States* (Berg, 2001).

⁹ Stocking, *The Shaping of American Anthropology*; Thomas R. Trautmann, *Lewis Henry Morgan and the Invention of Kinship* (University of California Press, 1987); Regna Darnell, *And Along Came Boas: Continuity and Revolution in Americanist Anthropology*. (J. Benjamins, 1998).

¹⁰ Some examples of treatments of Southwesternists in a biographical genre are worth noting, especially because their in-depth profiles aided my ability to describe the anthropologists in my case studies. For Adolph Bandelier, see Charles H. Lange and Carroll L. Riley, *Bandelier: The Life and Adventures of Adolph Bandelier* (University of Utah Press, 1996). For John Gregory Bourke, see Joseph C. Porter, *Paper Medicine Man: John Gregory Bourke and His American West* (University of Oklahoma Press, 1986). For Frank Hamilton Cushing, see Curtis Hinsley, “Ethnographic Charisma and Scientific Routine: Cushing and Fewkes in the American Southwest, 1879-1893,” In George Stocking (ed.), *Observers Observed: Essays on Ethnographic Fieldwork* (Univ. of Wisconsin, 1983): 53-69; Eliza McFeely, *Zuni and the American Imagination* (New York: Hill and Wang,

to incorporate emerging trends, including explorations through the lenses of colonialism, gender, and race. Work from within anthropology as well as in cultural studies has targeted questions of Western identity constitution through encounters with the Other, often through the dialectic of the savage/civilized.¹¹ The role of women ethnographers, and the gendered dynamics of fieldwork and disciplinary engagement, has been examined, especially in the US Southwest, by Barbara Babcock, Nancy Parezo, Catherine Lavender, and Louise Lamphere.¹² Race and its contestation from within anthropology has been a dominant analytic organizing principle in studies of cultural anthropology.¹³ There also exists a rich body of scholarship that has analyzed the role of racial questions in other branches anthropology, such as archaeology and anthropometry and physical anthropology.¹⁴ Histories of anthropology that focus on physical anthropology, for instance, make clear claims about the use of ethnological information for projects of racial categorization and, ultimately, white supremacy.¹⁵

2001). For Matilda Coxe Stevenson, see Darlis A. Miller, *Matilda Coxe Stevenson: Pioneering Anthropologist* (Univ. of Oklahoma Press, 2007). For Matthews, see Katherina Spencer Halpern and Susan Brown McGreevy, *Washington Matthews: Studies of Navajo Culture, 1880-1894* (University of New Mexico Press, 1997); Robert Poor, *Washington Matthews: An Intellectual Biography* (University Microfilms International, 1975). For Parsons, see Desley Deacon, *Elsie Clews Parsons: Inventing Modern Life* (University of Chicago Press, 1997); Louise Lamphere, "Feminist anthropology: the legacy of Elsie Clews Parsons," *American Ethnologist* 16 (1989): 518–533; Peter H. Hare, *A Woman's Quest for Science: Portrait of Anthropologist Elsie Clews Parsons* (Prometheus Books, 1985); Rosemary Levy Zumwalt, *Wealth and Rebellion: Elsie Clews Parsons, Anthropologist and Folklorist* (University of Illinois Press, 1992) For a general overview of Southwestern anthropology told through snapshot biographies of fieldworkers, see Don D. Fowler, *A Laboratory for Anthropology: Science and Romanticism in the American Southwest, 1846-1930* (University of New Mexico Press, 2000).

¹¹ Henrika Kuklick, *The Savage Within: The Social History of British Anthropology, 1885-1945* (Cambridge University Press, 1991); Adam Kuper, *The Reinvention of Primitive Society: Transformations of a Myth* (Routledge, 2005); Steven Conn, *History's Shadow: Native Americans and Historical Consciousness in the Nineteenth Century* (University of Chicago Press, 2004).

¹² Barbara A. Babcock and Nancy J. Parezo, *Daughters of the Desert* (University of New Mexico Press, 1988); Nancy J. Parezo, *Hidden Scholars: Women Anthropologists and the Native American Southwest* (University of New Mexico Press, 1993); Catherine Jane Lavender, *Scientists and Storytellers: Feminist Anthropologists and the Construction of the American Southwest* (University of New Mexico Press, 2006); Louise Lamphere, "Feminist anthropology: the Legacy of Elsie Clews Parsons," *American Ethnologist* 16 (1989): 518–533.

¹³ George W. Stocking, *Race, Culture, and Evolution; Essays in the History of Anthropology* (Free Press, 1968); Lee D. Baker, *From Savage to Negro: Anthropology and the Construction of Race, 1896-1954* (University of California Press, 1998); 2010; Scott Michaelsen, *The Limits of Multiculturalism: Interrogating the Origins of American Anthropology* (University of Minnesota Press, 1999); Barry Alan Joyce, *The Shaping of American Ethnography: The Wilkes Exploring Expedition, 1838-1842* (University of Nebraska Press, 2001); Ronald E. Martin, *The Languages of Difference: American Writers and Anthropologists Reconfigure the Primitive, 1878-1940* (University of Delaware Press, 2005).

¹⁴ For archaeology, see David J. Meltzer, Don D. Fowler, and Jeremy A. Sabloff, *American Archaeology, Past and Future: A Celebration of the Society for American Archaeology, 1935-1985* (Smithsonian Institution Press, 1986); Alice Beck Kehoe and Mary Beth Emmerichs (eds.), *Assembling the Past: Studies in the Professionalization of Archaeology* (University of New Mexico Press, 1999); David L. Browman, *Cultural Negotiations: The Role of Women in the Founding of Americanist Archaeology* (Univ. of Nebraska, 2013). For physical anthropology, see Stephen Jay Gould, *The Mismeasure of Man* (Norton, 1981); George W. Stocking (ed.), *Bones, Bodies, Behavior: Essays on Biological Anthropology* (University of Wisconsin Press, 1988); Ann Fabian, *The Skull Collectors: Race, Science, and America's Unburied Dead* (The University of Chicago Press, 2010).

¹⁵ Gould, *The Mismeasure of Man*; Fabian, *The Skull Collectors*; Elise Juzda, "Skulls, Science, and the Spoils of War: Craniological Studies at the United States Army Medical Museum, 1868-1900," *Studies in History and Philosophy of Biological and Biomedical Sciences* 40, no. 3 (2009): 156-67; Samuel J. Redman, *Bone Rooms: From Scientific Racism to Human Prehistory in Museums* (Harvard University Press, 2016).

Anglo anthropologists in all four subfields streamed to the Southwest after the 1880s because they saw a plethora of data to be extracted from a diversity of Indian communities in the region. Prior scholars have produced rich analyses of Southwesternist institutional networks, theoretical orientations, and the subsequent popular, romanticized representations of Southwestern Indians.¹⁶ It is clear that strong institutional-intellectual connections shaped the course of Southwest anthropology, as the region came to be thought of as a “laboratory of anthropology” that facilitated rapid growth of Americanist ethnology.¹⁷ Regional, on-the-ground anthropological fieldwork experience was bolstered by the professionalization of allied social sciences and their attendant training regimes.¹⁸

Much prior work on the region has informed my own understanding of the nineteenth- and twentieth-century American Southwest as a culturally diverse milieu of overlapping identities, languages, and customs. There were, and still are, many different Native peoples in the Southwest, including the Apache, Ute, Southern Paiute, Comanche, and O’odham. These peoples intermingled, traded, warred, and married with the Puebloan and Navajo peoples that I focus on in this dissertation. So, too, did Hispanophone and Anglophone peoples, of European and mixed descent. Historians have conveyed that this diverse milieu exchanged ideas, goods, and kin before and throughout the nineteenth century.¹⁹

¹⁶ Adam Fulton Johnson, *American Archaeology and the Conceptualization of Preservation: Edgar Lee Hewett and the Crafting of the 1906 Antiquities Act* (Master’s Thesis, Massachusetts Institute of Technology, 2011); James E. Snead, *Ruins and Rivals: The Making of Southwest Archaeology* (University of Arizona Press, 2001); Fowler, *A Laboratory for Anthropology*; Babcock & Parezo, *Daughters of the Desert*; Parezo, *Hidden Scholars*; Curtis M. Hinsley and David R. Wilcox, *The Southwest in the American Imagination: The Writings of Sylvester Baxter, 1881-1889* (University of Arizona Press, 1996); Jesse Green, *Cushing at Zuni: The Correspondence and Journals of Frank Hamilton Cushing, 1879-1884* (University of New Mexico Press, 1990); Gwynneira Isaac, “Whose Idea Was This?” *Current Anthropology* 52, no. 2. (2011).

¹⁷ Fowler, *A Laboratory for Anthropology*; Hinsley & Wilcox, *The Southwest in the American Imagination*; Curtis M. Hinsley and David R. Wilcox (eds.), *Coming of Age in Chicago: The 1893 World’s Fair and the Coalescence of American Anthropology* (Univ. of Nebraska Press, 2016); Snead, *Ruins and Rivals*.

¹⁸ Recent works have focused on educational institutions and training of social scientists which have nuanced prior studies of the professionalization of social science (Thomas L. Haskell, *The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth Century Crisis of Authority* (University of Illinois Press, 1977); Dorothy Ross, *The Origins of American Social Science* (Cambridge University Press, 1991)), focusing on institutional interdisciplinary interstices or gray areas and the propagation of a democratic public culture through scientific training in major universities. See Joel Isaac, *Working Knowledge Making the Human Sciences from Parsons to Kuhn* (Harvard University Press, 2012); Andrew Jewett, *Science, Democracy, and the American University: From the Civil War to the Cold War* (Cambridge University Press, 2012).

¹⁹ Juliana Barr, *Peace Came in the Form of a Woman: Indians and Spaniards in the Texas Borderlands* (University of North Carolina Press, 2007); David J. Weber, *The Spanish Frontier in North America* (Yale University Press, 1992); Brooks, *Captives & Cousins*; Gutiérrez, *When Jesus Came*; Andrés Reséndez, *Changing National Identities at the Frontier: Texas and New Mexico, 1800-1850* (Cambridge University Press, 2005); Gary Clayton Anderson, *The Indian Southwest, 1580-1830: Ethnogenesis and Reinvention* (University of Oklahoma Press, 1999); Stephen H. Lekson, *A History of the Ancient Southwest* (School for Advanced Research Press, 2009); William C. Sturtevant and Alfonso Ortiz (eds.), *Handbook of North American Indians. Volume 9*. (Smithsonian Institution, 1979); William C. Sturtevant and Alfonso Ortiz (eds.), *Handbook of North American Indians. Volume 10*. (Smithsonian Institution, 1983); Ned Blackhawk, *Violence Over the Land: Indians and Empires in the Early American West* (Harvard University Press, 2006); Brian DeLay, *War of a Thousand Deserts Indian Raids and the U.S.-*

In addition to Southwestern history, works that explore representations of American Indians via American anthropology are crucial for my project because they illustrate how images and texts circulated in nineteenth-century America and came to inhabit American consciousness.²⁰ As this dissertation reveals in Chapters 4 and 5, these Anglo-produced texts and images also made their way into Indian communities; Pueblos and Navajos saw representations of themselves and offered different reactions to them, ranging from proud, to equivocal, to outraged.²¹ By examining the circulation of ethnographies back to the sites of their origin, I show that ethnographic data accumulation had real consequences for host communities—and that these consequences can amass and continue to structure social memory in the present day.

Early twentieth-century anthropologists did not often consider the contingency and heterogeneity of race and culture, and historians have explored these disciplinary blind spots. Some have investigated the role of anthropology in popular consciousness, as the “discoveries” of the field wound their way back to the metropole.²² In the 1980s, for instance, George Stocking analyzed early anthropological fieldwork as a form of philosophical exploration wedged between (and attempting to reconcile) the romantic and the modern, and Curtis Hinsley articulated the developing years of the Americanist discipline as a dichotomous and dichotomizing enterprise that retread the boundaries between savagery and civilization.²³ These authors were critical of past anthropology’s cultural bifurcation and hierarchy-making, analyses that fit with criticism of contemporary anthropology in the 1970s from Vine Deloria, Tal Asad, and Dell Hymes.²⁴

Mexican War (Yale Univ. Press, 2008); Karl Jacoby, *Shadows at Dawn: A Borderlands Massacre and the Violence of History* (Penguin Press, 2008).

²⁰ McFeely, *Zuni and the American Imagination*; Brad Evans, *Before Cultures: The Ethnographic Imagination in American Literature, 1865-1920* (University of Chicago Press, 2005). See also James Joseph Buss, *Winning the West with Words: Language and Conquest in the Lower Great Lakes* (University of Oklahoma Press, 2011); Shari M. Huhndorf, *Going Native: Indians in the American Cultural Imagination* (Cornell University Press, 2001); Leah Dilworth, *Imagining Indians in the Southwest: Persistent Visions of a Primitive Past* (Smithsonian Institution Press, 1996).

²¹ For example, for reactions to photography, see Luke Lyon, “History of Prohibition of Photography of Southwestern Indian Ceremonies,” in Anne V. Poore (ed.), *Reflections: Papers on Southwestern Culture History in Honor of Charles H. Lange* (Ancient City Press, 1988). For inscription, see Chip Colwell-Chanthaphonh, “Sketching Knowledge: Quandaries in the Mimetic Reproduction of Pueblo Ritual,” *American Ethnologist* 38, no. 3 (2011): 451-467. See also Elizabeth Brandt, “On Secrecy and Control of Knowledge: Taos Pueblo,” in Stanton Tefft (ed.), *Secrecy: A Cross-Cultural Perspective* (Human Science Press, 1980), 123-146; Elizabeth Brandt, “Native American Attitudes towards Literacy and Recording in the Southwest,” *Journal of the Linguistic Association of the Southwest* 4, no. 2 (1981): 185-195.

²² See, for example, John S Gilkeson, *Anthropologists and the Rediscovery of America, 1886-1965* (Cambridge University Press, 2010); Michaelsen, *The Limits of Multiculturalism*; Lee D. Baker, *Anthropology and the Racial Politics of Culture* (Duke University Press, 2010).

²³ George W. Stocking, *Victorian Anthropology* (Free Press, 1987); Hinsley, *Savages and Scientists*.

²⁴ Vine Deloria, *Custer Died for Your Sins: An Indian Manifesto* (University of Oklahoma Press, 1969); Talal Asad, *Anthropology & the Colonial Encounter* (Humanities Press, 1973); Dell Hymes (ed.), *Reinventing Anthropology* (Pantheon Books, 1972).

Critical appraisals have expanded more recently to include investigations of the association of anthropology with imperialism and settler colonialism, as anthropologists both preceded and followed European settlers into various corners of the globe.²⁵ Outside of anthropology, historians have also overturned the lasting image of nineteenth-century indigenous groups as having a singular, homogenous, and often romantically harmonious cultures. Issues of the dynamic, cross-cultural elements of race and otherness have figured greatly into work at the intersection of Native history and American identity/consciousness, as exemplified in Southwestern history by Gary Anderson and Andrés Reséndez.²⁶ Change among social groups necessarily happens all the time. During anthropological fieldwork between 1880-1930, Anglo settlement was ongoing in the Southwestern United States; the development of Southwesternist anthropology was concurrent to power-differentiated negotiations with, and displacements of, American Indian populations, as recognized by prior scholars such as Robert Bieder and Lee Baker.²⁷

The concept of settler colonialism—in which the settler state seeks land instead of labor, disappearance instead of assimilation, and where the form of colonialism is not simply an event but a structure—is useful for situating my study of cross-cultural relationships based around information exchange.²⁸ Impressive recent work by Audra Simpson on Kahnawà:ke (Mohawk) refusals to accept “recognition” from the Canadian government because it undercuts their long history of political organization (back to the pre-Columbian Iroquois Confederacy)—joining work by Elizabeth Povinelli on the pitfalls of the politics of “recognition” by settler nation-states that indigenous people continually face—illuminates the troubles of power-differentiated cross-cultural exchanges.²⁹ Simpson and Povinelli rigorously home in on the structural and systemic processes that divide indigenous peoples by basing claims on historical, “factual” cultural authenticity. Unsurprisingly, notions of “authenticity” used by settler states are often drawn from

²⁵ Patrick Wolfe, *Settler Colonialism and the Transformation of Anthropology: The Politics and Poetics of an Ethnographic Event* (Cassell, 1999); Roger Sanjek, “Anthropology’s hidden colonialism: Assistants and their ethnographers,” *Anthropology Today* 9, no. 2 (1993): 13-18.

²⁶ Anderson, *The Indian Southwest*; Reséndez, *Changing National Identities at the Frontier*.

²⁷ Robert E. Bieder, *Science Encounters the Indian, 1820-1880: The Early Years of American Ethnology* (University of Oklahoma Press, 1986); Baker, *From Savage to Negro*.

²⁸ Wolfe, *Settler Colonialism and the Transformation of Anthropology*.

²⁹ Audra Simpson, *Mohawk Interruptus: Political Life Across the Borders of Settler States* (Duke University Press, 2014); Elizabeth A. Povinelli, *The Cunning of Recognition: Indigenous Alterities and the Making of Australian Multiculturalism*. Duke University Press, 2002. See also, Kim TallBear, *Native DNA: Tribal Belonging and the False Promise of Genetic Science* (University of Minnesota Press, 2013).

ethnographic work done around the turn of the twentieth century. In the present work, I draw inspiration from these works in my attempt to show the oscillating flexibility and rigidity of Pueblo and Navajo cultural forms over time, calling into question the idea of an “authentic” original to which Pueblos and Navajos could be held to account.

Relatedly, scholars have questioned who holds authority in producing “authentic” knowledge and stabilizing those concepts in time. In the colonial empires of Britain and France, for instance, colonies were often the site of anthropological investigation *and* the place where ethnographic information was applied, because colonial administrations drew on anthropological knowledge for governing “non-Western” or “uncivilized” populations.³⁰ Specific interlocutors, such as indigenous translators or guides, were able to shape scientific practices and guide its aims.³¹ My work joins a rich and growing scholarship on the interface of indigenous knowledge and the burgeoning science of anthropology. Isaiah Wilner, for instance, has illustrated the influence of Kwakwaka'wakw knowledge on the thought of Franz Boas, as well as the importance of informant-anthropologist relationships to the development of Boas' scholarly output through a study of his relationship with his chief informant, George Hunt.³² Like Wilner, I appreciate institutional and intellectual structures but recognize their diminished influence when anthropologists venture into the “field.” By rigorously positioning my narrative on-the-ground in the Southwest, among Native communities, I attend to overlapping structures of influence, specifically Pueblo/Navajo social systems and their structuration within a history of settler colonialism.

³⁰ See Stocking, *Victorian Anthropology*; Kuper, *The Reinvention of Primitive Society*; Kuklick, *The Savage Within*.

³¹ Joan H. Fujimura and Henry R. Luce, “Authorizing Knowledge in Science and Anthropology,” *American Anthropologist* 100, no. 2 (1998): 347-360; Akhil Gupta and James Ferguson (eds.), *Anthropological Locations: Boundaries and Grounds of a Field Science* (Univ. of California Press, 1997); Martin Nakata, “The Cultural Interface of Islander and Scientific Knowledge,” *The Australian Journal of Indigenous Education* 39, no. S1 (2010): 53-57; Donald, “Indigenous Métissage”; Marwa Elshakry, “When Science Became Western: Historiographical Reflections,” *Isis* 101 (2010): 98-109; Carla Nappi, “The Global and Beyond: Adventures in the Local Historiographies of Science,” *Isis* 104 (2013): 102-110; Agrawal, “Dismantling the Divide between Indigenous and Scientific Knowledge”; Arun Agrawal, “Indigenous Knowledge and the Politics of Classification,” *International Social Science Journal* 54, no. 173 (2002): 287-297. See also Margaret M. Bruchac, *Savage Kin: Indigenous Informants and American Anthropologists* (University of Arizona Press, 2018).

³² Isaiah Lorado Wilner, “A Global Potlatch: Identifying the Indigenous Influence on Western Thought,” *American Indian Culture and Research Journal* 37, no. 2 (2013): 87-114; Ned Blackhawk and Isaiah Lorado Wilner, *Indigenous Visions: Rediscovering the World of Franz Boas* (Yale Univ Press, 2018). Other examples of scholarship on indigenous knowledge and scientific knowledge include: Lyn Schumaker, *Africanizing Anthropology: Fieldwork, Networks, and the Making of Cultural Knowledge in Central Africa* (Duke University Press, 2001); Neil Safier, “Global Knowledge of the Move: Itineraries, Amerindian Narratives, and Deep Histories of Science,” *Isis* 101 (2010): 133-145; Arun Agrawal, “Dismantling the Divide between Indigenous and Scientific Knowledge,” *Development and Change* 26, no. 3 (1995): 413-439; Dwayne Donald, “Indigenous Métissage: A Decolonizing Research Sensibility,” *International Journal of Qualitative Studies in Education* 25.5 (2012): 533-55.

In the present work, I emphasize the “field” as a site of data accumulation, as an intersection of knowledge systems, and as the fulcrum for relationship-building between anthropologists and their indigenous informants. Scientific “fields” have become an emerging focus of knowledge production within the history of science. Recently, Adrian Young has traced the embedded histories of observation and scrutiny in social science field research on the remote Pitcairn Island, with its unique, isolated human specimens of interest to biological anthropology and language analysts.³³ Likewise, I sustain focus on Southwestern communities from 1880-1930, teasing out overlapping indigenous “encounters” with different scientific documentation techniques. At times, informants (such as Navajo medicine singers) allowed documentation but did so in order to manage the conditions of knowledge capture and establish the boundaries of interpretation. In this sense, *Secretsharers* joins recent work by Rosanna Dent, who has also investigated the interface and circulation of different modalities of knowledge—indigenous and scientific—with special attention the importance of interpersonal interactions and exchanges that undergird knowledge production in the field.³⁴

Previous studies of relationships between ethnographers and indigenous informants have probed the contribution of Native intellectuals to anthropological theory and practice. Margaret Bruchac has presented case studies of the collection of Native American stories and objects under the banner of salvage anthropology and demonstrated the omission of indigenous informants as intellectual collaborators in the co-production of Americanist anthropology. Bruchac also compellingly shows the irony of salvage anthropology: that “the salvage project caused some of the very losses it as predicated upon.”³⁵ Like Bruchac, I underscore not only the unforeseen consequences of ethnographic accumulation, but also the situated, entangled logics of epistemological intersections in fieldwork.

Historians and anthropologists have also ruminated on the perceived importance of “rapport” for successful anthropological data accumulation. Establishing rapport, in practice, did not always succeed; Holger Jebens, for instance, shows anthropologist Charles Valentine’s distrust of his informant’s truth-telling as he studied the Kivung cargo movement in what is now

³³ Adrian Young, “Mutiny’s Bounty: Pitcairn Islanders and the Making of a Natural Laboratory on the Edge of Britain’s Pacific Empire” (PhD Dissertation, Princeton University, 2016).

³⁴ Rosanna J. Dent, “Studying Indigenous Brazil: The Xavante and the Human Sciences, 1958-2015” (PhD Dissertation, University of Pennsylvania, 2017).

³⁵ Bruchac, *Savage Kin*, 19.

Papua New Guinea in the middle of the twentieth century. Valentine's field notes show his informants, too, suspected that white visitors withheld some key piece of knowledge from them regarding their apparent wealth.³⁶ Like Jebens, I analyze anthropologists' notes taken in the field to draw out moments of mistrust and miscommunication. In Valentine's case, his missteps and mutual distrust prevented him from preparing major publications on the Kivung. In my case studies, however, I argue that anthropologists of an earlier generation actually leveraged community suspicions to create exchange value for "secret" ethnographic information, wherein informants could reap some benefits from clandestine information-sharing with anthropologists.³⁷

A study of cross-cultural information exchange and intersecting knowledge systems must consider the production of knowledge in its context, entangled in a web of overlapping, "situated" knowledges.³⁸ Words like "accommodation" and "negotiation" and "contingency" arise in many studies of cross-cultural negotiation, such as the work of Juliana Barr and disparities between settler and indigenous interpretations of icons.³⁹ In the present work, scientific practices are enacted alongside a host of factors—field sites, communication systems, local traditions, the weather, and, most notably, Native epistemology—comprising a web of relations that comes together and is eventually called "scientific knowledge."⁴⁰ Following Kapil Raj, "science" is produced not by "dissemination," "transmission," or "communication of ideas, but [through] the processes of encounter, power and resistance, negotiation, and reconfiguration that occur in cross-cultural interaction."⁴¹ In the end, negotiation with, and resistance from, Pueblo and Navajo communities did shape ethnographers' approaches to documentation. This structuring of ethnographic documentation, however, did not prevent the circulation of

³⁶ Holger Jebens, *After the Cult: Perceptions of Other and Self in West New Britain (Papua New Guinea)* (Berghahn Books, 2010).

³⁷ Elements of secret-keeping in social cliques have been reflexively analyzed in more recent ethnographies. See, for instance, Erin Debenport, *Fixing the Books: Secrecy, Literacy, and Perfectibility in Indigenous New Mexico* (SAR Press, 2015); Lilith Mahmud, *The Brotherhood of Freemason Sisters: Gender, Secrecy, and Fraternity in Italian Masonic Lodges* (University of Chicago Press, 2014); Eric Gable, "A Secret Shared: Fieldwork and the Sinister in a West African Village," *Cultural Anthropology* 12, no. 2 (1997): 213–233; Chip Colwell, "Curating Secrets: Repatriation, Knowledge Flows, and Museum Power Structures," *Current Anthropology* 56 (2015): 263–275; Graham M. Jones, *Trade of the Tricks: Inside the Magician's Craft* (University of California Press, 2011).

³⁸ Donna J. Haraway, *Simians, Cyborgs, and Women: The Reinvention of Nature* (Routledge, 1991).

³⁹ Barr, *Peace Came in the Form of a Woman*.

⁴⁰ This "web" is a corruption of Geertz's notion of the coalescence of "culture." Clifford Geertz, *The Interpretation of Cultures: Selected Essays* (Basic Books, 1973).

⁴¹ Kapil Raj, "Beyond Postcolonialism... and Postpositivism: Circulation and the Global History of Science," *Isis* 104 (2013): 337–347.

information from the field to the metropole. Documentation finds a way, it seems, if a project is seen as essential to a group of relatively powerful actors.

Spotlighting the transformation of ideals and information is also important in the translation of fieldwork experience and documentation into publications. Scholarship on superlative fieldworkers such as Bronisław Malinowski and Franz Boas has identified the divergences between fieldwork experiences and subsequent monographic output.⁴² These draw attention, as my own work does, to the transformation of ethnographic data into smoothed, edited, and arranged publications for a scholarly audience. My special focus is the material transformation of experience into notes and notes into publications, a process of “redimensionalization” and “inscription.”

“Redimensionalization” in this project, following Hans-Jörg Rheinberger, describes the process in which a writing system, such as notetaking, attempts to capture rich, multisensory information and convey it on a two-dimensional page.⁴³ “Inscription” has become a well-known term in science studies and the history of science, whereby scientific processes are reduced to objects of information, often the printed word, and circulated as a coherent statement of a given inquiry. In short, an inscription process is anything that makes a scientific statement (or knowledge claim) more communicable.⁴⁴ Akin to the “literary technologies” used to circulate credible accounts of England’s Royal Society experiments in the seventeenth century, inscription technologies like pens and notebooks make knowledge claims moveable and replicable.⁴⁵ On a more localized scale, however, science studies scholars have shown that inscription processes also transform information—when copying notes or drafting a scientific paper, for example—and in turn can produce omissions, exaggerations, or insight in subsequent iterations.⁴⁶ My attention in Chapter 2 to the details of macro- and micro-scale transformations through

⁴² For Malinowski, see Michael Young, *Malinowski: Odyssey of an Anthropologist, 1884-1920* (Yale University Press, 2004). For Boas, see the essays in Ned Blackhawk and Isaiah Lorado Wilner (eds.), *Indigenous Visions Rediscovering the World of Franz Boas* (Yale University Press, 2018).

⁴³ Hans-Jörg Rheinberger, *An Epistemology of the Concrete: Twentieth-Century Histories of Life*. Duke University Press, 2010; Hans-Jörg Rheinberger, “Scrips and Scribbles,” *MLN* 118, no. 3 (2003): 622-636.

⁴⁴ Bruno Latour, “Visualization and cognition,” *Knowledge and Society* 6, no. 6 (1986): 1-40; Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society* (Harvard University Press, 1987).

⁴⁵ Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton University Press, 1985).

⁴⁶ Frederick L. Holmes, “Scientific Writing and Scientific Discovery,” *Isis* 78, no. 292 (1987): 220-35; Ursula Klein, *Experiments, Models, Paper Tools: Cultures of Organic Chemistry in the Nineteenth Century* (Stanford University Press, 2003)

redimensionalization and inscription is a novel contribution to the history of the human sciences, wherein I show Washington Matthews' experimentation with different material and interpersonal techniques to capture multi-sensory ceremonial practices, and his difficulties of adequately rendering them into scientific knowledge.

Scholars have adroitly shown the communicability of social scientific studies through social surveys, paper-based technologies that revealed information about heretofore unknown populations and presented them to a reading public. The work of Oz Frankel on Indian populations in the middle of the nineteenth century and Sarah Igo on a (largely white) American public in the middle of the twentieth bookend the period of my study, indicating the persistence of rationalized, tabular surveys and censuses for providing knowledge about populations to a public.⁴⁷ These studies productively show the governmental and popular interest in social surveys, and offer a glimpse into the accumulation of data, on paper, about human beings on the ground. In general, most work on population information accumulation and its deployment to shape or govern has focused on quantification and classification.⁴⁸ However, the study of ethnographic note-taking, a practice which I show shifts from survey-based information gathering toward qualitative writing around the 1880s, has not received as much attention. As my story unfolds, I show the limitations of qualitative data capture that ethnographers encountered with survey tables and chart the shift to contextualized, narrative note-taking in the field.

A related field that addresses the materiality of information—from papers and ledgers to identification tags and digital media—serves as a model for analyzing qualitative data production and its transformation into scientific knowledge.⁴⁹ “Raw data,” this literature shows, is myth; all “data” is an apparatus of decision-making, wherein assumptions about what can be measured are embedded into an experiment or survey design. Data is transformed once captured and brought into conversation with other data “sets;” once categorized and comparable, the presumed

⁴⁷ Oz Frankel, *States of Inquiry: Social Investigations and Print Culture in Nineteenth-Century Britain and the United States* (Johns Hopkins University Press, 2006); Sarah Elizabeth Igo, *The Averaged American: Surveys, Citizens, and the Making of a Mass Public* (Harvard University Press, 2007).

⁴⁸ Major work on measuring, quantifying and classifying human beings include: Ian Hacking, “Biopower and the Avalanche of Printed Numbers,” *Humanities in Society* 5 (1982): 279-95; Theodore M. Porter, *The Rise of Statistical Thinking, 1820-1900* (Princeton University Press, 1986); Alain Desrosières, *The Politics of Large Numbers: A History of Statistical Reasoning* (Harvard University Press, 2002); Andrea A. Rusnock, *Vital accounts: Quantifying Health and Population in Eighteenth-Century England and France* (Cambridge University Press, 2002); Geoffrey C. Bowker and Susan Leigh Star, *Sorting Things Out: Classification and Its Consequences* (MIT Press, 1999); John Carson, *The Measure of Merit: Talents, Intelligence, and Inequality in the French and American Republics, 1750-1940* (Princeton University Press, 2007).

⁴⁹ See, for instance, Lisa Gitelman (ed.), *“Raw Data” Is an Oxymoron* (MIT Press, 2013); Ben Kafka, *The Demon of Writing: Powers and Failures of Paperwork* (Zone Books, 2012).

saliency of data erases the criteria of the original design.⁵⁰ Following from these authors, I show that the categories of ethnographic data were originally outlined and standardized through ethnological circulars, data-tables which fieldworkers could fill out while interviewing informants. When ethnographers shifted toward contextualized narrative description, however, they struggled to standardize data for comparison across different indigenous groups. I demonstrate that *qualitative* data capture was not so much a process of distanced observation but rather one that came to be based on the relationships instanced by ethnographic documentary practices in situ and rapport with indigenous informants. When possible and appropriate, I place anthropologists and informants in a dialogical or symmetrical relationship with one another—an analytical strategy that identifies how ethnographers approached in-situ ethnographic documentation.⁵¹ In the turn-of-the-century Southwest, encounters between Indian communities and Anglo ethnographers were conditioned not by Anglo training or institutional affiliation, but by power-differentiated, negotiated actions around the technologies and practices of information gathering.

This dissertation contributes to the history of anthropology, and the history of the human sciences more broadly, in its focus on the shifting conditions under which anthropologists acquired ethnographic information from indigenous informants. Histories of anthropology have over-emphasized participant observation, and by doing so have overlooked ethical conflicts present in anthropologist's dueling motivations of discretion and disclosure. Immersive fieldwork, to be sure, generated much in the way of anthropological knowledge and propelled the discipline away from social evolutionary theory as ethnographers realized the importance of understanding social life from within the culture itself. Yet, as I show, participant observation could be readily eschewed—and often needed to be—so that ethnographers could access sacred or specialized knowledge. Indeed, much of the ethnographic archive on Pueblo and Navajo ceremonialism was obtained through surreptitious and unsanctioned documentation, individual informant tutoring, assisted and remote ethnographic writing by community members, and

⁵⁰ Bowker & Star, *Sorting Things Out*.

⁵¹ Barry Bloor, "The Strong Programme in the Sociology of Knowledge," in *Knowledge and Social Imagery* 2 (1976): 3–23; Michel Callon, "Some elements of a sociology of translation: domestication of the scallops and the fishermen of St. Brieuc Bay," *The Sociological Review* 32, no. 1 (1984): 196–233; Sally Wyatt, "Technological Determinism is Dead; Long Live Technological Determinism," in Edward J. Hackett (ed.), *The Handbook of Science and Technology Studies* (MIT Press, 2008).

correspondence with indigenous informants. Ethnographers' dueling discretion and disclosure practices continually prevented Indian communities, particularly in the Pueblo ecumene, from establishing their own boundaries between social and spiritual practices they wished to share with the public, and those they wished to keep private.

The history of anthropology has also left unexamined the social and material effects of inscription practices and qualitative ethnographic documentation, and thus has missed an opportunity to examine cross-cultural encounters through the politics of qualitative documentation. While Anglo representations of Southwestern Indian communities have been studied as reflections of American aspirations and aesthetic nostalgia, the forthcoming chapters take a symmetrical approach, analyzing how anthropologist-informant relationships were conditioned by documentary technologies, especially the medium of writing, and the respective situated assumptions about the value of record-making by Anglo and Indian communities. Even though some indigenous informants appeared to facilitate or sanction ethnographic documentation, little attention has been paid to instances when Anglo ethnographers compelled informants share sensitive information and thus to put themselves at risk of social censure. Anthropologists did make some attempts to protect informants and their continued access to communities by preventing the return of ethnographic documents. But the return of ethnographic documents to host communities had real consequences for informants. In the end, while the intrusions of ethnographers on community life proved disruptive, material acts of documentation threatened to diminish sacred knowledge and deepen community tensions. Joining a rich body of studies of quantitative inquiry in the history of the human sciences, I argue that the effects of qualitative documentation on human populations is an equally important area of study, one that allows us to see the power of knowledge production not only as one of enumeration, but also of the violation of private matters through written description.

This dissertation shows that Southwesternist ethnography, while certainly a field that bolstered the development of American anthropology in the late-nineteenth and early-twentieth centuries, was marked by contestation over notions of discretion and disclosure. I examine the negotiated politics of documentation between Native communities and Anglo anthropologists, specifically the shifting possibilities of fieldwork engagement within the structuring conditions of the *field*—i.e., the attitudes of the host community, indigenous hospitality, the intersections of different epistemologies on the status and availability of knowledge, the presence and role of

inscription technologies in indigenous communities, and historical regional enframing through Anglo and Hispano settler colonialism.⁵² The ever-situated nature of meaningful knowledge for Pueblos and Navajos came into tension with an Anglo logic of accumulation and publication. Aggressive and surreptitious tactics of ethnographic documentation put sacred, stewarded knowledges at risk of being diminished as cherished social practices. Anthropologists, heads down with their own concerns, missed a message that Southwestern Indians conveyed to them again and again, across the years 1880-1930: Some knowledge is not for everyone; some knowledge loses meaning when shared; some knowledge resists the Anglo ideal of universal, democratic circulation of information. To protect it, to keep it meaningful and culturally enlivening, let some knowledge stay where it is cultivated and cared for, let it remain unwritten.

⁵² This is a form of historical epistemology, as described by Hans-Jörg Rheinberger: How do historical conditions establish the acceptable forms of knowledge generation? Hans-Jörg Rheinberger, *On Historicizing Epistemology: An Essay* (Stanford University Press, 2010). I am also influenced by the notions of the knowledge “apparatus” and of historical ontology—the emergence of conceptual devices that become real, usable tools with effects in the world. For apparatus, see Giorgio Agamben, *What Is an Apparatus?: And Other Essays* (Stanford University Press, 2009). For historical ontology, see Ian Hacking, *Historical Ontology* (Harvard University Press, 2002). Peter Galison has proposed mixing historical epistemology and historical ontology, asking “under which circumstances and in what conceptual form do collecting and observing together bring new kinds of objects or classes into existence?” Peter Galison, “Ten Problems in History and Philosophy of Science,” *Isis* 99, no. 1 (2008): 111-124.

Chapter 1 The Paper Technologies of Ethnological Data Accumulation

After the founding of the Smithsonian Institution (1846), it did not take long for the nation's premier scientific organization to develop interest in the study of America's indigenous population. Joseph Henry, the first secretary of the Smithsonian, oversaw the production of *Instructions Relative to the Ethnology and Philology of America*, crafted by a linguist in the Institution, George Gibbs, in 1863. Henry penned the preface for these *Instructions*, printed as a circular to distribute to government agents and citizens across the country, stating,

The Smithsonian Institution is desirous of extending and completing its collections of facts and materials relative to the Ethnology, Archæology, and Philology of the races of mankind inhabiting, either now or at any previous period, the continent of America, and earnestly solicits the coöperation in this object of all officers of the United States Government, and travellers or residents who may have it in their power to render any assistance.¹

Ethnology had been a coherent intellectual pursuit since the founding of the United States, but it faced a massive absence of reliable data. This hole in the archive of ethnographic information was understandable, in part because of the breadth of the American state in the middle of the 19th century, stretching from Maine to Southern California, and in part because of the diversity of Indian communities in the American West and the relative lack of Anglo-American settlement (and other forms of territorial domination). And though ethnology was coherent as an intellectual pursuit, *ethnologists* were few and far between, many of them “armchair” synthesizers of data rather than collectors of it.² Hence Henry's charge for “officers, travellers or residents” to “render any assistance.”

¹ George Gibbs, *Instructions for Research Relative to the Ethnology and Philology of America: Prepared for the Smithsonian Institution* (Smithsonian Institution, 1863), 1. See also James C. Pilling, “Catalogue of Linguistic Manuscripts in the Library of the Bureau of Ethnology”, in John Wesley Powell et al., *First Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution, 1879- '80* (US Government Printing Office, 1881).

² In this dissertation, I use the terms ethnology and anthropology interchangeably. While there is some arguable difference between ethnology (the study of races of the world) and anthropology (the study of humans), both terms were used in the mid-to-

To aid those charged with ethnological accumulations, Gibbs included in the circular a series of “Hints to Ethnological Inquiry.” Ethnology should, Gibbs stated, work to reveal both the present condition of American Indian tribes as well as their past history (noting, of course, that the latter aim was a more difficult goal, for the Indians did not write down their prior history). In more specific language, Gibbs outlined the goals of American ethnology:

To ascertain, if possible, the origin of the aboriginal population of this portion of our globe, to trace the migrations and conquests of the various nations that composed it from one part of the continent to another, to disclose their superstitions, their manners and customs, their knowledge of the arts of war and peace—in short, to place before us a moving panorama of America in the olden time.³

Ethnology had under its charge a large suite of history, including genealogy, mythology, and social study.

Moreover, the ethnologist’s inquiries would help the tribes of the United States in their present condition. “We are accumulating data for beneficent, legislative, and philanthropic action on their behalf,” wrote Gibbs.⁴ This information could also, of course, be used to quell Indian agitations and direct diplomatic efforts or military strategy as Anglo-Americans continued to settle the West. This was a large project, but one to which individual citizens could lend their hands by compiling data points about local tribes: names (including the terms given to a tribe by surrounding Indian groups), geographical position, number in population, their dress and appearance, their dwellings and arts, elements of their social and political life, and more. To obtain the requisite data, ethnologists needed to be vigilant and exhaustive in their accumulations. Ethnologists should neglect “no source of information that promises to cast even a single ray of light into the obscurity with which the subject [of ethnology] is surrounded.”⁵

The second portion of the 1863 circular featured a guide to philological accumulation. Philology in the mid-19th century fashioned itself a science of historical linguistics, built around etymological (word-origin) research to ascertain “family relations” and affinity between the various nations and peoples of the world.⁶ Building on prior philologists such as Edward Hale

late nineteenth century to refer to the study of human societies and cultural practices. “Ethnology” was more commonly used in the nineteenth century before being displaced by “anthropology” in the twentieth.

³ Gibbs, *Instructions*, 7.

⁴ Gibbs, *Instructions*, 7.

⁵ Gibbs, *Instructions*, 7.

⁶ Sean P. Harvey, *Native Tongues: Colonialism and Race from Encounter to the Reservation* (Harvard University Press, 2015).

and Albert Gallatin, Gibbs understood that lay accumulators could only provide a limited amount of linguistic information because the subtleties of inflection and dialect were silent to the untrained ear. But by providing a guide to linguistic accumulation, Gibbs and his Smithsonian colleagues knew they could obtain a basic foundation for philological analysis. His text guided the accumulator through the difficulties of general categories (“Their languages are deficient in *generic terms*”) and apprised them of the tricky differences between categories of gender, age, and filial relations.

Gibbs included a section on orthography, too, in order to aid the linguistic accumulator in the difficult job preserving oral speech when transformed into writing (English having far less certainty in pronunciation than Spanish or French, or so claimed Gibbs). Adding to the problems of phonetic transcription, Gibbs noted that the variety of Indian languages featured sounds that were “uncouth” and many tribes spoke in ways “almost beyond our capacity to imitate and certainly to write, without some addition to the ordinary alphabet.”⁷ Gibbs therefore provided a guide to orthographic writing and a long list of “Comparative Vocabulary” to help the lay accumulator in his or her philological accumulations.

In response to the problems of ethnographic accumulation of a diversity of groups, Gibbs and other ethnologists and philologists looked toward paper technologies to construct a standardized system of ethnological and philological inquiry. Initially directed at non-ethnologists, such paper technology nonetheless stabilized the practices of ethnology and philology because their desiderata (their accumulative priorities) specified the sorts of data to be collected, and therefore established the conditions of possibility for analysis.

Language data collected could be compared with other, similarly rendered word lists. Mid-century ethnology and especially philology subscribed to a method of comparative analysis, whereby anthropological data was compared between social groups, both to understand their relations among one another (historical and contemporary) as well as their potential place on a scale of social evolutionary development. The “mental state” of Indian groups could also be investigated through comparative analysis of linguistic sophistication. Comparative ethnology and philology addressed the heterogeneity of American Indian communities, and (in theory) from this data set ethnologists and philologists could reach broad-based conclusions on, say, the

Julie Tetel Andresen, *Linguistics in America 1769-1924: A critical history* (Psychology Press, 1995).

⁷ Gibbs, *Instructions*, 17.

migration path of American Indians into the New World, the stages of social development possible in the pre-Columbian world, or the development of material culture and “ideology” (or mentality, for mid-century philologists) after European contact.

But large-scale comparative ethnology was an illustration painted by individual brushstrokes, each a tribe or community that held its own conception of their origin, history, and relationship to the present world. And within those communities, at a scale even further narrowed, the idiosyncratic data to be contributed to American anthropology was provided by one or two (and rarely more) Native informants. Data management, including the standardization of categories of analysis and the stabilization of a system in which spoken phrases could be rendered into writing, became a major endeavor that would structure American anthropology as the 19th century came to a close.

In what follows, I review the acts of information management that percolated through ethnology and philology throughout the 19th century, with a focus on the actual paper technologies—circulars and questionnaires for ethnological inquiry—that facilitated data accumulation.⁸ Paper technologies of ethnographic analysis and their desiderata, or the information prioritized for accumulation, expanded throughout the second half of the 19th century to tackle notions of Indian “inner life” and the ways in which they came to understand the world. As the scope of the circular’s data accumulation expanded, so too did the troubles of managing such information in a mode suitable to comparative analysis. I argue that the expanding desiderata for ethnology and philology ultimately revealed problem areas of data accumulation for comparative ethnology, especially the limitations of serialized, non-professional data accumulation in the field. I show that these limitations, while frustrating for ethnologists, systemized a process of data collection and also inadvertently opened up a new terrain of ethnography—the study of the “excesses” of the standardized suite of questions. Linked to renewed interest in the Indian “mindset,” new epistemological lines of inquiry, especially around Indian “opinions” and education, also known as “sophiology,” carved out a path for what came to be known as participant observation.

Nineteenth Century American Ethnology & The Design of Paper Tools

⁸ In subsequent chapters, the importance of individual informants will come into clear view.

To direct research along desired paths, ethnologists designed reproducible paper technologies, variously called circulars, questionnaires, schedules, and linguistic vocabularies, to collect ethnographic data. This diversity of terms actually describe the different lives of a piece of paper as information was scribbled down: it was circulated to a large number of people, featured questions to be answered, presented an ordered sequence of inquiry, and standardized the words and content to be recorded.⁹ These paper-based, epistolary apparatuses for collecting data in one location and transporting it to another all shared a prescriptive format, which directed the recipient's actions.

Prescriptive formatting in ethnological data accumulation was in part borrowed from early modern state administration, where an ordered collection of desired information was inscribed in pre-formatted lists or tables and then filled in at a later time.¹⁰ The form of the ordered table was borrowed from its use in studies of labor, disease and injury, birth and mortality rates, and other forms of census-taking.¹¹ Questionnaires could be taken into the field as ordered reminders that neatly captured an individual scholar's goals, which ranged from the preservationist to philanthropic. But ethnographic questions were also packaged as circulars and directed through the mail to people already present in the field who were well-placed to collect data.

Ethnology's standardization push was also influenced by archaeological and natural science practices, especially other field sciences such as geology and botany. In the mid-century United States, field science was supported through government exploring and surveying

⁹ For the social life of things, see the classic essay by Arjun Appadurai (1986), where different values/meanings are imbued in objects over time (and within different milieus), which give objects a history that also reflect the conditions or ideas of their various contexts. See Arjun Appadurai (ed.), *The Social Life of Things: Commodities in Cultural Perspective* (Cambridge University Press, 1986). Tracing this works particularly well with paper documents, which can actually acquire new inscriptions. Documents in circulation, and with accumulating inscriptions, is a vibrant area of study. See, for instance, Michael Warner, *The Letters of the Republic: Publication and the Public Sphere in Eighteenth-Century America* (Harvard University Press, 2009); Ursula Klein, *Experiments, Models, Paper Tools: Cultures of Organic Chemistry in the Nineteenth Century* (Stanford University Press, 2003); Anke te Heesen, "Accounting for the natural world: double-entry bookkeeping in the field," in *Colonial botany: science, commerce, and politics in the early modern world* (Univ. of Pennsylvania Press, 2005), 237-251; David M. Henkin, *The Postal Age: the Emergence of Modern Communications in Nineteenth-Century America* (University of Chicago Press, 2008); Elizabeth Yale, "Marginalia, commonplaces, and correspondence: Scribal exchange in early modern science," *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 42, no. 2 (2011): 193-202.

¹⁰ For enumeration in state administration, see Andrea A. Rusnock, *Vital accounts: Quantifying Health and Population in Eighteenth-Century England and France* (Cambridge University Press, 2002); Frankel, *States of Inquiry*; Libby Schweber, *Disciplining Statistics: Demography and Vital Statistics in France and England, 1830-1885* (Duke, 2006). For statistics as a governmental innovation transferred to science, see Theodore M. Porter, *The Rise of Statistical Thinking, 1820-1900* (Princeton University Press, 1986); Ian Hacking, *The Taming of Chance* (Cambridge University Press, 1990); Alain Desrosières, *The Politics of Large Numbers: A History of Statistical Reasoning* (Harvard University Press, 2002).

¹¹ Frankel, *States of Inquiry*; Rusnock, *Vital Accounts*. For the use of prescriptive formatting as used in medicine, see Volker Hess and J. Andrew Mendelsohn, "Case and Series: Medical Knowledge and Paper Technology, 1600-1900," *History of Science* 48, no. 3-4 (2010): 287-314.

expeditions. Large parties composed of soldiers, explorers, and scientists ranged across the American West. Geological survey was typically the priority of the scientific members of a party, but these expeditions (such as John Wesley Powell's explorations of the Colorado River in 1869 and 1871–72) also allowed time for ethnographic data accumulation among a variety of Indian peoples. Without extended time among different culture groups, ethnography was a primarily observational (ocular-centric) activity, although basic vocabulary accumulation duties could also be assumed by a party member.¹² Data accumulation, here, was primarily inductive and descriptive. As the West was settled by Anglos throughout the nineteenth century, a diverse conception of Native peoples would emerge, but surveys served to obtain a basic level of information about a region, including its topography, flora and fauna, and its indigenous inhabitants.

A notable early ethnological paper technology came right from the peak of American social and political life. Future President Thomas Jefferson headed an effort within the American Philosophical Society to develop a *Circular* for ethnological data collection in 1798; Jefferson himself had been collecting Indian word lists for over a decade prior.¹³ As President of the United States, Jefferson continued efforts to gather information on the Indian communities within, abutting and far from the United States. When Jefferson authorized the Lewis and Clark Expedition up the Missouri River, he directed that a set of *Instructions* for ethnological and linguistic information accompany the expedition.¹⁴

Information about Indians captured on questionnaires was later compiled and presented for popular, scholarly, or governmental consumption. Henry Rowe Schoolcraft began to publish a six-volume series on the *Indian Tribes of the United States* in 1853. He drew from a number of

¹² For the nineteenth century field sciences, see Jeremy Vetter, "Field life in the American West: Surveys, networks, stations, and quarries," *Scientists and Scholars in the Field: Studies in the History of Fieldwork and Expeditions* (2012): 225-258; Jeremy Vetter, *Field Life: Science in the American West during the Railroad Era* (University of Pittsburgh Press, 2016); Aaron Sachs, *The Humboldt Current: Nineteenth-Century Exploration and the Roots of American Environmentalism* (New York: Viking, 2006); Londa Schiebinger and Claudia Swan (eds.), *Colonial Botany: Science, Commerce, and Politics in the Early Modern World* (University of Pennsylvania Press, 2007); Mary Louise Pratt, *Imperial Eyes: Travel Writing and Transculturation* (London: Routledge, 1992). For continental surveys, see William H. Goetzmann, *Exploration and Empire: The Explorer and the Scientist in the Winning of the American West* (Knopf, 1966); Richard A. Bartlett, *Great surveys of the American West* (University of Oklahoma Press, 1980). For Powell's expeditions, see Donald Worster, *A River Running West: The Life of John Wesley Powell* (Oxford University Press, 2000).

¹³ Don D. Folwer, "Notes on Inquiries in Anthropology: A Bibliographic Essay," in Timothy H. H. Thoresen (ed.), *Toward a Science of Man. Essays in the History of Anthropology* (Walter de Gruyter, 1975); H. Christoph Wolfart, "Notes on the early history of American Indian linguistics," *Folia Linguistica* 1, no. 3-4 (1967): 153-171.

¹⁴ Thomas Jefferson, "Comparative Vocabularies of Several Indian Languages, 1802-1808," Thomas Jefferson Papers, APS. See also, Fowler, "Notes on Inquiries in Anthropology"; Harvey, *Native Tongues*.

sources, including correspondents he had enrolled through a questionnaire of his own, circulated in 1847. His questionnaire, *Inquiries, Respecting the History, Present Condition and Future Prospects, of the Indian Tribes of the United States*, featured 348 major questions. A concluding “question” listed an additional 350 vocabulary terms in three columns and directed the respondent to place Indian language terms to the right of the English equivalent.¹⁵

Because *Inquiries* was quite lengthy, Schoolcraft recognized the limitations of individual respondents. “It is not supposed that every person who sits down to answer these queries . . . will take an equal interest in them, or feel equally prepared, with facts and observations, to reply to all.”¹⁶ He hoped, instead, that the broad selection of questions would serve as “hints” that directed one’s inquiry. “Hints” also suited the inductive ethos of American science at the time because it suggested open-ended inquiry, a recognition that gathering information about diverse Indian groups was an endeavor filled with contingencies.

Questionnaires and other ethnographic paper technologies structured a network of information exchange and drew together the science of ethnology as a collective endeavor. The network instanced by circulars and correspondence also crystalized ethnological themes and questions that were “in” at a given moment. Thus inquiries reflected shifts in ethnological science—at least as it was rendered by scholars with the institutional connections, money, and far-flung contacts needed to properly circulate a circular.¹⁷

Moreover, circulars attuned budding ethnographers in the 1870s and 1880s to the topics that would bring them notice as scientific accumulators, solidifying their contribution to knowledge.¹⁸ Questionnaires could easily be carried into the field—they were simple sheets of paper, after all—and used as *aides de memoire* for questions one should ask an Indian, even if not followed to the letter. Thus, questionnaires could be used to give structure to an individual

¹⁵ Henry Rowe Schoolcraft, *Inquiries, Respecting the History, Present Condition and Future Prospects of the Indian Tribes of the United States* (1847); Reprinted in; Henry Rowe Schoolcraft, *Information respecting the history, condition and prospects of the Indian tribes of the United States: collected and prepared under the direction of the Bureau of Indian Affairs, per act of Congress of March 3d, 1847. Vol. 1.* (Philadelphia: Lippincott, Grambo, & Co. 1853).

¹⁶ Schoolcraft, *Information respecting the history, condition and prospects of the Indian tribes of the United States*, 567.

¹⁷ Circulars and questionnaires only functioned if they could reach remote locations. Thus, paper and the mail became an important medium and infrastructure of intellectual accumulation for the human sciences in the nineteenth century. Social theorists and human scientists of the mid-nineteenth century relied on data procured from Anglo or European correspondents who knew and lived among indigenous peoples and sent information back to theorists in writing. See Warner, *Letters of the Republic*; Henkin, *The Postal Age*.

¹⁸ Hannah Elizabeth Turner, “Information Infrastructures in the Museum: Documenting, Digitizing, and Practising Ethnographic Objects in the Smithsonian’s Department of Anthropology,” (PhD Dissertation, University of Toronto, 2015).

Curtis M. Hinsley, *Savages and Scientists: The Smithsonian Institution and the Development of American Anthropology, 1846-1910* (Smithsonian Institution Press, 1981);

project that followed from or built upon the paper technology's imagined use, and assure its relevance to Atlantic scholarly communities.

Before the expansion of the United States westward, linguistic scholars had studied Indian languages in local contexts, or with regionally circulated questionnaires.¹⁹ Lewis Cass, who negotiated treaties with local Indian communities in what became the Michigan territory of the Old Northwest, had established an epistolary network for sharing linguistic and ethnological information by 1823.²⁰ He later suggested that a Native American “synonymy,” or book of comparative synonyms, could be produced if enough data from a diversity of Indian tribes could be centralized and archived for analysis.²¹ Cass's accumulative impulse was not philanthropic; in fact, it was directed *against* compassion for Indians and any notion of Indian equality with white civilization (Cass would later go on to direct President Andrew Jackson's destructive policy of Indian Removal). Cass had conceived of his project for comparative language accumulation to reveal the more base and degenerate characteristics of Indian language—a response to missionary John Heckewelder and philologist Peter S. Du Ponceau's appreciation for the beauty and complexity of Indian languages, controversial for the day, which had in turn stimulated philanthropic efforts from sympathetic Anglos in the eastern States.²² The fissure between Cass's insistence that Indians were “feeble-minded” and the romantic view of Heckewelder and Du Ponceau lasted well into the nineteenth century; the argument directed philological accumulation, although to call such language-gathering unbiased would be farcical. Both sides of the debate, nonetheless, classified human difference in a Linnaean taxonomic scheme. The effect of such classification allowed for racial hierarchies and placed Indians (and other non-white) peoples closer to animals, as more integrated into the natural environment.²³

Albert Gallatin, the well respected former statesmen and public intellectual, encouraged continued comparative philological study across the expanding United States. Gallatin convinced the War Department to circulate a philological “Table” to Indian superintendents, agents, missionaries, and other relevant functionaries living near Indian communities. Gallatin found,

¹⁹ For early linguistics in North America, see Harvey, *Native Tongues*; Wolfart, “Notes on the early history of American Indian Linguistics.”

²⁰ Lewis Cass, *Inquiries, Respecting the History, Traditions, Languages, Manners, Customs, Religion, &c. of the Indians, Living Within the United States* (Sheldon & Reed, 1823).

²¹ Lewis Cass, “Indians of North America,” *North American Review* 22, no. 50 (1826): 53-119.

²² Sean P. Harvey, “‘Must Not Their Languages Be Savage and Barbarous Like Them?’: Philology, Indian Removal, and Race Science,” *Journal of the Early Republic* 30, no. 4 (2010): 505-532; Andresen, *Linguistics in America*.

²³ Pratt, *Imperial Eyes*; Conn, *History's Shadow*.

however, that responses to his questionnaire were “wholly incompetent,” in part because his inquiries (developed with Du Ponceau) prompted confusing exchanges with informants. Moreover, few correspondents had the training to adequately undertake the accumulative task.²⁴ His compromised data notwithstanding, Gallatin published a comparative analysis of Indian languages in 1836 as “A Synopsis of the Indian Tribes within the United States east of the Rocky Mountains, and in the British and Russian Possessions in North America.” The geographic specificity, omitting much the American West and all of the Southwest, pointed to the trouble of engaging Indian informants in the wake of the 1830 Indian Removal Act.²⁵

Henry Rowe Schoolcraft followed Gallatin with his own comparative analysis of Indian languages in 1847, as did the linguist Edward Everett Hale in 1848.²⁶ These came on the heels of the US Congress’ charge for a comprehensive report of Indian “history, present condition, and future prospects.” These studies produced knowledge useful not only for the advancement of linguistics but also for US governmental entities charged with managing relationships with Indian communities, helping to open communication and provide intelligence for Anglo settlers, business interests, and the military. Here, knowledge of Indian languages served scientific pursuits and as a strategic body of knowledge for future legal and extra-legal engagement with Indian groups. Some of these analyses, such as Hale’s, attempted to maintain an unbiased position of analysis, resembling a presentation and explanation of data acquired. But others, including the influential Schoolcraft, explicitly gave credence to the Cass argument about Indian peoples’ unrefined mentality. Schoolcraft’s ultimate conclusion was that Indian language produced a fixed mindset that was incompatible with American norms and was intrinsically inferior, and thus needed to be replaced with the English tongue.²⁷

With flurries of data circulating in this emerging ethnological network, a problem of standardization arose, particularly with regard to linguistic data. Linguists of the mid-nineteenth

²⁴ Harvey, ““Must Not Their Languages Be Savage and Barbarous Like Them?””.

²⁵ Albert Gallatin, *A synopsis of the Indian tribes within the United States east of the Rocky Mountains, and in the British and Russian possessions in North America* (Arx Publishing, LLC, 1836).

²⁶ Schoolcraft, *Inquiries*; Horatio Hale, *United States Exploring Expedition During the Years 1838, 1839, 1840, 1841, 1842: Ethnography and philology*. (C. Sherman, 1846). Smithsonian secretary Joseph Henry developed a circular later, in 1867, following the model provided by George Gibbs in 1863. The Smithsonian’s Spencer Baird also prepared a circular with a natural history focus; see Spencer Fullerton Baird, *General Directions for Collecting and Preserving Objects of Natural History* (Smithsonian Institution, 1848).

²⁷ Henry Rowe Schoolcraft, *The American Indians: Their History, Condition and Prospects, from Original Notes and Manuscripts* (Wanzer, Fott, 1851). For philology’s contributions to race science, see Harvey, ““Must Not Their Languages Be Savage and Barbarous Like Them?””

century found reams and reams of inaccurate or slipshod language documentation. A scholar could look to a number of texts about, for instance, Iroquois language, but would find dozens of orthographic idiosyncrasies. Correspondents from the frontier, moreover, were often actors outside of the typical scientific milieu; while the field sciences of geology and natural history had explored the West, the region lacked institutional structures for assessing authority and credibility in local scientific practices.²⁸ The problem was, as Henry Rowe Schoolcraft lamented, that “the vague vocabularies of tourists and travelers of the picturesque or galloping class” lacked orthographic standards (conventionalized spelling and character use).²⁹ Language study and orthographically standardized accumulation (unsurprisingly) had not been the primary concern of the well-heeled travelers and explorers, not to mention the trappers and mappers of the nineteenth century. Schedules and questionnaires designed by philologists, then, aimed to make language accumulation a primary objective, rendering it a studied and conscientious pursuit.

Linguistic Schedules and Philological Standardization

The linguistic circular launched into orbit in the middle of the century as a paper technology to help address the problem of standardization and to give a structure to the forms of data accumulation (see Image 1).³⁰ This standardization effort was largely housed in the Smithsonian Institution and its ethnologists and philologists, as disciplinary anthropology had not yet been established in the nation’s universities.³¹

Linguistic circulars were printed on paper and included blank spaces for data inscription, which were called “schedules.” The nineteenth-century ethnographic “schedule” descended not from time-tables or datebooks, with which we associate the word “schedule” today, but from supplementary explanatory documents attached to legal decrees and wills. Schedules were distinguished from narrative text for their use of tabular entries, classificatory lists, and matrices,

²⁸ As the railroad entered the West in the last quarter of the nineteenth century and universities emerged in western states and territories, the field sciences did gain institutional grounding. See Vetter, *Field Life*. Ethnology and the anthropological sciences, however, remained without a major institutional foot in the West until the early twentieth century.

²⁹ Schoolcraft, *Inquiries*, 4.

³⁰ For prior language accumulation projects, including state calls for documentation of the world’s languages, see Harvey, *Native Tongues*.

³¹ Nancy J. Parezo, “The Formation of Ethnographic Collections: The Smithsonian Institution in the American Southwest,” *Advances in Archaeological Method and Theory* 10 (1987): 1-47; Hinsley, *Savages and Scientists*; Daniel Goldstein, “‘Yours for Science’: The Smithsonian Institution’s Correspondents and the Shape of Scientific Community in Nineteenth-Century America,” *Isis* 85, no. 4 (1994): 573-599.

and began to appear in English legal documentation practices in the 16th century.³² As most schedules were more than a single page, they were bound at the top or along the left margin, like a book, to keep the pages in order and the inquiries in sequence. Because in its most basic format the ethnographic schedule featured a categorized list of elements, the actual form itself was easily replicable, and at times ethnographers inscribed the list into their own notebooks when they returned the completed schedule to its sender.³³

The well-traveled schedules of the 19th century included introductions and detailed instructions on how words should be rendered. Schedule-creators presumed a level of education and drew on frequently used Latin and French words as comparative vocabulary terms. At the same time, respondents were not expected to master complex symbols they may have never encountered in reading. Schoolcraft's vocabulary guide simplified orthographic symbols by minimizing the variety of accents and vowel marks. He most desired that "the syllables of Indian words be uniformly denoted, or separated by a dash or space," so that common English sounds could articulate the pronunciation, no matter how long or agglomerated (blended together) the words might be.³⁴ Although transcriptions could run rather long in this format, Schoolcraft recognized that neither the vocabulary collectors nor the Indian interpreters with whom they worked were likely to transform into philologists. The schedule was a technology that simplified language and simple correspondences to ensure the highest integrity of the data accumulated.

The aim of schedules was to document a variety of languages for philological comparison and analysis. For Schoolcraft, circulars and their schedules contributed to science in their accumulation of fundamental linguistic elements. Nineteenth century philology primarily analyzed etymology, the origin of words and their transformation over time. In the North American context, etymology was regularly applied to a goal shared by Schoolcraft and others

³² "schedule, n.," *OED Online*, March 2017 (Oxford University Press).

³³ There is a rich body of literature techniques of data entry in the history of science. For listing and tabulating, for example, see Lorraine Daston, "Super-Vision: Weather Watching and Table Reading in the Early Modern Royal Society and Académie Royale des Sciences," *Huntington Library Quarterly* 78, no. 2 (2015): 187-215; Staffan Müller-Wille and Isabelle Charmantier, "Lists as Research Technologies," *Isis* 103, no. 4 (2012): 743-52; Anke Te Heesen, "Accounting for the natural world: double-entry bookkeeping in the field," in Londa Schiebinger and Claudia Swan (eds.), *Colonial Botany: science, commerce, and politics in the early modern world* (Univ. of Pennsylvania Press, 2005), 237-251. For notetaking and the use of paper tools for scientific inquiry, see Lorraine Daston, "Taking Note(s)," *Isis* 95, no. 3 (2004): 443-48; Ann Blair, "Note Taking as an Art of Transmission," *Critical Inquiry* 31, no. 1 (2004): 85-107; Ann Blair, "The Rise of Note-Taking in Early Modern Europe," *Intellectual History Review* 20:3 (2010): 303-316; Hess and Mendelsohn, "Case and Series"; Klein, *Experiments, Models, Paper Tools*. Turner, "Information Infrastructures in the Museum," investigates the evolving paper technologies of ethnographic inquiry within the Smithsonian specifically.

³⁴ Schoolcraft, *Inquiries*.

interested in Native American languages: to establish (and understand) the link between Asiatic languages and North American indigenous languages. To this end, Schoolcraft did not seek simply the rendering of various words. The real scientific evidence for such was to be found in the radicals of language.³⁵ If philology aspired to a structural analysis of human groups, however, it would need accurate, fine-grained data about Indian languages, and the linguistic circular, even with its faults, became the major vehicle for accumulating such information.³⁶

For Schoolcraft, linguistic analysis of language radicals required, of course, standardized orthography and extensive examples that accurately represented Indian languages. But in his wake, another mode of linguistic analysis was developed by Lewis Henry Morgan, in which a language's terms of extended family relations (father's brother, maternal great-grandmother, sister's daughter) could be used to suggest historical connections between diverse populations of the world.³⁷ Morgan had subscribed to the historical hypothesis of Schoolcraft and other philologists of the day: if language terms could be gathered from the far-flung Asiatic and Amerindian peoples, similar grammatical structures might be found between them; from there, shared historical connections may be drawn.³⁸ Contrary to etymological analysis, Morgan argued that human groups could be traced by their use of kinship terminology, wherein disparate groups that used similar modes of reckoning family relations suggested closer affinity before migrations around the world. Morgan developed a set of schedules with extended kinship terminology, about which the fieldworker would ask his or her native informant.³⁹

³⁵ See Thomas R. Trautmann, *Aryans and British India* (Yoda Press, 2006) for the influence of Sir William Jones, whose emphasis on grammatical roots in language study came to dominate American linguistics as philology.

³⁶ On the history of philology in the United States, see Andresen, *Linguistics in America*; Harvey, *Native Tongues*.

³⁷ For Morgan's life in relation to his thought, see Trautmann, *Lewis Henry Morgan and the Invention of Kinship*; see also Daniel Noah Moses, *The promise of progress: The life and work of Lewis Henry Morgan* (University of Missouri Press, 2009). For an exploration of Morgan's identity in relation to imagined American Indian life, see Philip J. Deloria, *Playing Indian*. (Yale University Press, 1998).

³⁸ Lewis Henry Morgan, *Circular Letter in Regard to the Possibility of Identifying the Systems of Consanguinity of the North American Indians with That of Certain Peoples of Asia* (1859); Bieder, *Science Encounters the Indian*, 217-219; Folwer, "Notes on Inquiries in Anthropology." Morgan's British counterpart, Edward B. Tylor, also created tabulated data for some 350 kinship systems in Edward B. Tylor, "On a method of investigating the development of institutions; applied to laws of marriage and descent," *The Journal of the Anthropological Institute of Great Britain and Ireland* 18 (1889): 245-272.

³⁹ The easiest way to understand Morgan's relational-terminological kinship is to think about maternal and paternal cousins. In Amero-European kinship, cousins are not terminologically distinguished from one another. In other cultures, however, "cross" cousins (cousins from a parent's siblings of a different sex, as in a father's sister's children) may be termed differently from "parallel" cousins (cousins from a parent's same-sex sibling, as in a father's brother's children). Other variations of relational terminology exist, for instance, where matrilineal cousins are considered "closer" in relation to patrilineal cousins.

To study kinship terminology and its variations, Morgan set in motion a project of comparative ethnology global in scope. When his paper technology returned to him and he pondered and puzzled over the data, Morgan found that it was not similar words or roots that indicated a relationship between Native Americans and East Asian peoples. Instead, as he argued in *Systems of Consanguinity and Affinity of the Human Family* (1871), group relationships could be gleaned through similar para-linguistic structures of kinship terms—how patrilineal cousins referred to each other as compared to matrilineal cousins. While under European kinship systems both pairs of cousins were blood-related (what he called consanguine), Morgan mapped an alternative kinship structure based in relationships of marriage and gender (affinity, as between matrilineal “clans”).

By using pre-printed forms in which the complexities of familial terms need not be thought-up but merely filled-in, Morgan was able to obtain information even from substandard orthographic documentation. While orthographic standards were still important, Morgan ultimately did not need rely on the subtleties of sound, between *b* and *v*, and how these were written down on paper. Instead, fieldworkers enrolled by Morgan would ask after extremely complicated forms of relationality: “My Father’s Brother’s Great-Grand-Daughter (said by a female).” The accumulator, to put it simply, needed to chart how terms of relationship differed, which would establish how affinity was reckoned—for example, whether a group gave preference to matrilineal or patrilineal lines.

Morgan’s schedules standardized language terms for acquisition and rendered them into sets of discrete terms. Through atomized kinship terms, Morgan was able to compare terms across different languages and develop general conclusions about regional or racial patterns that emerged from the tables of data. Morgan initially relied on philology’s comparative method. As Thomas Trautmann has shown, however, Morgan eventually sidelined philological analysis when his interest in developments in geology (notably the events of a “time revolution” that deepened the earth’s chronological scope) led him to reframe his historical analysis into an evolutionary one, in which he sought to develop not merely distant historical connections but also an analytical paradigm of kinship and family relationships and their developmental stages along the path to civilization.⁴⁰

⁴⁰ Trautmann, *Lewis Henry Morgan and the Invention of Kinship*; Thomas R. Trautmann, “The revolution in ethnological time,” *Man: a monthly record of anthropological science* (1992).

Morgan's circular of schedules promoted systematic and applied modes of inquiry, as well as showed that paper-based inquiry could be successful in a global network of correspondents (see Image 2).⁴¹ By prompting his field agents to ask an Indian group of its terms for "Great-Great-Grandfather (Maternal)," Morgan showed that a rigorous form of directed inquiry could produce data that non-Native fieldworkers, or any outsider for that matter, might easily miss.

Morgan's schedules had initially focused on a historical problem of relatedness within philology, and this research area structured the field labor the schedule enrolled. By contrast, George Gibbs's 1863 "Comparative Vocabulary" was designed in a more generalist frame, with the aim of accumulating linguistic information about North American Indian groups and compiling these vocabularies in a centralized archive in the Smithsonian Institution. With such data, one could obtain ethnological knowledge useful for diplomacy, trade, or social statistics. Additionally, by accumulating data and housing it in a central archive, the institutional salience of ethnology would be improved bit by bit, as circulars piled up.⁴²

Gibbs had developed an interest in Indian languages of the Pacific Northwest while working on the Pacific Railroad Survey of the 47th and 49th parallels and the Northwest Boundary Survey in the 1850s and early 1860s. In addition to his own extensive notebooks, Gibbs thought to circulate a "folio paper of three leaves," consisting of 180 terms that could be filled in by people knowledgeable with (and tolerated within) local Indian communities. This was based on a prior paper technology used by John Russell Bartlett, who, during his time as United States Boundary Commissioner, had collected language terms from Western and Southwestern Indian communities between 1850-1853. Bartlett's typical vocabulary consisted of 180 words, composed of words of relation, parts of the body, numbers, and climatic and geographical terms.⁴³ In the aftermath of his field work, Bartlett had systematized his own collection and engaged in comparative work within his own archive of Indian language terms. Gibbs knew Bartlett's work; from it, he modestly expanded his circular.

⁴¹ Indeed, Morgan's thesis on kinship reflected the trans-Atlantic comportment of ethnology of the mid nineteenth century. His British counterparts, Edward Tylor, Henry Maine, and Herbert Spencer, also prepared treatises with global implications. Schedules and far-flung correspondence were crucial to keeping such scholars abreast of novel practices and tongues around the world.

⁴² For Gibbs as an institution-building in the Smithsonian, see Turner, "Information Infrastructures in the Museum."

⁴³ See, for instance, J H [sic] Bartlett, "Miscellaneous vocabularies of 32 different tribes," NAA. Later, when Gibbs acted as a linguist in the Smithsonian, he acquired Bartlett's vocabularies for his own studies of Indian languages. There are many other individual documents by Bartlett, many with supplemental notes by Gibbs.

Gibbs circulated his questionnaire to “such persons as, in his judgment, were competent to furnish the material desired, and many of them, filled or partly filled, were returned to him,” as an 1881 bibliography of collected schedules put it.⁴⁴ Such success no doubt inspired the Smithsonian, Gibbs’s employer, to work toward a general circular that could instruct and provide the paper technology to capture terms from all Indian communities under US jurisdiction.⁴⁵ In 1863, the Smithsonian published Gibbs’s collection project as *Instructions for Research Relative to the Ethnology and Philology of America*, which included the “Comparative Vocabulary.” The “Comparative Vocabulary” was pre-formatted with English, Spanish, French, and Latin vocabulary words on the verso (left) page; the recto (right) page was blank, a space for the fieldworker to insert indigenous language terms.⁴⁶ Gibbs’s 1863 general linguistic table included 211 terms, including gender and kinship terms, names for common flora and fauna, numbers, and a slew of common verbs. Joseph Henry, the Smithsonian Secretary at the time, announced Gibbs’s circular and asked for “coöperation in this object of all officers of the United States Government, and travelers or residents who may have it in their power to render any assistance.”⁴⁷ Instead of sending hundreds of ethnologists, the thinking went, why not simply enroll the labor of qualified, educated people already present in the “wilderlands” of the American West? By doing so, the Smithsonian could also set the standard for terms accumulated, aligning the universalistic imperative to the vagaries of fieldwork.⁴⁸

The 1863 document, including the “Comparative Vocabulary,” was given to members of Lieutenant Wheeler’s expedition for survey and exploration west of the 100th meridian. As a small schedule, fieldworkers could fill it in an afternoon, talking through a translator (at times cycling through a multitude of languages) with an indigenous informant. Total accuracy of linguistic terms could not be expected, and at times the only recourse to capture information about a specific tribal language might be to use information given by outsiders such as fur

⁴⁴ Pilling, “Catalogue of Linguistic Manuscripts.”

⁴⁵ Pilling, “Catalogue of Linguistic Manuscripts.” See also George Gibbs Papers, circa 1850-1853, 1857-1862, Smithsonian Institution Archives (SIA).

⁴⁶ Gibbs, *Instructions*.

⁴⁷ Henry quoted in Pilling, “Catalogue of Linguistic Manuscripts.”

⁴⁸ See the collection of essays on “Lay Participation in the History of Scientific Observation” in *Science and Context* 24, no. 2 (2011). For the use of local labor for scientific activities in the American West, see Jeremy Vetter, “Cowboys, scientists, and fossils: The field site and local collaboration in the American West,” *Isis* 99, no. 2 (2008): 273-303; Jeremy Vetter, “Lay observers, telegraph lines, and Kansas weather: The field network as a mode of knowledge production,” *Science in Context* 24, no. 2 (2011): 259. The role of “lay expertise” in scientific production has long been a concern in the history of science and technology. For a recent discussion of the differences between “residential” and “cosmopolitan” knowledge, see Robert E. Kohler, “Finders, Keepers: Collecting Sciences and Collecting Practice,” *History of Science* 45 (2007): 428-54.

traders, missionaries, or other Indians. An appointed schedule-filler was undoubtedly beset with other tasks during the expedition; there were times that a table was filled by a doctor or secretary who only put in the most basic effort to check off tasks, with little conscious reflection on the source, phonetics, or need for accuracy.⁴⁹

As previously noted, government projects were common in the American West during the 19th century, and these outfits could gather ethnographic information on behalf of human science, as a part of the requisite information needed to control and dominate the “untamed” spaces of the frontier. Geological, military, and exploration surveys featuring narrative ethnographic depictions, on the one hand, as well as tables, almanacs, and censuses, on the other, were all components of US scientific and governmental management. Military memoranda and governmental surveys reported ethnographic information.⁵⁰ As structured bureaucratic entities, government-sponsored expeditions in the West specified the scope of documentation—which included ethnographic documentation. Data-gathering labor could be divided within an expedition, and an individual collectors’ role dictated by a manual or set of instructions provided by the bureaucracy that set the exploration in motion.⁵¹ And, because Indians were consistently considered part of the “natural” world, ethnological questions could be derived from instructions for information accumulation relevant to natural history.⁵²

Informal ethnological knowledge had long been produced by settlers, travelers, traders, and other people not attached to large-scale bureaucratic systems. As settlement of the West began following the Louisiana Purchase, a genre of western travel literature emerged from Americans on the frontier. Travel narratives in which the writer engaged with indigenous populations frequently documented these populations to some extent, with varying degrees of accuracy and fancy.⁵³ (Non-Anglos such as the Spanish-speakers in the Southwest, and the francophone and russophone communities in the Northwest, also reported their experiences with indigenous

⁴⁹ Numerous schedules from the Wheeler survey are contained in Manuscript 1870, NAA.

⁵⁰ Don D. Fowler, *Cleaving an Unknown World: The Powell Expeditions and the Scientific Exploration of the Colorado Plateau* (University of Utah Press, 2012); Frankel, *States of Inquiry*.

⁵¹ For studies of surveys in the American west, see Goetzmann, *Exploration and Empire*; Bartlett, *Great Surveys of the American West*.

⁵² For an example of a mid-century natural history manual produced by the Smithsonian Institution, see Baird, *General Directions*. Ethnologically specific manuals, especially regarding object accumulation, were more common later. For an influential example, see Otis T. Mason, *Ethnological Directions Relative to the Indian Tribes of the United States* (United States National Museum, 1875).

⁵³ For literature and the perception of the American West conveyed through narratives, see Richard Slotkin, *Regeneration Through Violence: The Mythology of the American Frontier, 1600-1860* (Wesleyan University Press, 1973).

populations in writing.) Most commonly, writers included sections in their memoirs describing Native “manners and customs,” a sort of illustration of Others on the frontier and how their ways converged with or diverged from US norms. Though often romantic or indicating deep racial bias, travelogues were essential pieces of data to emerging anthropological ethnography; when data was scarce about indigenous populations in the greater American West, any information from Anglo travelers was potentially useful for ethnologists.⁵⁴ While travel narratives did follow conventions—common descriptive formulae from which another scholar could distill into generalizable and universal categories of social being—the story’s contents followed the felicities of travel itself. Data about Indian communities was thus anchored to the life history of the particular settler or trader. This anchoring effect also arose when men and women living in the west sent bits of ethnological data to scholarly or government institutions.

That is not to say that schedules were not filled with care and intellectual dedication. Patrick MacElroy, an interpreter at the Indian Agency in Cimarron, New Mexico, completed Gibbs’s schedule for Jicarilla Apache in 1875 and attached an extended preface. MacElroy celebrated the Jicarilla’s language for its clarity of expression and internal coherence: “For almost every sensible object around them the [Jicarilla] Apaches have in their vernacular a definite corresponding term and for any strange new object presented to them they prefer to make a new term for expressing such object out of the abundant resources of their own language rather than adopt the expression of foreign idioms.” MacElroy had clearly reflected on philological method and suggested that further study of Apache would reveal structural similarities to European languages.⁵⁵

While used on Government surveying expeditions and within the Indian Agencies, Gibbs’s schedule could enroll virtually anyone as a fieldworker, provided they had contact to an Indian community and the ability to record language phonetically. But Gibbs’s schedule did not relegate the collector to a tiny answer space; the full blank recto page also allowed for additional commentary. Celeste Willard, the wife of a lieutenant stationed at Fort Union, New Mexico, completed a Navajo vocabulary based on Gibbs’s schedule, which she returned to the

⁵⁴ Examples of travel narratives with “manners and customs” portions include W. W. H. Davis’s *El Gringo; or, New Mexico and Her People* (1857); Gregg, *Commerce of the Prairies* (1844). The goal of “manners and customs” ethnography was not to understand the practical systems of community of Others but to assume fault with them. Many travelogues and similar documents primarily traded on the thrill of reading about difference, and secondarily (if unconsciously) on the caricatured preservation of a place that was not yet modern. See also, *Regeneration through Violence*

⁵⁵ Manuscript 116, NAA.

Smithsonian in 1869. Willard made additional ethnographic notes at certain points in the table when language terms failed to resonate. For the “spring” and “summer” entries, for instance, Willard wrote “the seasons are spoken of as when the grass is so high, when the corn is ripe, etc. etc.”⁵⁶ In addition to her marginal notes, she included a list of “Additional Navajo Words” that featured items of trade, food, and regionally specific flora. With correspondents such as Willard, Gibbs’s schedule proved able to enlist laypeople in the American West to do ethnological work for the Smithsonian and provided her a manageable set of elements to accumulate—data which would benefit human science, but also might diplomatically improve her own life among, and interactions with, the Navajo bands nearby.⁵⁷

Schedules filled by the likes of MacElroy and Willard siphoned linguistic information from Indian communities in situ to Washington. If schedules were compiled in one place, philologists could work on different constellations of languages; they could experiment and group language families; they could chart terminological shifts; they could standardize an official dictionary for a given dialect. But Gibbs’s schedule was also short; it could be filled in an afternoon by a traveler passing by an Indian community who could obtain a local translator. And, while Celeste Willard interviewed actual Navajos to fill her schedule, Gibbs’s circular was short enough that a trader or missionary claiming knowledge of an indigenous language could fill out most of the schedule on behalf of an Indian community. In short, the schedule could be fudged, its authenticity never certain and the filler’s credibility never assured. Its flexibility and simplicity meant Gibbs’s schedule was an imperfect object for exhaustive philological accumulation, but a good start for systematic, standardized gathering of ethnographic data and building an ethnological institution to develop nascent American anthropology. Gibbs’s schedule suited the general aims of young institutions like the Smithsonian, interested in building their archives of data for future researchers and raising the institution’s scholarly profile, and it served as a stabilizing document for the nascent practice of scientific ethnographic fieldwork.

The Smithsonian, which had printed and circulated both Gibbs’s and Morgan’s schedules, sought an update to the schedule in the 1870s.⁵⁸ As director of ethnology at the Smithsonian,

⁵⁶ Celeste N. Willard, “Navajo Vocabulary..., May 1869,” Manuscript 875, NAA.

⁵⁷ Celeste N. Willard, “Navajo vocabulary and autograph of Chief Delgadito May, 1869,” NAA.

⁵⁸ For a recent media history of the Smithsonian’s Department of Anthropology, see Turner, “Information Infrastructures in the Museum.”

John Wesley Powell set out to expand on the manual and schedule blanks provided by Gibbs's 1863 *Instructions for Research Relative to the Ethnology and Philology of America*. Powell's experience in ethnology had come from his field expeditions with the USGS. A former US Army Major, Powell headed a series of expeditions around the watersheds of the Green and Colorado Rivers, collecting geological and ethnological information. Notably, in 1869, and despite having only one arm, Powell led his crew through the rough waters of the Colorado streaming through the Grand Canyon, the first such navigation by white men.⁵⁹ But while navigation and geological survey were major accomplishments, Powell himself was captivated by Indian communities that lived near and relied on the Colorado River. It was in these encounters that Powell began collecting objects and information from Hopi, Pueblo, Ute, and Paiute groups in the region.

After his Western excursions, Powell began to work in the Smithsonian under Secretary Spencer Baird. Baird shared Powell's interest in Indian ethnography and had directed western explorers to make collections of Indian objects, especially those that were being replaced by Anglo-American goods.⁶⁰ While object collection continued, Powell also continued the accumulation of language data from Indian groups, especially from Southwestern indigenous communities.⁶¹ To facilitate further language study, Powell developed paper tools for accumulation that he had lacked in his own expeditions. Powell understood the need for scientific credibility through the accumulation of systematic and broad-reaching information about American Indian groups. His book-length manual of ethnology, *Introduction to the Study of Indian Languages—with Words, Phrases, and Sentences to be Collected*, a volume of instructions and schedules for data accumulation, directed the field of American anthropology for two decades following its publication in 1877 (including a modestly updated second edition in 1880).⁶² His scholarly credentials established through the ethnographic and linguistic

⁵⁹ For a study of Powell's life in the field, see Donald Worster, *A River Running West: The Life of John Wesley Powell* (Oxford University Press, 2000).

⁶⁰ Mason, *Ethnological Directions*; Parezo, "The Formation of Ethnographic Collections;" Hinsley, *Savages and Scientists*.

⁶¹ For work on material collection in the Southwest, see Nancy J. Parezo, "Cushing as part of the team: the collecting activities of the Smithsonian Institution at Zuni," *American Ethnologist* 12, no. 4 (1985): 763-774; Parezo, "The Formation of Ethnographic Collections;" Hinsley, *Savages and Scientists*. On the desiderata of object collection and the institution's organizing logic, see Turner, "Information Infrastructures in the Museum." Departments aligned with the Smithsonian also collected human remains; see Samuel J. Redman, *Bone Rooms: From Scientific Racism to Human Prehistory in Museums* (Harvard University Press, 2016).

⁶² John Wesley Powell, *Introduction to the Study of Indian Languages: with words, phrases and sentences to be collected* (US Government Printing Office, 1st edition 1877, 2nd edition 1880).

accumulation project, when the Bureau of Ethnology was established in 1879 John Wesley Powell became its founding director.

For the *Introduction to the Study of Indian Languages*, Powell took the basic tabular entries from Gibbs's work but expanded the serial inquiries considerably. Powell removed the comparative French, Spanish, and Latin terms and instead sought to list a "series of explanations of certain characteristics almost universally found by students of Indian languages—the explanations being of such a character as experience has shown would best meet the wants of persons practically at work in the field on languages with which they are unfamiliar."⁶³

Powell's circular pushed the fieldworker to collect ever more fine-grained terms. The recipient could still be a conveniently-situated layperson, but Powell's volume of schedules presumed that the linguistic accumulator was not merely a passing traveler who fancied a contribution to scientific knowledge. Instead of comparative languages, Powell reserved blank fields for linguistic terms and "remarks," in which the fieldworker might comment on some curious element or nuance that a literal translation could not capture. Powell's list of terms was paired with the occasional full-page blank, where the collector was asked to give an account (for example) of mortuary customs. While this did not necessitate direct observation of discrete customs, such prompts could attune the fieldworker to various events of which they might take note and subsequently craft into an ethnographic description.⁶⁴ As paper tools for ethnology expanded and asked more nuanced information, these documents directed accumulators' efforts with increased specificity while also building-in and safeguarding an increased credibility to the data collected.

Powell and bureau staff organized the schedule categories to facilitate sequential data collection. As Smithsonian linguist James Pilling noted, "it is believed that the system of schedules, followed seriatim, will lead the student in a proper way to the collection of linguistic materials; that the explanations given will assist him in overcoming the difficulties which he is sure to encounter; and that the materials when collected will constitute valuable contributions to philology."⁶⁵ Powell acknowledged that comparative ethnological questions could be better

⁶³ Pilling, "Catalogue of Linguistic Manuscripts."

⁶⁴ Circulars continued to be sent from the Bureau of American Ethnology and the Smithsonian Institution. See, for example, James Mooney, "Circulars and other material concerning Indians and traces of Indians in Virginia, Maryland, Delaware, and North Carolina 1889-1912," NAA; J R Bartlett, "Miscellaneous vocabularies of 32 different tribes," NAA.

⁶⁵ Pilling, "Catalogue of Linguistic Manuscripts."

served by exhaustive lists, directed by ethnologists that had previously struggled with inconsistent entries. Powell's manual also served the ends of philologists, focused as they were on the structural elements of language, in its expanded inquiries that asked for prefixes, suffixes, and grammatical tenses. Thus, the *Introduction* furthered disciplinary stabilization and standardized data accumulation efforts ongoing in ethnology, propping up both anthropology and philology.

In his *Introduction to the Study of Indian Languages*, Powell combined the disciplinary and institution-building aims produced by Gibbs's questionnaire and the directed line of inquiry in Morgan's—including the latter's attention to kinship details of an extended family. Powell included a visual kinship chart in his 1880 edition, which referenced the individual terms in the schedule, and could aid the fieldworker with comprehending the confusing lines of descent that kinship-oriented philologists desired. While Powell's schedules did the sort of archival collecting that would bolster philology, his kinship sections provided more data for internal social relationships in Indian groups, an area that buttressed Morgan's theory that a culture's evolutionary status was dependent on their development of political institutions, from simplified social organization to a more formalized “political governance.”⁶⁶ Significantly, too, Powell's manual charted out an expanded exploration of Indian terms of relationship—not simply kinship terms, but also the Indian conceptualization of categories of objects and ideas. Powell's work, in other words, sought the interior of the Indian mind, from indigenous explanations of cause-and-effect to the logic and sequence by which they taught their children about the world.

The durable, hard-bound *Introduction to the Study of Indian Languages* was meant to be brought into the field. The 1879 Bureau of Ethnology expedition to the Southwest, led by James Stevenson and including future ethnologists Matilda Coxe Stevenson (his wife) and Frank Hamilton Cushing, used Powell's schedule to orient their studies. During the next several years in the field, the Stevensons spent much time in artifact collection, items which were also inscribed into tabular registers designed specifically for the expedition, but they also filled in Powell's questionnaire for a number of Southwestern communities they encountered.⁶⁷

⁶⁶ For social evolutionism, see Stocking, *Race, Culture, and Evolution*; Stocking, *Victorian Anthropology*. For social evolutionism in relation to American racial theory, see Baker, *From Savage to Negro*; Dain, *A Hideous Monster of the*; Smedley, *Race in North America*; Stanton, *The Leopard's Spots*; Horsman, *Race and Manifest Destiny*; Gould, *The Mismeasure of Man*.

⁶⁷ Matilda Coxe Stevenson, “[Zuni] Vocabulary, December 1882,” Manuscript 875, NAA. Matilda Coxe Stevenson, “Avesu-Pai vocabulary, October 1885,” Manuscript 1114, NAA.

Powell's appropriately titled *Introduction* was a manual to instruct fieldworkers on the study of Indian languages.⁶⁸ This manual and its series of schedules attuned novice fieldworkers to ethnological questions of the day, given by the categories for collection and their ordering in the manuscript. Alexander Stephen, a former miner and trader who lived near Hopi and Navajo communities, acquired a copy of *Introduction to the Study of Indian Languages* around 1888. In its tables, Stephen inscribed Hopi and Navajo words alongside their English counterparts; over the course of several years, he continued to add linguistic and cultural information to his copy of Powell's schedule.⁶⁹

As an inscription technology, the tables of the *Introduction to the Study of Indian Languages* could be replicated in the field by hand to produce the same organized results of a printed schedule. Frank Russell, studying Jicarilla Apache in 1898, resorted to copying by hand the numbered series of inputs in Powell's schedule, his bound volume unavailable for whatever reason. He wrote in his notes, "Numbers indented at left margin list have been added to the list as given in the 'Introduction.'" In addition to language terms listed according to Powell's plan, Russell wrote an ethnographic description of a "medicine feast," captured a number of Apache stories, and made "miscellaneous notes" on Jicarilla gambling debts, widow treatment, evil spirits, the afterlife, and a host of other items shared by his informants.⁷⁰ Referencing and reproducing Powell's categorization system allowed Russell to capture information in an enumerated, standardized form, useful to other scholars, but he also stretched and extended ethnographic description outside of tabular inputs.

Bureaucratic Productions of Paperwork

The orbital pattern of the Powell's volume of schedules was that of a teardrop, like messages sent with homing pigeons: they were released into the field, to be filled, and were to be returned to their point of origin, the Bureau of Ethnology. Once the arc was completed, once a schedule-carrying pigeon returned, staff at the Bureau archived the documents according to

⁶⁸ While Powell had been influenced by Morgan's *Systems of Consanguinity and Affinity in the Human Family* for schedule-creation, he considered Morgan's later, more theoretical work on *Ancient Society* (1877) the guiding light for ethnological research of the day. Powell enshrined *Ancient Society* as a guiding principle for his fieldworkers in *Bureau of American Ethnology*. See Bieder, *Science Encounters the Indian*; Hinsley, *Savages and Scientists*; Carl Resek, *Lewis Henry Morgan, American Scholar* (University of Chicago Press, 1960).

⁶⁹ Stephen, "[Indian Languages] Notebook," Elsie Clews Parsons Papers, APS.

⁷⁰ Frank Russell, "Jicarilla Apache Vocabulary, 1898, 1302-A," NAA.

language family. 19th century circulars from Smithsonian departments, for example, were housed in physical archives and could be called on for reference by inquiring individuals.⁷¹ After the Smithsonian's founding in 1846, philologists in the institution had organized a catalogue of Indian vocabularies, which could be accessed by visitors to the Smithsonian Castle's library in Washington, D.C.⁷² Collections of language tables amassed across the circulars of Gibbs and Powell's book-length manual, in addition to other vocabulary lists submitted independently of the Smithsonian-produced schedules.

As schedules returned to the Bureau, they were processed by employees, labeled and sorted, and stored in the Smithsonian's archives. What emerged in the growing catalog of manuscripts—often labelled “linguistic” but also containing ethnographic data pertinent to a host of ethnological pursuits—was a register of categorized data about different Indian communities in North America.⁷³ The Smithsonian made use of its own pre-printed tables for language transcription and archiving, a synthesis of terms collated in a single document; redundant language schedules were to be compared and combined into a single language table, a stabilized ideal of a given Indian language.⁷⁴ Bureau philologists did further work of organizing language tables, smoothing and organizing terms, correcting and clarifying. For example, in an 1890 notebook, Albert Gatschet, a Bureau philologist, compiled language data from a variety of sources on Keres, a language of the Pueblo Indians, and organized them by regional dialect. In notebooks such as this, Gatschet further condensed linguistic information into a single source. Provided the language data was rendered on the official schedules of Gibbs or Powell, the transferal to a finalized, ideal-typical schedule was simple. In a model of “aperspectival objectivity,” the Bureau of Ethnology scholars extracted individual accumulator subjectivity and smoothed the resulting hybrid product, thereby rendering linguistic knowledge communicable and coherent.⁷⁵

⁷¹ Frankel, *States of Inquiry*.

⁷² For a list of schedules/vocabularies in 1880, see James Pilling's “Catalogue” inventory in the *First Annual Report of the Bureau of Ethnology*.

⁷³ While linguistic papers were archived and their contents condensed into generalized vocabularies, other fruits of ethnographic fieldwork—pottery, clothing, tapestry, and other material objects—underwent a different processing. Hannah Turner illustrates the cataloging of material culture artifacts in the National Museum (an arm of the Smithsonian Institution) as a process of typological sorting: objects descriptions were entered into ledgerbooks for reference, grouped by their functional category (in Anglo-American terms)—Navajo baskets with Pueblo baskets, for example. Turner, “Information Infrastructures in the Museum.”

⁷⁴ See, for example, Gibbs's entry of Apache words from a manuscript by N. S. Higgins into a pre-printed “Comparative Vocabulary” form (1866); Manuscript 157, NAA.

⁷⁵ For aperspectival objectivity, see Lorraine Daston, “Objectivity and the Escape from Perspective,” *Social studies of science* 22,

Information did not always arrive in standardized format, of course, and Bureau clerks did textual work to transfer linguistic data from such sources to the organized system represented in the schedule. Letters, personal diaries and small ethnological projects (such as vocabularies or illustrations of Indian groups) documented Anglo encounters with indigenous people, which might be useful to ethnologists. Personal diaries, for instance, captured daily processes, which involved many encounters with Indians from a variety of communities.⁷⁶ Two small leather-bound journals by Dr. John B. White found their way to the Smithsonian archives in the 1870s or 1880s. These journals contained notes on Apache language and customs, as well as the author's self-fashioned comparative vocabulary for local Yuman and Athabaskan dialects. From these volumes, a clerk prepared an extraction of Yuman vocabulary, and moved this data to another manuscript, to which the chief philologist Albert Gatschet added additional notes. With the Yuman language data extracted, these leather notebooks (their major content being Athabaskan Apache) could be grouped with other Athabaskan material.⁷⁷ As ethnological statements, the small bits of information of life among Indians contained in the personal diaries of Anglos supplemented the standardized and sanctioned paper technologies of accumulation.

But while linguistic distillation was a form of “smoothing” over the noise of different linguistic accumulators, the resulting product was not always a model of neat scientific data. Linguistic schedules were subject to additional writing after reception in the central repository. Scholars working on languages often wrote clarifying or contextualizing information on the sheets received. Gibbs, Gatschet, and Pilling all contributed their own marginalia to numerous linguistic schedules as they devised (and revised) systems for organizing the schedules in such a way that data may effectively be extracted from them. In some cases, vocabulary lists contained generations of scribbles that added, corrected, or contextualized Indian language terms, akin to

no. 4 (1992): 597-618. See also, Daston & Galison, *Objectivity*.

⁷⁶ Thales Haskell Diary, HLSC; Electus Backus Diary, HLSC.

⁷⁷ Incidentally, at times even the head philologist of the Bureau of Ethnology also abandoned the tabular schedule format for synthetic work. In several notebooks in the National Anthropological Archives, Gatschet appears to have done the intermediary work of sorting linguistic data between multiple sources. In the case of Southwestern communities like the Pueblo and Navajo, Gatschet and other philologists found that the diversity of working languages stymied any aspirations for a singular representative of “Pueblo” or “Navajo.” For example, Gatschet developed a notebook on Keres language in which he specified the dialects by individual Pueblo communities. Only by differentiating at this level could Gatschet correct and organize prior vocabularies that had been collected with diminished attention to details, such as those scribbled by the incidental, lay vocabulary-collectors of Lt. Whipple's expedition. Thus, Gatschet needed to compile notebooks that would allow a more sophisticated interpretation of previous schedules, a system that noted linguistic dialects of, say, Keres and would allow older reports to be brought into line with the work of more professional and accomplished linguistic accumulators. See Manuscript 499, NAA.

what Ursula Klein has called the “paper tools” that make sense of the maze of a scientific discipline.⁷⁸ Inside the Bureau of Ethnology, schedules became historical representations of language. But they also became living documents, an infrastructure that allowed ethnographic and linguistic analysis of individual indigenous groups, related communities or languages, and across vastly different social system.

The Study of Indian Opinions

With a range of ethnological data streaming into the Bureau of Ethnology, Powell found time to think deeply about the profession of which he was, for all intents and purposes, the figurehead. Powell spent his last years within the Bureau’s archives, developing “a system of classification designed to indicate the place of the American aborigines among the peoples of the earth.”⁷⁹ Powell used two major sources for his classification scheme: linguistic manuscripts accumulated by the Smithsonian and later the Bureau of Ethnology, and studies and accounts of Indian mythology. Eventually, over the course of the 1880s and 1890s, Powell articulated a vision for American ethnology as composed of five sciences: esthetology, technology, sociology, philology, and sphiology.⁸⁰

With the first four sciences in his plan for ethnology, Powell did not veer far from other scientific practices (meteorology, vital statistics) that relied on inductive data from formalized paper technologies. Esthetology was considered the science of arts and aesthetics and focused on the activities human beings participated in that gave them pleasure. Technology, the science of industry, homed in on the activities and technologies that facilitated social welfare, sustenance, shelter, commerce, and other activities essential to the maintenance of life. Powell’s version of sociology, the science of institutions, considered the role of government, law, and social organization, and charted the placement of societies on a developmental scale from savagery to barbarism, to monarchy, to republicanism (representative democracy).⁸¹ Philology, the science of

⁷⁸ For example, Bartlett’s Tiwa vocabulary from Taos contains both Gibbs and Gatschett’s handwriting. See Bartlett, “Tiwa vocabulary of about 180 words October 2, 1852,” NAA. For “paper tools,” see Klein, *Experiments, Models, Paper Tools*.

⁷⁹ John Wesley Powell, *Twentieth Annual Report of the Bureau of American Ethnology to the Secretary of the Smithsonian Institution, 1898-99* (Government Printing Office, 1903.)

⁸⁰ Daniel Brinton also developed a nomenclature system for anthropology which featured “sphiology,” or the science of opinions. See Daniel G. Brinton, “The Nomenclature and Teaching of Anthropology,” *American Anthropologist* 5, no. 3 (1892): 263-272.

⁸¹ Powell drew heavily on the scheme presented in Morgan’s *Ancient Society*, adding monarchy to stages of the development of social governance. Lewis Henry Morgan, *Ancient society; or, researches in the lines of human progress from savagery, through barbarism to civilization* (H. Holt, 1877).

expression, was charged with collecting material related to human physiological expressions (such as tiredness or happiness) as well as the expression of concepts and mentation through conventionalized body movements, oral language and sign systems.⁸²

Climbing these subfields, one finally reached “sophiology,” which Powell understood as both the “science of instruction” and the “science of opinion.” Sophiological study could entail anything that humans used to give explanation to the workings of the world: rituals and icons, ceremonies and secret societies, prayers and religions, science and experimentation. Further, sophiology also considered the instruction, transmission, and debate of beliefs, as well as their as classifications over time as errors or as truth by the “leaders of human thought” for a given community.⁸³ In other words, sophiology examined how beliefs were presented and maintained in a given community.

Powell initially struggled to articulate sophiology; it was “more elusive and complex than any other branch of knowledge” because it sought to freeze indigenous concepts in time in order to analyze them.⁸⁴ The four prior fields relied on questionnaire and schedule inquiry, and certainly the emerging practice of sophiology could also draw on reports that focused on Indian “superstition” and myth-histories. Powell recognized that American indigenous communities held different traditions of “opinions,” and that the logics, justifications, and nuances of their belief systems should be recorded for posterity. To study the formation and transmission of ideas, ethnographic inquiry would need to push beyond tabular data accumulation and grapple with Indian ideas in the context of their use. In sophiology, then, Powell understood the limits of schedule-based accumulation.

To Powell, Indians certainly differed from whites in their understanding of the world and its workings, and how they explained the presence of phenomena to their kin. To him, theirs were subjective accounts; in his explanation of sophiology, Powell skirted around ascribing non-Western peoples the production of true “knowledge.” Powell laid out a philosophical model of causal understanding, asserting that mentation derived from sense impressions, and that experience over time and memory of past sensations allowed thoughts to continually reappear, thus cohering as concepts or notions about how sense impressions were created. These, Powell

⁸² Charles Darwin, “Queries about Expression for Anthropological Inquiry,” *Smithsonian Annual Report* (1867), 324; Charles Darwin, “The expression of the emotions in man and animals” (D. Appleton & Company, 1897 [1872]).

⁸³ Powell, *Twentieth Annual Report*, clxxii.

⁸⁴ Powell, *Twentieth Annual Report*, xxi.

believed, were “opinions,” and such opinions stabilized in their contexts and were transmitted between individuals; eventually, through their aptitude to describe the world to a given community, opinions continued to be passed on through time.

Correct opinions developed in the individual and propagated from man to man become immortal, while only incorrect opinions ultimately die; but the vast body of opinions as they arise from moment to moment are born only for an ephemeral life. Of those that have appeared upon the stage of history because they have been accepted by the great thinkers, it remains to be said that still the many die and the few live. While they live they are esteemed as science, when they die they are esteemed as errors; hence sophiology can be defined as the science of opinions and their classification as errors or truths when accepted as such by the leaders of human thought, together with the methods of discovering and propagating such opinions.⁸⁵

In Powell’s five-science scheme, sophiology took the top place because it could provide an answer to questions of purpose for the former four categories—why games were played, why functionless symbols ornamented material technologies, why certain institutions were maintained, why some words appeared to contain explanations of cause-and-effect that could not be verified.

Because sophiology would identify Native superstition and the means of its perpetuation, it could also identify how to best instruct Indian groups (especially children) in “correct opinions,” the scientific and moral axioms held by Powell and other influential Americans. Leaning on an evolutionary scheme for intellectual development, Powell suggested that Anglo science made, and conveyed, knowledge of the world most truthfully. By placing the history of science within sophiology, Powell presented scientific study (and its emphasis on verification of cause and effect) as a progressive force, capable of evolving societies into a state of civilization.

Thus, from an anthropological point of view, sophiological study had a doubled potential: to identify and record superstition and ignorant reasoning in indigenous communities *and* to dispel the illogical and mythological from Native systems of thought. In this vein, Powell warned that faulty logic in opinions or explanations of a given society could infect the other four areas of human activities that he designated. He worried, for instance, that social institutions founded on

⁸⁵ Powell, *Twentieth Annual Report*, clxxii.

mythological beliefs begot secret societies or evinced thaumaturgy or magical practice, which for Powell were among “the monster passions of mankind that stifles the pure love of truth.”⁸⁶

Sophiology proposed to penetrate deeper into a given culture’s historical explanations in the form of mythology and folk tales, their expression of learned opinions such as taboos, and their spiritual practices and social codes that constituted community life. The data desired, then, were also among most difficult of information to obtain from Indian communities—the things that could not be readily seen or heard but would emerge in situations distinct to the education and rearing of community members—situations that frequently excluded outsiders.

Sophiology represented an aspiration to move beyond object and linguistic accumulation, to include data that was more ephemeral yet fundamental to the constitution of the lives of Others. The term corralled the diversity of epistemological fields of anthropologically inflected study that developed in the same period, namely the fields of folklore and mythology, psychology, and religion.⁸⁷ Sophiology, combining folklore, psychology, and religious studies, illustrated a shared desire to inspect the Indian “state of mind” and to organize study around these elements of life through terminology and a scholarly vocabulary. Unlike previous aspirations within ethnology to study Indian mentality, Powell sought to understand the practical application of indigenous knowledge to the development of its particular character, rather than root out a developmental explanation of why Indian groups had never advanced to “civilization” on the ladder of social evolution. Sophiology’s aims were practical yet meta: to explain explanations, to instruct on instructions, to know knowledge. The search for these required sustained studies in the field, trusted informants, and experiential knowledge of a community’s life processes. Here, then, was another way to understand Anglo human science and its transforming aims as it intersected with indigenous life and history in the shared experience of the ethnological encounter in the 19th century Southwest.

Limitations of the Schedule

While Powell had discovered the value of intimate relationships with informants and came to understand the limitations of ethnological questionnaires, his *Introduction to the Study of*

⁸⁶ Powell, *Twentieth Annual Report*, clxxxiii.

⁸⁷ For an internalist history of the discipline of Folklore studies, and the rift between literary and anthropological folklorists, see Rosemary Lévy Zumwalt, *American Folklore Scholarship: A Dialogue of Dissent* (Indiana University Press, 1988).

Indian Languages still circulated in the field. Ethnographic inquiry by the 1890s still had a rigidity to the modes of accumulation—or, at least, a structure was built into the paper schedules themselves. This rigidity, however, did not always hinder the sort of “thick” accumulation that Powell came to envision in the 1890s. In practice, the circular or ethnographic manual could be used as a launching point for inquiry, but one that went beyond the lines and columns of the schedule.

Washington Matthews, working among the Hidatsa in North Dakota in the 1870s, followed the sequence of words listed in George Gibbs’s 1863 vocabulary to construct a dictionary of his own. Speaking with No Shield, his informant, the two outlined a list of Hidatsa words. If communication broke down, No Shield taught terminology within Hidatsa conceptual categories—from limbs to genitalia to skulls and bones—and Matthews dutifully wrote these down.⁸⁸ As a standardized questionnaire, the document depicted the representative concerns of emerging professional ethnology. While Gibbs and his collaborators clearly imagined the data’s return to Washington, Matthews did not deposit a Hidatsa vocabulary into the Smithsonian vaults. But even omitting a return journey of the paper technology, ethnology could be advanced by fieldworkers who came across the documents and oriented their fieldwork practices to fit the state of the art. Instead of returning the document, Matthews used the schedule to frame and elaborate a broader project that amounted to an *Ethnography and Philology of the Hidatsa Indians* (1877).⁸⁹

Matthews was clearly inspired to do ethnographic work, and Gibbs gave him an orienting device for work in the field, as well as a governmental institution to which he could entrust his intellectual labor. When Matthews was transferred to the Southwest by the US Army, he corresponded with John Wesley Powell, by then the director at the Bureau of Ethnology (housed within the Smithsonian), about potential research in the region.⁹⁰ Stationed at Fort Defiance, at the outskirts of the Navajo territories, Matthews had Powell’s 1877 volume in hand when he arrived in 1881. And as a skilled and recently-published linguist, Matthews was certainly competent enough to fill out Powell’s schedules and return them to Washington. Yet, he did not

⁸⁸ Washington Matthews, “Short Notebook and Word List (Ree),” WMA.

⁸⁹ The ethnographic section gives standard procession through Location, Dwellings, Drying Frames (non-standard, of course), Caches, Cemetery, Places of worship, Fortifications, Farms, Inhabitants (different groups), Population, Conversation, Arts, Inter-Tribal Trade, etc. Washington Matthews, *Ethnography and philology of the Hidatsa Indians* (US Government Printing Office, 1877).

⁹⁰ John Wesley Powell Correspondence, Washington Matthews Papers, WMA.

quite complete the imagined circuit for the schedule volume. Instead, he reconfigured the tabular directive to suit his own research ends.

Instead of filling in boxes and adopting pre-determined categories, Matthews used Powell's volume as a notebook of his own design, as a convenient and durable collection of paper to write upon. And instead of pre-formatted questions, Matthews let his informants—Paper Carrier and Tall Chanter and Jesús—lead him to novel parts of Navajo thought. Over Powell's schedules for "Religion" and "Dispensation of the Dead," for instance, Matthews scrawled notes on Navajo beliefs about the causes of thunder, about their ichthyophobia (avoidance of eating fish), about the use of rain and how to speak about it. In short, the formal logic of the *Introduction to the Study of Indian Languages* did not fit the sort of research Matthews found himself engaged in.

Ironically, the tabular form of the schedule may have prompted investigators such as Matthews to experiment with new ways of accessing indigenous knowledge. Many of the surviving *Introduction to the Study of Indian Language* volumes (both 1877 and 1880) contain marginalia from the fieldworker, and hint at their negotiation of meaning and sentiment in their interaction with a Native informant. These marginalia are a sort of "scribble," in the sense given by Hans-Jörg Rheinberger: emerging, developing forms of knowledge, not yet ordered or smoothed for scientific communication. Scribblings show processes of "redimensionalization," where "the organization of an experiment in time and space is projected onto a two-dimensional surface... [The] reduction to a surface facilitates exploration of new ways of ordering and arranging data: sequential events can be presented in synchronic form, temporal relations as spatial."⁹¹ By writing over Powell's tabular form, Matthews laid out new material and could work to discern patterns at various levels of condensation, which in turn provided him with a framework for practicable research, preventing any potential foreclosure that the schedule itself might have brought about.

Matthews, of course, did not write exclusively on repurposed Bureau of Ethnology schedules; he kept a variety of notebooks across his career, exemplifying a variety of strategies of documentation in late-nineteenth-century ethnography. Like Gibbs and other linguists in the Smithsonian archives, Matthews transferred information between notebooks while in the field.

⁹¹ Hans-Jörg Rheinberger, *An Epistemology of the Concrete: Twentieth-Century Histories of Life*. (Duke University Press, 2010), 245.

He wrote most of his “live” notes in palm-sized field journals. New words learned in the course of fieldwork were later transferred into more formal books on Navajo language. Matthews arranged his dictionaries phonetically, or alphabetically in his own orthographic interpretation of Navajo language. Like many ethnologists in the field in the 1880s, he wrote his dictionaries in address books, with physical-tabular-alphabetical organization. He also devised handy phrasebooks. With his own system of information management, Matthews did not contribute to the language-archiving project of the Bureau of Ethnology, but he was mastering the tools for an emerging ethnographic method, the development of sustained relationships with informants and their milieux—what became participant-observation.⁹²

Across his different notebooks, an Anglo rendition of knowledge of Navajo language and ceremonialism circulated and looped. New words learned from ceremonial practice were inscribed into dictionaries; vocabulary conveyed by an informant could be glimpsed in ritual practice, and a fuller understanding of its meaning could thus snap into place in text and the substance of Matthews’s mind. These scribbles knotted into specific ethnographic research problems detailing unique qualities of Navajo life. For Matthews, language-learning gave insight into how the Navajos thought, why they believed what they believed, how they drew meaning from their practices of healing and veneration. Notably, from his studies, his major monographs were not linguistic but rather intensive studies in the field with willing Navajo informants, who allowed him to witness portions of the nine-night healing ceremonials—the Mountainway and the Nightway—which he attempted to document their complexities with just a pen and notepad. His informants also began to teach Matthews the ins and outs of their ceremonialism, sharing with him the underpinning logics and purposes of a ceremonial’s elements. These teachings, too, he scribbled down in his notebooks.

Through these scribbled knots, Matthews stumbled upon hints and suggestions of new areas of Navajo life heretofore unknown. His pursuit of these hidden or oblique areas of Navajo cultural practice, such as the massive sandpaintings drawn by healers in medicine ceremonials, markedly advanced the study of Navajos in particular and indigenous ceremonialism in general.

⁹² Matthews also transferred material from the smaller field notebooks to larger volumes that operated as manuscript drafts or organized ethnographic topics. See chapter 2, which presents a case study of Matthews’s written documentary practice. Matthews witnessed complex specialist material practices and scribbled down detailed information, live, in-the-moment.

For now, I will keep the reader in suspense—a full account of Matthews’s documentary practice is presented in Chapter 2.

Ethnographic strategies like that pursued by Washington Matthews identified intellectual objects that were more difficult to glimpse in the systematic logic of the linguistic schedule. These objects were tricky and elusive because, often, the suggestion of an Indian group’s hidden ceremonial knowledge or other “secrets” was oblique. Fieldworkers like Matthews sensed something was there to pursue but did not know what it was. The table itself was not strictly constraining, but rather the constraints of the table made it possible to conceive of what lay *outside* the table—things “known” only because they were “unknowns.” To reveal these objects, Matthews needed to forge relationships, to make diplomatic inquests and solidify bonds of trust and respect.

Matthews pioneered extended, in-depth study of a cultural facet of an Indian ceremonialism in conjunction with strong, bonded informants. Though professionally a surgeon, he also engaged in a form of anthropology recognizably “professional,” and certainly with attention to scientific, authoritative accumulation of data. Matthews’s self-directed ethnography, and his misappropriation of *Introduction to the Study of Indian Languages*, moved beyond the constraints of formal, tabulated paper technologies.

By bringing the paper technology of questionnaires into the field and grappling with them as he attempted to gather data from his informants, Washington Matthews superseded and advanced the forms of information accumulation for the desiderata of institutionalized anthropology at the time. He supported the mission of comparative ethnology and philology but found their tools (or their margin-space) only marginally useful. And yet, the constraints of the paper questionnaire made it possible for Matthews to conceive of what lay outside the table—“known unknowns” to be discovered. These early inscription devices produced their own outsides, their own excesses, elusive and even secretive topics. These outsides and excesses, as we will see in subsequent chapters, came to guide a significant portion of Southwestern ethnography.

Schedule-based ethnology framed the study of Indian cultures within a bureaucratic logic of identification and accumulation, as social groups to be archived and studied, in part because they were likely to dissolve under the force of US settler colonialism. Ethnologists across the

19th century used questionnaires and schedules as inductive technologies to collect ethnographic information—data that could be collected, archived, sorted and compared—and which could be made useful to other areas of the US government and curious citizens. Not only was ethnographic information a contribution to knowledge, but it served business and government interests as guides for interacting with different (non-Anglo) populations as the United States came to grips with a continental empire composed of diverse peoples.

On its face, the schedule network was an operational scientific process of evidence accumulation—labor was enrolled to fill blanks, data was collected, and results were derived through the apparatus of the schedule. But the schedule network and its paper technologies also served to materialize “anthropology” as an assemblage of practices, technologies, and information management systems.⁹³ It did this in conjunction with scholarly societies and networks of archaeology or anthropometry; as scholarly data coalesced, anthropology formed an archive of rationally rendered data and developed a system of credibility checked by the formatted paper technologies. Additionally, the linguistic schedule let people in faraway lands understand that, elsewhere, this developing field actually existed; schedules tracked between and intellectually linked the center of scholarship in Washington with the periphery, the “field.” For those who came to ethnography by way of another profession, such as Washington Matthews, the schedule made anthropology legible as a discipline. In turn, this legibility was useful to their superiors for justifying the act of collecting data about indigenous communities as a task contributing to Anglo American social and intellectual advancement, including questions of the future of Indian populations within the United States.

In addition to the labor of lay accumulators working with the paper technology of the ethnographic circular, the Bureau of Ethnology supported research such as that of Matthews, fieldworkers who could go beyond the directives of the schedule and depict the range of differences within and among Indian communities. To advance anthropology and contribute to developing specializations within anthropology fields such as the study of Indian inner life, the Bureau needed fieldworkers who could pursue leads, dive deeper in to the opinions and modes of instruction of the mystifying Other and form strong relationships with informants to facilitate information extraction. But, as a government entity beholden to the rules and systems in which it

⁹³ Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society* (Cambridge, Mass: Harvard University Press, 1987).

was ensconced, late 19th-century research on Indian novelty was still joined to the Bureau's classification of Indian groups within a taxonomy of social sophistication, linguistic family, and technological development. By the end of the century, a proposed field—sophiology—enshrined a disciplinary goal to understand Indian thinking, which included the placement of Indian intellectual accomplishments in a system of hierarchy, and to foster new educational initiatives that would enlighten and modernize Native communities. What could not be anticipated, however, was that Indian communities could—and did—resist the imposition of Anglo education on their system of understanding and making meaning in the world and continue to devote intellectual energy to their own epistemological systems.

Chapter 2 Documenting the Ineffable: Navajo Singers and the Science of Ceremonialism

Laughing Singer had an acute awareness of the strangers streaming into the high desert plateaus and ranges in Navajoland (Dinétah) and had nonetheless decided that he would present his as a smiling face, open to the world. He was known for his geniality and pleasant demeanor, his voice always ready to crackle into laughter, and as a knowledgeable medicine singer among his people, the Diné (the Navajo's ethnonym). From these attributes came his name Navajo sobriquet, Hatali Natloi, meaning Laughing Singer or Smiling Chanter.¹

From what can be gleaned of his past, Laughing Singer (c. 1833–1923) had been raised in the south-central portion of Navajoland. Under the tutelage of No Sense Old Man, Laughing Singer trained in Diné medicine, as a *hataalii* or medicine singer, before the forced relocation of the Navajo in the Long Walk to Bosque Redondo in 1864. When he returned from Bosque Redondo in 1868, he eventually made seasonal homes near Cottonwood Springs and Dilkon, abutting the Hopi Mesas.²

As a *hataalii*, Laughing Singer mastered the longest ceremonials in Navajo medicine, the Nightway and the Mountainway, as well as shorter chantways, the Plumeway and Coyoteway. With the Nightway and Mountainway each lasting nine nights, including hundreds of songs and prayers, and featuring four elaborate sandpaintings, Laughing Singer's knowledge of these two major chantway ceremonials marked him as a formidable intellectual among the Navajo, for he was accomplished in healing and knew intimately the history and cosmology of his people.³

¹ Washington Matthews, *Navaho Legends* (American Folk-Lore Society, 1897).

Hatali Natloi, also Hatáli Natlóí, was the spelling Matthews gave to Laughing Singer's name rendered in Navajo. For purposes of clarity for the reader, I will use the name Laughing Singer throughout this chapter.

² James C. Faris, *The Nightway: A History and a History of Documentation of a Navajo Ceremonial* (University of New Mexico Press, 1990), 49. See also Nancy J. Parezo, *Navajo Sandpainting: From Religious Act to Commercial Art* (University of Arizona Press, 1983).

³ Faris, *The Nightway*, 82-83.

Laughing Singer was also amiable to the Anglos who had come to the region in the 1870s and 1880s. Some of these white men took an interest in Hatali Natloi's knowledge of healing. Although he was probably warned against sharing knowledge with the white men by other medicine singers because their presence might bring misfortune or corruption to the healing practice, Laughing Singer appeared sure enough in his abilities that he allowed several Anglos, James and Matilda Stevenson and some of their ethnographic party, to witness a Nightway performance in 1885.⁴ He would continue his policy of openness in the years to come.

During this time, another Anglo, Washington Matthews, was also visiting chantways in Navajoland and asking about their medicine system. This visitor was a healer among the Whites, and he was known in south-southeastern Navajoland as one who could set bones and who offered a constellation of remedies different from the Navajo medicine system. Perhaps Laughing Singer had heard from a friend and fellow singer about this inquiring white man, who asked after *hataalii* knowledge and practice, and that this friend, Tall Chanter, had obliged. Tall Chanter had, in any case, journeyed to Washington, D.C., in the later part of 1885, and taught the inquiring white man named Matthews about the Mountainway ceremonial.

Laughing Singer came to know Washington Matthews in the 1890s. The white man had studied under Tall Chanter and several other chantway singers and appeared respected among his own people for his abilities as a surgeon. Laughing Singer was generous with Matthews and helped him with his inquiries several times throughout the decade. The singer consented to Matthews's presence during at least one Nightway ceremonial. The singer also visited with the ethnographer outside of ceremonial performance; Laughing Singer had been paid by Matthews in several instances to teach the latter about ceremonial paraphernalia, about Navajo legends and history, and about the songs and prayers of the longest chantways.

Over the winter in 1891-92, Laughing Singer trained Matthews in the construction of prayerstick bundles (*k'eeet'áán*) and sacrificial drumsticks, imparting to the man the complex recipes of materials that composed the individual artifacts.⁵ Matthews was impressed by the singer's diligence and care. At one point during this winter training session, the *hataalii* took a thin reed in hand and with a knife deftly cut notches in one end. These were cigarette-reed

⁴ Faris tentatively argues that Laughing Singer was the Nightway singer in the account given in James Stevenson, *Ceremonial of Hasjelti Dailjis and Mythical Sand Painting of the Navajo Indians* (Library of Alexandria, 1891); I follow Faris's account. Faris, *The Nightway*.

⁵ Matthews Notebook, 29 December 1891, Ceremonial Tech Notebook, WMA.

offerings, he explained to Matthews, and his cuts were a form of individualization, so that the Holy People would recognize their specific offerings and not waste time bickering over who-gets-which.⁶

Later, during a hunt for yucca leaves, Laughing Singer passed by hundreds of yucca plants until, an hour into their search, he came to one that was almost satisfactory. Here he explained to Matthews the qualities he sought after: long and straight leaves, unbroken and unblemished, all on the same plant, plucked without having to be cut. During the construction of the prayer sticks in the *hoogan* (a Navajo dwelling), the singer continued to display his meticulousness. Making the bundles required attention—to the directions the yucca leaves faced, to the orientation of their points and bases, to the number of times the bundle was wrapped with string—and in calling for attention to such detail, Laughing Singer helped Matthews understand the *hataalii* reasoning that rested behind the entire process.⁷ This instruction provided by Laughing Singer, as well as language instruction from his translator Ben Damon, allowed the outsider Matthews a glimpse of the consistency, symmetry, and intricacy of the Navajo knowledge system. Surely those Navajo present hoped he would be a good steward of this knowledge.

While Laughing Singer kept all of his knowledge in his mind, he understood that Anglo ways were different. The student dutifully wrote down what his tutor said in small notebooks—a familiar practice of some visiting white men, from the Navajo perspective. In 1894, Matthews brought with him a wax cylinder recorder, and Laughing Singer sung a host of songs into the novel device; later the cylinder played back for him his own voice. Laughing Singer also sanctioned Matthews to make permanent, illustrated copies of the sandpaintings presented in the Nightway. While he knew that many of his peers opposed permanent painting, Laughing Singer consistently appeared open to sharing his knowledge with Matthews, teaching the Anglo man and regularly consenting to his documentation of the facets of Navajo ceremonialism.⁸

The notebook and cylindrical recorder captured something from Navajoland and allowed the Anglos to look at (or hear) this thing in a different time and place. While the parceling of the ceremonies elements did not seem to bother Laughing Singer (if such objections could even be gleaned from Matthews's notes), presumably the chantway singer felt assured that the totality of

⁶ Matthews Notebook, 29 December 1891, Ceremonial Tech Notebook, WMA.

⁷ Matthews Notebook, 30 December 1891, Ceremonial Tech Notebook, WMA.

⁸ Washington Matthews *The Night Chant: A Navaho Ceremony* (Knickerbocker Press, 1902). Laughing Singer may have even brought other students to see Matthews's drawing, to speed along their acquisition of sandpainting knowledge.

a chantway, its contextual and sensorial elements, could never be shipped out of Navajoland and practiced by Others. After all, the importance of chantways was linked to the land and to the people.

Washington Matthews, in his visits to Navajoland in the 1880s and 1890s and his instruction under medicine men such as Laughing Singer and Tall Chanter, found a treasure-trove of ethnological data about Navajo ceremonialism.⁹ The complexity of Navajo chantway systems, and their encapsulation of many facets of Navajo belief and practice, provided Mathews a route to study the constitutive mental and social structures of Navajo life. For Matthews, working toward a “science of ceremony” might provide the tools to study Navajo notions of causality, to study their explanations for the workings of the world. Not only did Navajo ceremonialism provide epidemiological and etiological theory (the causation and control of an affliction), but in the constitution of its healing power the practice could also offer information on Navajo origins and the logic of its social and spiritual rules. But finding the treasure-trove was one thing; it was another to cash it in, to transport it from the field, render it into material for use in the libraries and archives of the East. Each chantway featured a host of formal elements—sonic, olfactory, gustatory, visual, haptic—that piqued the ethnographer’s senses in interwoven patterns throughout a lengthy ceremonial. The problem with such a complex ceremonial system was the documentation of its ineffable qualities. How could Matthews build a science of ceremony when the “stuff,” from the ceremonialist’s perspective, could not be measured, replicated, or divorced from its context?

While a “science of ceremony” presumed a comparative method, whereby measurable elements could be compared to other localized forms of knowledge to reveal common tropes or modes of reasoning across the cultures of the globe, Matthews’s work was intimately bound to his experience in Navajoland and his relationship with his tutors. These local and interpersonal connections far exceeded his ability to document the ceremonial, a caveat that he often mentioned in his writings. But his relationships and even his appreciation of Navajo ceremonialism did not contradict his aspiration to collect ethnographic data on paper. Matthews,

⁹ For biographical details, on Washington Matthews, especially his intellectual aims and influences, see Poor, *Washington Matthews: An Intellectual Biography*; Halpern & McGreevy, *Washington Matthews: Studies of Navajo Culture*. A full biography of Washington Matthews has yet to be written.

after all, still hoped to communicate data to a scholarly community, to take something from his Navajo experience and render them meaningful to Anglo audiences.

In this chapter, I begin by presenting Matthews's investigation of Navajo ceremonialism, including his documentary techniques and emerging intellectual questions. For Matthews and his study of ceremonialism, Navajo singers, assistants, and translators took on pedagogical roles, instructing the Anglo surgeon in the meaning and role of ceremonial performance and paraphernalia. Indeed, there was little other way for him to understand the acts before him—without guidance, it was near impossible for the even the linguistically-able Matthews to understand the icons of the Navajo pantheon that singers and assistants dry-painted on the sandy canvas of the high desert, or to comprehend the substance of the hieratic language of chant and prayer.

Matthews was a pupil of several medicine men in addition to Laughing Singer and Tall Chanter.¹⁰ While the men who chose to teach Matthews were liberal with the knowledge they conveyed, they communicated to Matthews the potential risks of handling sacred knowledge. He came to understand that ceremonialists drew power and credibility from their mastery of ceremonial scripts, songs and illustrations, all of which called upon the Holy People (deities) for aid, blessing, and protection. The precision and intricacy needed to perform ceremonial knowledge made medicine men powerful, respected, and materially wealthy among the Navajo.

Given the highly structured form of Navajo ceremonialism and the importance of temporality in the material and aural practices of chantways, the very idea of making a record of chantway processes was a daunting prospect. Yet, Matthews attempted to capture it, in cases both sanctioned and unsanctioned: He transcribed songs and prayers on paper and recorded them in wax; he sketched dancers and their feats; he collected and even constructed ceremonial objects like prayerstick offerings to the Holy People; and copied complete sandpaintings and made notations of their various mythological and historical elements. And while knowing the power

¹⁰ Matthews maintained a strong relationship with a translator, Jake, also known as Biolzog (Silversmith) or Náltsos Nigéhani (Paper Carrier), who performed some lesser chantways and assisted with many others. He forged additional lasting relationships with other regional medicine men. Matthews's notebooks also mention Old Torlino and his son, Torlino; Gordo; Nosey; Manuelito; the translators Jesús Arviso, Ben Damon, and Chee Dodge. His sources also mention a Mountainway singer called Tsi-tca'ci (about whom almost nothing is known, and even his name may be an ill-fitting rendering). Tsi-tca'ci, also given as Tsi-toa'ci, is not to my knowledge mentioned in any secondary literature on Matthews. I give this brief mention of Tsi-tca'ci for this reason and in an attempt to recover some of evidence of Navajo singers (even if only a shred), in recognition of their importance as knowledge keepers and as teachers of traditions, histories, and practices. See Matthews Paper collections the Wheelwright Museum Archive (WMA).

and importance of knowledge stewardship for his informants, Matthews shared his rendering of Navajo ceremonial knowledge with his disciplinary community, publishing a host of papers on chantways and ceremonial paraphernalia.

By this act, Matthews transformed the contained, temporal event of the chantway, a situated application of knowledge, into discrete written information about Navajo ceremonialism. Instead of allowing sandpaintings to be brushed away from the earth and the last note of a song to fade into silence, Matthews documented these elements specifically—materializing the ceremonial elements that were, to convey their healing and purifying aims, designed to be dematerialized and stricken from the earth, to the realms of gods and spirits.

In Navajo chantways, stewarded specialist knowledge was performed and elements of the healing practice were materialized through the process. Then, as the ceremonial came to a close, these disappeared into the wind. This process of materialization and dispersal was, for medicine singers, part of the healing process. Disappearance, however, was materially incompatible with ethnographic documentation. Matthews's inscription work under Navajo tutors and from his experiences during chantway ceremonials in the medicine lodge were acts of "redimensionalization"—flattening time and space into transportable marks on paper or grooves on a wax cylinder. Following the work of Hans-Jörg Rheinberger, the process of redimensionalization explores "the organization of an experiment in time and space,... projected onto a two-dimensional surface." In Rheinberger's conception of the effects of the redimensionalization of phenomena in the world to the two-dimensional space of the paper page, the "reduction to a surface facilitates exploration of new ways of ordering and arranging data: sequential events can be presented in synchronic form, temporal relations as spatial."¹¹ For Washington Matthews's studies of Navajo ceremonialism, he needed to condense long and complex events into a two-dimensional form; once done, patterns could be identified at various levels of condensation, which in turn provided him and other anthropological researchers with frameworks for practicable research. Matthews's redimensionalization and materialization of the elements of chantways mapped out a documentary strategy for ineffable phenomena. His tactics and method primed further investigation and, importantly for the discipline of anthropology,

¹¹ Rheinberger, *An Epistemology of the Concrete*, 245. While Rheinberger's work focuses on the life sciences, it is simple enough to understand Matthews's ceremonial studies as an ethnographic experiment; he certainly had no prior model for the documentation of something like a chantway ceremonial, which featured complex aural and physical gestures, rich symbolism drawing on Navajo myth and history, and required many days of vigilant observation.

provided an argument for the preservation of indigenous knowledge traditions with the documentary tools of the human sciences.¹²

But in what *senses* did Matthews really capture such a complex event? Navajo singers, one may presume, would reckon that his documentation not only miscast the ceremonial, but in its replication also irresponsibly provided the tools for others to practice chantway singing without the proper training and attention to knowledge stewardship. Given this, why did singers such as Laughing Singer and Tall Chanter sanction and facilitate Matthew's study? In return for Matthews's companionship (and gifts, including money), I suggest that singers imparted an important lesson to Matthews about the contextual nature of truths—even if he did not fully recognize this exchange. Singers such as Laughing Singer and Tall Chanter explained the way their knowledge system operated, wherein knowledge was transmitted to worthy seekers by an experienced person, who in himself (or herself) had the ability to decide the worthiness of a student. Even if Matthews could not appreciate lessons from the singers' perspective, he endeavored to take a snapshot of this process, materializing a series of experiences as an exportable form of knowledge. In doing so, he further opened a passage for ethnological questions of Indian ways of knowing and teaching, epistemological inquiries that began, imperfectly but perceptibly, to analyze knowledge from within the context of the Native view not simply as superstition or ignorance, but as a systematic mode of thought.

When Matthews eventually produced his two studies of the Mountainway and Nightway, his hybrid form of ethnographic documentation established a proof-of-concept for the ability to study ceremonial complexity, to document the ineffable with some modest success. The very complexity of his ceremonial documentation revealed, however, that a comparative science of ceremony could only begin with the establishment of thorough, specialist texts on individual indigenous ceremonial practices. Looking at his attempts to document sensorial and metaphysical complexity, this chapter illustrates that Matthews allowed himself to be led by intuition and field experience, especially given his intimate relationships with his informants, instead of the formalities of a scientific scheme. And yet, his ethnographic efforts were primarily

¹² For work on materialization in the history of science, see Lenoir, *Inscribing Science*; Annemarie Mol, *The Body Multiple: Ontology in Medical Practice* (Duke University Press, 2002); Hacking, *Historical Ontology*; John Tresch, "Cosmologies Materialized: History of Science and History of Ideas," in Darrin M. McMahon and Samuel Moyn (eds.), *Rethinking Modern European Intellectual History* (Oxford, 2014); Hull, *Government of Paper*; Elizabeth Edwards and Janice Hart, *Photographs Objects Histories: On the Materiality of Images* (Routledge, 2004).

products for his scientific community, rather than for the posterity of Navajo specialist knowledge.

Learning of Navajo Chantways

When Laughing Singer showed Matthews the theory and practices of making prayerstick offerings in the early 1890s, he was instructing a student who had already spent many years studying Navajo life and spiritual beliefs. Matthews had been posted to Fort Wingate as a medical officer in 1880, coming to New Mexico with the intention of pursuing ethnological work among the Navajo as a side project to role as the fort's doctor. Previously, he had held a post at Fort Berthold and engaged in a philological study of the Hidatsa and had published a dictionary and short ethnography of these people. Matthews relied on his knack for linguistics during his early time in the Southwest to accumulate a vocabulary of Navajo and begin to learn the language, as well as engage in an ethnographic study of Navajo industry, specifically silversmithing and weaving.¹³

In addition to his direct study of Navajo language and industry, Matthews became aware of a deeper facet of Navajo life. Unlike the neighboring Pueblo Indians, about whom much could be read, the Navajo were less ethnographically documented, and what could be inspected portrayed them as anarchic wanderers, without government or religion.¹⁴ Matthews, in contrast to former ethnographic reporters, kept an open mind to the presence of Navajo social structure and religious life, dimly aware that Navajo medicine men, called singers or *hataalii*, held an important place in Navajo life, and that they, like him, were called upon to heal the sick and psychically afflicted among them.

Initially, Matthews believed that Navajo singers were knowledgeable men who gave blessings and served as the directors of dances, where masked players mimicked gods and reenacted legends. Dutifully writing ethnographic notes in his ledger, in 1882 Matthews witnessed a part of a Navajo religious event, a public dance around a large bonfire that lasted

¹³ Washington Matthews, "Navajo Silversmiths," *Second Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution* (1883); Washington Matthews, "Navajo Weavers," *Third Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution* (1884).

¹⁴ Of the few early texts, most influential was a military reconnaissance report of Lt. James H. Simpson in 1849. See J. H. Simpson and Frank McNitt, *Navaho Expedition: Journal of a Military Reconnaissance from Santa Fe, New Mexico, to the Navaho Country Made in 1849* (University of Oklahoma Press, 1964). Matthews was also familiar with Jonathan Letherman, *Sketch of the Navajo Tribe of Indians, Territory of New Mexico* (Smithsonian Institution, 1856).

long into the night.¹⁵ The “dance” was striking to Matthews because of its sexual content—scenes of accusations of infidelity, of simulated masturbation, and of lusty pursuit and intercourse. Though there was much laughing from the audience, Matthews personally found the playacted scene “horrid.” However marked by the uncouth scenes, Matthews nonetheless took notes in a serial form, and became especially interested in the masks made of yucca leaves worn by the performers. However, at this moment he was not able to inspect the masks worn by performers and was unsure what the “dance” represented except as a play for laughs.

It was not until later that Matthews realized how much he had underestimated the complexity of Navajo “dances.” Anglo visitors to Navajoland prior to Matthews presented the Navajos as possessing neither government nor religion of any sort. “[They] even have not, we are informed, any word to express the idea of a Supreme Being,” wrote Army Surgeon Jonathan Letterman in his report of 1856.¹⁶ Letterman declared the near impossibility of sorting out a history of the Navajos: “It is impossible to learn anything from the people themselves, as they have no traditions. A volume of no mean size might be written, were all the stories of interpreters taken for truth; but it would be found one mass of contradictions, and of no value whatever.” Letterman, then, had foreclosed the possibility of an ethnography of Navajo religion because he did not find (or chose to deem inauthentic) evidence of consistent spiritual practices. Matthews, however, was open to revising this position.¹⁷

Between 1882 and 1884, one of the Navajo interpreters who worked with Matthews explained the “dance” in more depth, and Matthews realized that the sexual play he had witnessed was a small part of a larger performance.¹⁸ The “dance” he had witnessed was in reality the final part of a nine-day ceremonial that contained numerous rites, songs, unique ceremonial paraphernalia and, most surprising to Matthews, large sandpaintings, drawn on the ground of the medicine lodge. These multi-day ceremonials—also called chantways—presented Matthews with a dilemma of documentation, for their interwoven material and oral practices escaped the ethnological categories with which he was most familiar, and forced him to think deeply about new modes of capturing ethnographic information.¹⁹ Beyond language and action,

¹⁵ Matthews Notebook, 1882 Hackan Dance Notebook, WMA.

¹⁶ Letherman, *Sketch*. The published report incorrectly stated the author’s surname.

¹⁷ Matthews, *Navaho Legends*.

¹⁸ In fact, when Matthews published the extended chantway, this portion was suppressed for its content.

¹⁹ “Chantway” was not a term used by Matthews but is in use in Navajo anthropology today. I will use it here because it brings more specificity to the things in Navajo medicinal practice that Matthews focused on.

the complexity and novelty of chantways opened a door to new forms of ethnological understanding, and in turn required new strategies of documentation.

Matthews was especially keen to see ceremonial sandpaintings; this “discovery” would prove to be a hot ethnographic item because it recast Navajo religion as possessing a complicated aesthetic component that Anglo scholars could appreciate, even without deep knowledge of Navajo culture. In October of 1884, his interpreter, Jake the Silversmith, led Matthews to Niho’tlizi (Hard Earth), a camp seated in the pine forest foothills near of the San Mateo mountains, near a mountain sacred to the Navajo, Tsoodzil (Mount Taylor). Matthews pitched his tent on the outskirts of these encampments, a bit away from the brush-and-earth medicine lodge and sleeping corrals that doubled as preparation areas for the medicine ceremonial. Jake knew of Matthews’s special interest in the sandpainting and had asked the presiding chantway medicine singer if the Anglo could witness part of the event.²⁰

Medicine singers came to such camps in the late fall and winter, when the bears and snakes were sleeping away the cold. Here, stories could be told and patients could receive a healing through a *sing*.²¹ Chantways were primarily ceremonials for the curing of an individual’s disease, and secondarily to provide benefits and blessings for friends and family. The camps served as gathering places for psychically or physically afflicted Navajos. A singer would be found and paid the appropriate fee—a suite of horses or other beasts of burden, ceremonial supplies such as eagle feathers or rare sorts of wood, food and water, perhaps some American dollars—and then the preparations for a sing would begin.²² Kinfolk and friends joined patients in the sings, which took place in a special medicine *hoogan* or dwelling. While portions of the ceremony had a serious air, over the many days it took to complete the ceremonial cycle a small community would cohere and commiserate, share stories and gossip, eat well, and recount histories of the people from the earliest times.²³

²⁰ This came at an unfortunate time for Matthews, for he was recalled to Washington, D.C., to the Army Medical Library in April of 1884. Probably fearing pre-emption, he arranged a research trip to New Mexico for October and November of 1884.

²¹ Matthews Diary, 1880-84 Bureau of Ethnology Notebook, WMA.

²² Matthews Diary, 16 October 1887, 1887 Conditions near Manuelito, WMA.

²³ Washington Matthews, “The Mountain Chant: A Navajo Ceremony,” *Fifth Annual Report of the Bureau of Ethnology, 1883-1884* (Government Printing Office, 1887).

Jake had brought Matthews to a Mountainway, to be “sung over” a female patient with an unrecorded affliction.²⁴ The singer, Matthews noted, was Tsi-tca’ci (Tsi-toa’ci?), about whom almost nothing is known. Matthews looked on as the singer proceeded to make ceremonial objects and anoint with various herbs and materials the one-sung-over (the patient). He captured his experience on paper with the help of Jake the Silversmith, as well as consultation with Chee Dodge, a Navajo trader and translator.²⁵ Across four days, he saw four elaborate sandpaintings executed—“large and beautiful sketches in 5 different colors drawn on the sanded floor of the medicine lodge to represent different cosmogonic and religious conceptions.” The one-sung-over was then directed by the singer to sit atop the sandpainting. Healing continued through singing and anointment with prayerstick bundles.²⁶ At the end of the procession of songs, the patient retired from the hoogan to lodges erected by friends and family. Meanwhile, the singer erased the sand painting and removed the sand to a discrete place beyond the camp. This extraction was done because the sand had been rendered infectious, the disease transferred from the patient to the sand-depictions of the Holy People.²⁷

Though extant for only a short period, Matthews had been able to illustrate the sandpainting before the singer and his assistants destroyed it and removed all trace of the sand. The ethnographer wrote to Powell: “I obtained accurate watercolor paintings of these sacred pictures and accurate description of the process of forming them (and this is a material and significant fact).” He considered himself lucky for having drawn the designs, for “soon after each picture was finished it was... erased, and the very sand on which it was drawn removed from the lodge.”²⁸ Matthews was also struck by the novelty—and, it follows, the ethnological value—of

²⁴ Significantly, the tools of Navajo medicine in chantways such as the Mountainway were not directed at the manifestation of disease, but rather pointed toward the factor causing it. In an example record by Father Haile in early 1900s, a bear was given as the cause of a woman’s injured shoulder—for the woman had stumbled into a mother bear’s territory, and regretted her violation of the space of the matron and her cubs. The bear in this case was the cause of the disease, and a Mountainway was given to appease the influence of the bear on the woman’s shoulder. In contrast to Anglo-American medicine at this time, the shoulder was not treated in isolation or in terms of acute pain, and instead the hataa’ii performed the chantway most appropriate to disease caused by creatures of the mountains, hence the Mountainway. Thus the individual components of the ceremonial were structured by the etiological assessment, for the various ceremonies and rites that completed the ceremonial complex were directed toward the identified causes of ailment. See Berard Haile, *Origin legend of the Navaho Enemy Way: Text and Translation* (Yale University Press, 1938).

²⁵ Matthews Notebook, 1884 San Mateo and Hackan, WMA.

²⁶ Matthews, “The Mountain Chant.” Matthews, *The Night Chant*. On the Mountainway, see Leland Clifton Wyman, *The Mountainway of the Navajo* (University of Arizona Press, 1975); Faris, *The Nightway*.

²⁷ Nancy Parezo phrases this nicely: “Dramatic and ephemeral, beautiful in that it attracts holiness actively, a drypainting exists in time and space only for the duration of a ceremony; when its use is fulfilled, it is ritually erased.” See Halpern & McGreevy, *Washington Matthews; Parezo, Navajo Sandpainting*.

²⁸ Matthews to Powell, October 1884 (draft), WMA.

his experience; to his knowledge, no white man had ever written about or mentioned Navajo sandpainting. Learning of the ceremonial complex—the songs, the sacred materials, the sandpainting, the lengthy duration—opened up a whole range of ethnological possibilities; a hidden treasure of ethnological data had been revealed to Matthews.²⁹

Although the surgeon was clearly captivated by his ethnographic project, his continued fieldwork was slowed by his employment. Matthews was in the midst of a transfer out of the field, to the Army Medical Museum and Library in Washington, DC. In fact, he had already begun his new post in the fall of 1884, but with news of a Mountainway ceremonial to occur in late October, Matthews had arranged for short stay in New Mexico to study this promising new ethnological ground. After seeing the Mountainway, he inquired with other medicine singers about the complicated rites he had witnessed. “I opened negotiations with three different medicine men, offering them as liberal terms for their services as I could afford, but only to meet with the most positive refusal.” Matthews needed an informant to help him understand the Mountainway he had seen, and near the end of his stay he “heard of an old shaman living beyond Nutria about 70 miles distant, who it was thought would be found less scrupulous than the others had proved.”³⁰

A well-known singer, Tall Chanter (Hatali Nez), arrived at Fort Defiance just before Matthews’s departure for the east. “With some trouble I concluded a bargain with him and he began his narration on the morning of the 16th. I have been engaged with him ever since, and do not believe I will be through with him until it is time for me to leave Defiance. No doubt I could profitably spend several months with him did circumstances permit.”³¹ His limited time with Tall Chanter notwithstanding, Matthews intended to move forward with a robust study of the Mountainway, and Navajo ceremonialism in general.³² He was captured by the intricacy of a religion many had thought not to exist, and surely felt himself charged with an ethnological duty to develop his secret knowledge into a viable scientific form for the advancement of the

²⁹ Although he did present a paper on the Mountainway in 1885, Matthews was annoyed to find that another Bureau of Ethnology employee, James Stevenson, had witnessed a Nightway in 1885 and rushed to publish his account. Scholars today credit Matthews with ethnological priority, and suspect Stevenson actually heard Matthews’s 1885 presentation on the presence of sandpaintings in Navajo ceremonialism.

³⁰ Matthews, “The Mountain Chant.”

³¹ Matthews to Powell, November 1884 (draft), WMA.

³² Though drawn away from the field, Matthews first communicated his findings on Navajo ceremonialism in 1885 in *American Naturalist*, where he emphasized the presence of sandpaintings. Washington Matthews, “Mythic Dry-Paintings of the Navajo,” *American Naturalist* 19 (1885): 931-39. An abstract of this paper was also published in the *Bulletin of the Philosophical Society of Washington* 8 (1885).

discipline. Even during his post in the capital, he may have reasoned, he could still begin a study of Navajo ceremonialism. So, it followed, if he could not witness a chantway in Navajoland, he could nevertheless bring a Navajo singer to the Capitol. So he asked Tall Chanter to journey east.

Tall Chanter and Teaching Chantway Practices

The renowned Diné medicine singer Tall Chanter (c. 1841–1929) came to Washington, DC, in November of 1885. Like his colleague Laughing Singer, Tall Chanter had been trained as a *hataalii* before the events of the Long Walk, 1864-68, and had spent time learning the Nightway chant under his father, Mr. Cane.³³ From his home in Rainbow Springs, in the southeastern portion of Navajoland, Tall Chanter journeyed to the Anglo capital on behalf of the Navajo. The trip was provided through an arrangement with the Navajo Agent Dennis Riordan, and included an introduction to the new American President, Grover Cleveland. Tall Chanter and his Navajo entourage spent time in Washington meeting various scientific and governmental personnel, with Chee Dodge as the Navajo interpreter. But Tall Chanter was also scheduled to visit and work with Washington Matthews at his post at the Army Medical Museum. After his initial visit with Matthews following the Mountainway in 1884, Tall Chanter and Matthews had clearly forged a bond. This bond strengthened in Washington, and thereafter Matthews relied on Tall Chanter as a consistent tutor in all manner of Navajo ceremonialism.

Tall Chanter required Matthews's trust and goodwill in order for training to occur, and Matthews reciprocated the respectful relations of his "elder brother."

[Tall Chanter] perhaps has a better knowledge of the legends than any other man in the tribe. Before he would confide any of his secrets to the author he said: "The chanters among the Navahoes are all brothers. If you would learn our secrets you must be one of us. You must forever be a brother to me. Do you promise this?" He has ever since addressed the author as Sitsi'li, "My younger brother," and has in turn been called Sinái, "My elder brother."³⁴

Singers formed a sort of fraternity of practitioners, bound by their shared expert knowledge of Navajo cosmology. Singers took the healing role seriously, and they built their reputations

³³ Faris, *The Nightway*.

³⁴ Matthews also claimed to have been initiated into chantway practice, although the details of this initiation are hazy. See Washington Matthews, "A Navajo Initiation," *Land of Sunshine* 15 (1901): 353-356.

through the patients and communities they served. Across the scattered range of Navajo bands, singers could become cohesive practitioners, linking family groups across diverse terrain.

Ceremonial training under singers like Tall Chanter and Laughing Singer was a system of practices designed to maintain *hózhó*, harmony and balance, of people and environments. Navajo medicine, like all systems of healing, treatment, and etiology, composed a body of knowledge directed at states of health and wellbeing of the human body.³⁵ Much like Anglo medicine, too, this corpus of knowledge was distributed among different specialists, and ailing Navajo went to a number of practitioners based on the condition that ailed them. Herbalists, diagnosticians, prayer-makers, and chantway singers were termed *nahalaii* (“practitioners”), indicating skill and knowledge in matters of healing. *Nahalaii* possessed deep knowledge of the stories of their people and the intricacies of the desert canyonlands and arid mountain landscapes, places that seemed barren to Anglo eyes but which *nahalaii* could navigate with skill to access the variety of micro ecosystems where important plants grew and creatures resided.³⁶

Medicine singers (*hataali*) such as Tall Chanter (*Hatali Nez*) and Laughing Singer (*Hatali Natloi*) had undergone additional training, and received the honorific title given to learned singers, *hataalii*, who had mastered at least one entire chantway.³⁷ Chantways were the most

³⁵ Anthropologists have long sought to understand the Navajo ceremonial system through organization of the elements in formal, philosophical terms. Aside from Matthews’s pioneering work, Leland Wyman and Clyde Kluckhohn developed a Navajo “ceremonial classification” based upon the apparent importance of the ceremonials. Leland Clifton Wyman and Clyde Kluckhohn, *Navaho Classification of their Song Ceremonials* (American Anthropological Association, 1938). Haile divided ceremonials by the presence or absence of a rattle, which distinguished a chant (with rattle) from a rite (without). Berard Haile, “Navaho Chantways and Ceremonials,” *American Anthropologist* 40, no. 4 (1938): 639-652. Subsequent authors built upon these classifications. Kluckhohn later investigated the notion of harmony and balance and the role of religion in maintaining these elements. Clyde Kluckhohn, “The Philosophy of the Navaho Indians,” *Ideological Differences and World Order* (1949): 356-384. Gladys Amanda Reichard sought the philosophical underpinning of Navajo lifeways in part through chantways and rites of Navajo religion. Gladys Amanda Reichard, *Navaho Religion* (Pantheon Books, 1950). Spencer organized chantways according to mythological content. Katherine Spencer [Halpern], *Mythology and Values: An Analysis of Navaho Chantway Myths* (University of Texas Press, 1957). Wyman’s later work analyzed individual chantways and, like Reichard and Kluckhohn, worked toward a categorization scheme for the interfacing components of Navajo religion; see, for instance, Leland Clifton Wyman, *Sandpaintings of the Navaho Shootingway and the Walcott Collection* (Smithsonian Institution Press, 1970).

³⁶ Herbalists concocted remedies and purgatives for various illness and were called upon for minor and major issues alike. Serious ailments could be identified by diagnosticians, often called “hand tremblers” for their manifest process of divination. Diagnostician nahalaii discerned the sources of ailments and, if more simplified prayers or herbal formulae would not suffice, recommended chantways and, in turn, the hataalii who could perform them. While the specialist nature of nahalaii was ideally directed toward wellbeing, witches, too, were considered nahalaii for their ability to “influence the course of events by ceremonial means.” Kluckhohn and Wyman, quoted in Maureen Trudelle Schwarz, *Blood and Voice: Navajo women ceremonial practitioners* (University of Arizona Press, 2003). Witchcraft was defined by Kluckhohn as “the influencing of events by supernatural techniques that are socially disapproved,” and witches were also present within the complex of Navaho “medicine,” although these were considered not healers but disruptors of harmony. See Clyde Kluckhohn, “Navaho Witchcraft,” *Papers of the Peabody Museum, Harvard University* (1944).

³⁷ I have kept Matthews spelling for singers, properly named, as “Hatali.” I use hataalii in reference to Navajo singers in general, following contemporary preferences by Navajo.

powerful remedy for ailment one could seek in Navajoland.³⁸ *Hataalii* administered two, five, and nine-night chantways, and they were necessarily skilled in a paradigm of medicine focused on blessing and dispelling, returning patients to a state of happy equilibrium, to *hózhó*.

Tall Chanter had spent years mastering the Nightway. But Tall Chanter could not recreate a Nightway in Washington because the Holy People, the Navajo deities called forth through chantways and compelled to use their powers to restore balance, did not dwell in this land. The Navajo theory of curing associated Holy People with particular etiological phenomena. Holy People were drawn to the site of healing through the songs, prayers, and sandpaintings presented by the *hataalii* and his assistants. As Leland Wyman explained of early 20th-century ceremonials, conjuring the Holy People acted as a catalyst for the transference of powers—positive powers to the patient and negative forces away from the patient to the Holy People. “In a ceremonial, the Holy People, the supernatural beings invoked, are the judges of the completeness and correctness of the ritual, and if satisfied they are compelled by the ethic of reciprocity to restore universal harmony and thus cure the patient. Hence prayers and offerings in Navajo ceremonials are invocatory and compulsive, to attract and obligate the holy ones, not to glorify or thank them.”³⁹

In lieu of a full presentation of a chantway, it appears Tall Chanter told the ethnographer myths and histories that undergirded the chantways. According to Navajo thought, Holy People unfurled the designs for sandpaintings on the clouds and taught them to various storied heroes and heroines, who had passed them down through generations of *hataalii*. Contemporary singers used the designs, prayers and songs to call forth the Holy People. Tall Chanter also likely imparted some knowledge of the flora and fauna of Navajoland and constructed some ceremonial objects such as prayer-sticks that were used in a chantway.⁴⁰ Since he had seen a chantway, too, Matthews used his time with Tall Chanter to ask specific questions. Though there is no record of

³⁸ In English, chantways have been typically denoted as a form of directed “chant,” often toward something—hence the suffix “-way” that often appends a ceremonial’s name. Chantway ceremonials were complex, multi-day processes that brought together not only the singer and the patient, but also a host of assistants, health-seekers, and the patient’s family and friends. To Westerners and Navajo alike, chantways consisted of songs and spoken prayers, sudatory (steam bathing), herbal anointment, ingestives and emetics, smoking, ritual performance, and the depiction of godly power in sandpainting. My use of the term chantways follows Spencer, *Mythology and Values*.

³⁹ Wyman, *The Mountainway*, 5.

⁴⁰ See Matthews, 1885 Flora/Fauna Terms, WMA; Matthews, *The Night Chant*.

Tall Chanter performing the Mountainway, he surely knew much about this important chantway and provided clarifying information about it to Matthews during his time in Washington.⁴¹

Tall Chanter met with Matthews in autumn of 1887, when Matthews returned to New Mexico to witness a Mountainway under the singer Gordo. Matthews arrived early and stayed late (although he became violently ill from the nearby rancher's well-water, too). At this camp, he documented the construction of the medicine lodge and the purgative rites featured in the first days of the ceremonial and made many sketches of sandpainting designs and ceremonial objects as the nine-night event proceeded. While the singer Gordo appeared to welcome Matthews, he did not offer the Anglo visitor commentary on the Mountainway performance. But Matthews used his former teachers, Tall Chanter and the translator Chee Dodge, both of whom helped the ethnologist better understand the events before his eyes.

Matthews took his notes and experiences and, back in Washington, transformed them into a scholarly text. Through knowledgeable participants in Navajo life such as singers like Tall Chanter and hired translators, Matthews was able to synthesize an account of the Mountainway, drawn from his experiences of the ceremonials performed by Tsi-tca'ci in 1884 and Gordo in 1887. In 1888, in the *Annual Report of the Bureau of Ethnology*, Matthews published his first major work on Navajo ceremonialism, *The Mountain Chant: A Navajo Ceremony*, a distillation of a nine-night procession of songs, prayers, dances, sandpaintings, anointments, fumigation, and a host of other sensorial events, into an illustrated, 88-page text.⁴²

The Mountain Chant (1887)

The Mountain Chant implicitly pursued a complicated ethnographic question: how to represent a nine-night ceremonial event that escaped the senses? Matthews was adept at taking notes, and his ethnographic chops were in order from his study of Navajo language and material culture. But sandpaintings, songs, and prayers (and the symbolism that gave them meaning to patients and singers alike) presented a more challenging puzzle for permanent record-keeping. Matthews had sat through the length of the Mountainway, some elements more than once, and

⁴¹ Even if unable to perform the Mountainway ceremonial, Tall Chanter would have been considered a knowledgeable onlooker, or what has been called an authoritative witness, one who knew the cosmology the singer referenced and could critique or commend as needed. See Faris, *The Nightway*.

⁴² Matthews, "The Mountain Chant." While the volume identifies its publication date as 1887, it was not actually published until 1888, which accounts for Matthews's ability to include information collected in Fall of 1887 in the article.

now he needed to convey that experience—the entirety of the ceremonial as a collage of material, visual, haptic, and sonic elements—and render it legible to an interested, scientifically inclined audience.

Matthews's 88-page text began with a long mythological account of the gift of the ceremonial, the "Myth of the origin of *dsilyídje qaçàl* [literally: song toward the mountains]," and only after some thirty pages did he proceed to describe the actual events of the Mountainway.⁴³ His account of the ceremonial was synthetic, a blend of his two viewings of the Mountainway in addition to information provided by Tall Chanter, Chee, and Jake, and perhaps others. His description of the sequence of events focused on conveying the temporal sequence without deviation in specifics. Sandpainting preparations, for instance, were described in the text of the chantway sequence, but the sandpainting itself was bracketed.⁴⁴ Matthews "deferred until all might be drawn together," and he placed a thorough description of the four sandpaintings associated with the ceremonial in a third section of the work. Songs transcriptions, too, he placed after the sequence of events, in a fourth section. Songs and prayers were transcribed in Navajo, followed by a literal English translation and another, more poetically rendered free translation. Each song was followed by a short explanation of the song's meaning or role in constituting the chantway.

The text included numbered paragraphs and an expanded table of contents, to facilitate the reader's orientation in the text and to enable references to be made to prior or forthcoming sections. For example, the first sandpainting, "The Home of Bear and Snakes," was mentioned in the main text in paragraph 93, and the reader was alerted that a description would come later, at paragraph 160. Upon reaching paragraph 160, the reader could also reference the mythological account (paragraph 53) that the painting evoked. Songs, too, were indexed throughout the sequential text as well as the later section featuring the songs in Navajo and English translation.⁴⁵

In all, *The Mountain Chant* was a prototype of ethnographic documentation, and its use of cross-references set a high standard for an integrated, dynamic description of a cultural world. Matthews gave ample detail in a disinterested tone while also conveying his esteem for Navajo

⁴³ The rendering of "dsilyídje qaçàl" is from Matthews's orthographic scheme. For the idiosyncrasies of Matthews's Navajo orthography, its changes over time, and its retooling for contemporary linguistics, see Halpern & McGreevy, *Washington Matthews: Studies of Navajo Culture*.

⁴⁴ See, for example, Matthews, "The Mountain Chant," 422.

⁴⁵ Matthews, "The Mountain Chant." For a discussion of the various components of the Mountainway and developments since Matthews's time, see Wyman, *The Mountainway*.

ceremonialism, especially the long and complicated training of singers and their ability to reproduce the ceremonials from memory alone. *The Mountain Chant* was the first sustained study of a single Indian group's "religious" activities to grace the pages of the Bureau of Ethnology's *Annual Report*. Similar in its sustained focus to work by a colleague, Frank Hamilton Cushing on Zuni pottery and ritual carving, Matthews's work on the Mountainway was remarkable for its systematic portrayal of Navajo ceremonial life—or one possible iteration of it. In it, Matthews had presented a model for a concentrated and particularistic mode of ethnographic study: the complexities of indigenous ceremonial systems could be organized and described in logical and signposted forms, and links between different components (as in the sequence and the sandpainting and the myth), established in indices and cross-references, could be drawn to approximate the totality of the event.⁴⁶

A Science of Ceremony

Throughout the 1880s and 1890s, Matthews consistently pushed forward a vision of a systematic study of ceremony. Drawing on the recent expansion of the study of religions to encompass Native American spiritual practices, Matthews argued that a science of ceremony would attune researchers to important components that were at risk of being lost amid Anglo-American settler colonialism. Documentation and preservation of Indian belief systems was of chief importance because, as the most esoteric and often secretive elements of indigenous society, "religions" were most in danger of being lost without even being recognized in the first place.

In an 1897 article in the *Journal of American Folk-Lore*, Matthews presented a case for the "Study of Ceremony."⁴⁷ Having asked a "scientific friend, an anthropologist," what Matthews might name a science of ceremony, the friend replied, "you can create no science of ceremonies, and can form no laws concerning them." Matthews demurred; he aspired to bring the study of ceremony to a scientific level but recognized the resistance within the scientific world. But a science of ceremony offered many benefits from Matthews's point of view. "I

⁴⁶ An interesting hiccup surrounds *The Mountain Chant* in that part of the text was "suppressed" by the Bureau of Ethnology. The suppressed portion, which contained a description of the sexualized public performances that came in the final evening of the event, which Matthews had elsewhere described as "horrid." After protests from scholars, many lamenting the patronizing censorship of the Bureau and government science in general, a supplemental "The Suppressed Part of the Mountain Chant" was published in 1892. See Matthews, "The Suppressed Part of the Mountain Chant," Manuscript 4834, NAA.

⁴⁷ Washington Matthews, "The Study of Ceremony," *Journal of American Folk-Lore* 10, no. XXXIX (1897): 257–263.

believe, as the result of an extensive experience, that ceremony offers material for the study of human development equal to that offered by art, government, legend, or any other subject of ethnologic investigation.”⁴⁸

Matthews gave a list of problems that had prevented ethnographers from conducting a scientific study of ceremony and outlined the interlocking requirements for an ethnography of ceremony. To begin, he asserted, researchers must spend time in the field. “The gleaners of ethnological notes have been, heretofore, mostly of the wandering kind,” wrote Matthews. The ethnologists needed to plant roots in the community, needed to acquaint themselves with the informant and gain his or her trust. Given time and acceptance, the fieldworker would find new information available to him, especially regarding ceremonial matters. “Ceremony, even of a merely worshipful character, is one of the things about which people are sensitive and reticent,” Matthews acknowledged; by building relationships, the anthropologist’s subjects would be more open with sacred matters. This “insider” status also enabled the fieldworker to comprehend the overall spiritual system better, for these are not to be fully comprehended until the antecedent or more secret elements of the rite are also known.”

To make a science of ceremonialism, the elements of time and trust had attached to an unbiased mind. Matthews believed that “until recently there were very few white men who could entirely divest themselves of their early bias, who could altogether rid themselves of an inbred contempt for pagan rites, or who, in the presence of pagans, conceal their antipathy to the performance of what George Catlin calls ‘hocus-pocus.’” Calling out the famed painter of Indian life, Matthews emphasized that unemotional interest in ceremonial practices was essential, not just for scientific credibility, but also because “the slightest evidence of disdain on the part of the inquirer easily closes the door to knowledge.” Just as the Navajo medicine singers knew the Holy People would respond only to correct comportment and legitimate appeals for healing, the ethnographer needed open-mindedness to witness the full extent of a group’s ceremonialism, both to gain the assurance of their hosts and to endear themselves as respectful visitors, even in the face of potentially shocking or strange customs.

⁴⁸ Matthews, “The Study of Ceremony.” In this text, he toyed with assigning the study of ceremony a scientific nomenclature—Jesse Walter Fewkes, a researcher working nearby at the Hopi Mesas had already proposed a field to be called “ceremoniology”—but, ultimately, he admitted he could not think of a proper term to designate the proposed scientific field.

With clear distaste for haughty ethnologists, Matthews insisted that ethnographers had to take seriously their human subjects, to be willing to be surprised by their ingenuity and complexity. This also required patience for acts that appeared repetitive, monotonous, silly, or tedious to the inquirer, for “in omitting to note the apparently most trifling particulars, he may lose the most valuable material for comparative study.” Here, Matthews linked patient, respectful, in situ observation to scientific credibility. This did not mean scholars needed to hold Indian life up to the level of Anglo-American “civilization,” but it did mean they needed to take Native spiritualism on its own terms.

Finally, Matthews provided a practical tip for ethnological inquiry: fairly pay informants for information they held dear. Informants with high social status, Matthews argued, understood the value of their knowledge. The ceremonial specialist, it followed, “is not willing to surrender all he knows to a stranger for a trifle. If he thinks he will receive but a cup of coffee and a plug of tobacco for his pains, he likely to impart information to that value and no more.” At times the knowledge may be too great or too precious to convey to an outsider, Matthews acknowledged. To foreclose an anticipated scorn for “conservative” Indians from his readers, Matthews drew a parallel to the Freemasons of the time, to demonstrate the power of a shared bond. Many knowledge specialists, Matthews reasoned, made good judgments of who can and cannot access knowledge, and Indians were no different from whites in this regard. Matthews’s estimation of his informants, grounded in his experience with savvy chantway singers like Laughing Singer and Tall Chanter, signaled that the science of ceremonialism must be humble and conscientious in its pursuit—there was simply no other way about it.

If ceremony researchers carefully practiced these traits, they might penetrate “rites of an esoteric character.” The ethnologist may eventually gain permission, as he had, to witness the obscure practices of ceremonial specialists. He insisted that “some of the most interested survivals in the history of human development are to be found in the rites of secret societies.”⁴⁹ This privilege was earned. One could not expect to waltz into an Indian settlement and simply chat up the priest about the most intimate and sacred parts of spiritualism.

While Matthews’s call for a science of ceremony had a salvage objective, it also forwarded a notion that Indian religion was not naive superstition but rather highly complex in

⁴⁹ Matthews, “The Study of Ceremony,” 259.

its theories of cause and effect. Though he believed them to be incorrect, Native “superstitions” did not differ significantly from traditional religious beliefs of Anglo-Americans. In this context, Matthews argued for religious freedom for Indian communities, pointing out a stark hypocrisy in the rights allowed to indigenous people corralled into the United States: “Religious freedom is assured to all within the borders of this ‘glorious republic,’ except to the original owner of the soil. He alone may not worship according to the dictates of his conscience.”⁵⁰ Restrictions on Indian “religions” were placed when communities received government rations or other material support, but the suppression of these rites would, to Matthews mind, erase potential historical artifacts embedded in ceremonials.

Matthews’s proposed science of ceremony was liberal in its treatment of Native practice, but parochial in who was deemed an authoritative researcher, who could produce legitimate scientific data for comparison. Even though he had received formal training under Laughing Singer and Tall Chanter and had strong relationships with knowledgeable Diné informants such as Ben Damon, Chee Dodge, and Jake the Silversmith, Matthews ultimately believed an outsider—himself—the proper figure for gathering information about Navajo ceremonialism. In print, Matthews took an equivocal position on the ability of Navajos to explain their “religion” to outsiders. “There is little to be gained by asking a Navaho direct questions about this. Learned controversialists and theologians, capable of analyzing and discussing their faith, have not arisen among them, or, if they have, they cannot easily communicate their philosophy to us.”⁵¹ Indeed, he placed the abilities of the ethnographer as the foremost tools for a communicable description of Navajo ceremonialism; the ethnographer was thus a sort of cypher or medium through which Navajo “religion” could be made legible for Anglos.⁵² In lieu of an informant’s explication of a complicated knowledge system, Matthews argued indigenous beliefs were best represented by the ethnographer; in his position as a scientific-minded outsider looking in, only the ethnographer was able to capture and analyze data relative to the trends in human science.

The “civilized” scholar was, to Matthews, the only one equipped for comparative analysis in a scientific mode. He situated Indian religion on a scale of intellectual advancement, in which

⁵⁰ Matthews, “The Study of Ceremony.”

⁵¹ Matthews, *Navaho Legends*, 33.

⁵² Matthews, *Navaho Legends*, 33.

he saw the European secularized social practices and events as a holdover of previous religious (non-scientific) beliefs and practices. “A comparative study of worship will show that the same principles control the forms of worship among the lowest and the highest.” Further, comparative study would reveal the historical evolution of ceremonialism into its secular, social forms at the turn of the 20th century. Thus, Matthews asserted there was wide field for ceremonial study, “not only among the barbarous races of the old world, and rustic Europeans, but among the most enlightened and exalted members of our own race. Among the latter we trace, with astonishing clearness, the survival of savage customs.”⁵³ Study of ceremonial or esoteric practices, then, proposed to provide insight into human sociality, from the “primitive” to the “civilized.”

Matthews’s science of ceremony did not radically depart from the dominant ethnological theory of his day. He implicitly accepted the reigning social theory of the late nineteenth century, social evolutionism and its linear scale of culture from savage to civilized, but stressed that the judgment of a culture within a hierarchical scheme should not alter the analysis of its contents. He knew, too, from his own experiences that Indians could impress with sophisticated systems of reasoning, if only the scientist kept an open mind. These ideals, progressive for their time, had been forged by Matthews’s experiences with Navajo singers and his struggle to produce a scientific monograph about their “religion” and its myriad facets. He had accumulated notebooks full of data, from a range of events that touched on all of his senses. But even though he had his own prototype for a ceremonial monograph, he did not present a practical method to budding ethnographers. The science of ceremony was a goal for a data-rich field; ethnographers, it seemed, needed to devise their own methods to fill notebooks with scribbles they could eventually turn into ethnological information. “The civilized scholar has abundant material from which to study their religion,” Matthews quipped, “and must do the analyzing himself.”

The Problems of Chantway Documentation

In *The Mountain Chant*, Matthews had demonstrated a method for dealing with problems of sensorial complexity. But documentary method was little discussed by Matthews or his peers. Scientific propriety prevented a public discussion of the method, and formalized training in anthropology would only begin in the 1890s.⁵⁴ While paper technologies such as circulars

⁵³ Matthews, “The Study of Ceremony.”

⁵⁴ Mark, *Four Anthropologists*; Hinsley, *Savages and Scientists*.

dictated documentary methods, as shown in the first chapter, they were largely self-contained paper technologies of data accumulation in that they provided both the guidance and medium for inscription. Audio and visual technologies, moreover, presented a new potential for ethnographic documentation to produce (presumably) a more objective form of data, since it ostensibly removed the collector's bias. But these nascent media could only supplement complex studies, such as those of ceremonialism, which required attention across the sensorial spectrum.

Although he had shown a prototype of ceremonial ethnography in *The Mountain Chant*, Matthews did not publish a guide to ethnography. But he had developed a model for his own continued ethnological research into Navajo ceremonialism. So, after finishing his study of the Mountainway in 1887, Matthews began research into Navajo ceremonialism that linked to larger ethnological concerns percolating in journals and conferences in the US around Indian folklore and mythology, including the study of Indian "opinions" as Powell and others would come to identify it (what Powell deemed "sophiology"). Matthews focused his research on two interlinked areas. First, drawing energy from the thriving field of folklore, Matthews continued to collect and study Navajo histories (what he called "legends") and myths and other stories of the divine. These stories gave Matthews insight into chantways in general, and from this ground Matthews pursued a second project wholly devoted to another important Navajo chantway, the Nightway. Also stretching nine nights, the Nightway targeted ailments of the head and paralysis. (While it is not clear if this was the case in Matthews's time, more recently the Nightway has been associated with "balanced relationships which involve gender and authority."⁵⁵). Like the Mountainway, it too could only be performed during the winter, when the potential of frost kept bears and snakes away from Navajo camps.

As he had done previously for linguistic and material ethnography, Matthews divided his labors of documentation, devoting some notebooks to the project of "legends" and others to the Nightway. During the 1880s and 1890s, he kept separate notebooks for song transcriptions, "ceremonial tech," flora and fauna terms, weaving notes, and dictionaries and phrasebooks. Within discrete notebooks, too, Matthews penned a host of miscellaneous items of information. Nearly all of this information needed to be conveyed to him through dialog or hands-on training with a singer; little could be passively witnessed or interpreted without a knowledgeable guide. A

⁵⁵ Faris, *The Mountainway*, 236.

well-organized research plan could ease Matthews's labors, but only through his relationships with Tall Chanter and other singers was he able to understand the ways in which chantways reflected Navajo cosmology, and how histories and myths constituted the healing materials of chantway practice.

Strong relationships were part and parcel of chantway practice, and Navajo medicine men often emphasized this point. *Hataalii* healed patients but also invigorated Navajo life with meaning. But Navajo singers were not "gifted" or special individuals with an inborn or hereditary power.⁵⁶ Thus, *hataalii* needed to pass on their mastery, for it was only through apprenticeship that new medicine singers could become adept in the suite of practices that composed chantways. Singers-in-training were often the sons of practicing *hataalii*. Tall Chanter and Laughing Singer, for instance, both reckoned their chantway knowledge through a lineage of training under their fathers—Tall Chanter under his father Mr. Cane and Laughing Singer under his father No Sense Old Man. In turn, Tall Chanter and Laughing Singer took on students. In addition to Matthews, Tall Chanter and Laughing Singer both trained a singer named Hosteen Klah (Mr. Left Handed), who went on to become a famous medicine man, known even beyond Navajoland. Tall Chanter in particular passed on the Nightway to a number of trainees.⁵⁷

Chantway students might also be chosen from among the assistants who aided the chanter during a particular ceremony.⁵⁸ Such assistants were needed for preparation of the colored earth, for the binding of prayersticks, for the adornment of masks, for making mutton stew, and for the performances and songs that helped sing away the disease of the patient. Jake the Silversmith, who often translated for Matthews, was one such assistant for chantway singers. Though little is known of Jake's training, he mastered the Apache Windway, a five-night ceremonial that offered healing from afflictions caused by wind and weather, and which Matthews witnessed Jake perform in 1891. Assistants could thus be drawn from Navajo visitors whom the singer knew, as well from visiting *nahalaii* (practitioners of healing) and *hataalii*, whose expertise helped keep the ceremonial running smoothly. Each ceremonial, then, was an opportunity for an aspiring

⁵⁶ Faris, *The Mountainway*, 235.

⁵⁷ For a discussion of training lineages, see Faris, *The Mountainway*, 82-83. For Hosteen Klah, see Franc Johnson Newcomb, *Hosteen Klah: Navaho Medicine Man and Sand Painter* (University of Oklahoma Press, 1964).

⁵⁸ Matthews, 1884 San Mateo and Hackan, WMA.

singer to learn or perfect a song, memorize sandpainting motifs, and learn the taxonomy of prayerstick recipes, in addition to other ceremonial content.⁵⁹

Teaching also occurred outside of ritual settings. The training period was an extended one: there were many songs to learn, stories to master, and paintings to perfect. An aspiring singer needed an impressive memory and devotion of time; sand paintings could not be sketched or illustrated permanently, so trainees practiced sandpaintings in sections, perhaps dividing up elements into successive days.⁶⁰ In the end, they were expected to commit the sandpainting design to memory, and each time the ceremony was performed they instructed a host of assistants in the illustration of a design that was considered to be descended unchanged from the Holy People who first presented it. Although singers continued to restrict permanent illustration of a fully detailed sandpainting, by the time Matthews was working with Tall Chanter and Laughing Singer, there appears to have been some flexibility in the use of permanent illustrated aids for sandpainting memorization.⁶¹

Clearly, a singer-in-training needed a strong memory and constant practice. Song mastery was a daunting task for students, for *hataalii* were expected by their peers to perfectly know the hundreds of songs that might accompany a chantway. Trainees learned the formula that directed, as Matthews put it, “songs of sequence”; these featured many repeated lines with a singular changing element that altered the scale or embodiment evoked—from the sky, to the mountains, to the hills, to the canyonland.⁶² Ceremonial paraphernalia, too, were composed of a myriad of objects, and attention needed to be paid to the circumstances of their collection and the orientation of the composition as an object was made. Singers prepared invocatory prayersticks or *k’eeet’áán* (ceremonial bundles of various ingredients), which would be deposited outside of the camp such that Holy People could easily access them. Prayersticks hailed specific Holy People. Based on the ailment and a host of environmental and patient-specific criteria, the singer determined which Holy People to call to offer blessings or release their hold on the patient.⁶³ In each chantway, *Hataalii* tailored prayersticks and their associated Holy People for the patient’s

⁵⁹ Matthews, Jake Windway Notebook, WMA.

⁶⁰ Matthews, *The Night Chant*. If a sandpainting did not contain the full image, the appeal to the Holy People was not communicated.

⁶¹ Matthews, *The Night Chant*.

⁶² Washington Matthews, “Songs of Sequence of the Navajos,” *The Journal of American Folklore* 7, no. 26 (1894): 185-194.

⁶³ Wyman, *The Mountainway*, 42-56. Readers interested in the complexities of prayersticks should see this analysis, especially the discussion of prayerstick diagrams given to Berard Haile by Yucca Patch Man.

ailment, employing a variety of herbs, wood, bark, feathers, leathers, and other materials.⁶⁴ Thus trainees had a host of ingredients, recipes, and invocatory associations to master to ensure the success of a given chantway.⁶⁵

Over the winter of 1891-92, Laughing Singer and Washington Matthews spent several weeks of instruction in the methods of making ceremonial paraphernalia. These were mostly materials that singers and assistants would produce in the lead-up to a Nightway, and which would be disassembled, distributed, or otherwise transformed during the course of the ceremonial (a singer had his own *jish*, or medicine bundle, that contained objects such as masks or smoothing batons that would be used in many ceremonials).⁶⁶

Laughing Singer, as described at the beginning of this chapter, was known widely as a jovial and generous singer, and he appeared accommodating to Matthews's requests for training in the material ceremonial preparations. Laughing Singer explained or demonstrated the production of multiple ceremonial objects: a turquoise bead noise-maker; a buffalo horn filled with tallow of different mountain animals; an eagle-bone whistle; charms made of turkey and eagle parts; thin plumes and slender twigs; and a variety of "kethawns," or prayer-stick offerings of twig, feathers, and other materials bound with string. Of these, Matthews wrote ingredients and drew sketches of item construction in his notebook.

Matthews also lent a hand in the actual making of ceremonial items. During one training session, Matthews had trouble fully understanding an intricate explanation of the required properties of a yucca-leaf drumstick, and Ben Damon, the translator, could not render all Laughing Singer's words into English. Laughing Singer offered to show Matthews how to construct a drumstick from yucca leaves, having been told "anybody may make the drumstick." In this instance, Laughing Singer demonstrated a belief in the benefits of learning through doing. Several days later, on December 30, 1891, Matthews, Ben Damon, and Laughing Singer set out

⁶⁴ As Berard Haile phrased it, in light of his tutelage under the Mountainway practitioners Yucca Patch Man and Salt Water Man, "the offender has not so much injured or abused the animal form, as he has [offended] the human or supernatural [Holy People] form of the animal in question. Because of his actions or omissions, therefore, this supernatural being ... is 'exercising a hold' on the transgressor." Haile is quoted in Wyman, *The Mountainway*, 43.

⁶⁵ In 1890, Chee Dodge provided Matthews with a "list of Navajo healing dances" featuring 16 different chantways. See Matthews, "List of Navajo Healing Dances," WMA; Halpern & McGreevy, *Washington Matthews: Studies of Navajo Culture*, 133-139.

⁶⁶ For *jish*, see Charlotte J. Frisbie, "Traditional Navajo women: Ethnographic and Life History Portrayals," *American Indian Quarterly* (1982): 11-33.

to obtain the materials for the drumstick. Matthews recounted in his notebook the attention to detail and perfectionist sensibilities Laughing Singer (here, Natloi) brought to their search:

I go out to find yucca for drum sticks with Ben and Natloi in P.M. We hunt an hour and pass a hundred yuccas before we find the right one. Then he isn't quite satisfied, the leaves must be large tall straight, green, introverted. The four good leaves must be found on it. The fringes & tops of all are taken off and put into heart of plant, leaves must be pulled out not broken off. Must be made in house — peculiar way of making. How shall I describe, east & west put back to back, tied together, lateral binding strings put in, two strands tied round, binding-strings passed through, 5 lines once for each night; finished out with more strands mealed.⁶⁷

Though difficult to translate into written form (“how shall I describe...”), Matthews attempted to adequately capture the metaphysics of orientation in chantway preparation. He knew that cardinal directions had particularly meanings for Navajos, especially the East, the direction from which goodness came. But in learning how to make prayersticks and drumsticks, he also came to appreciate the deliberate ways that singers held materials in certain directions or grasped them in specific ways. In short, Matthews found himself oscillating between different scales of detail in an attempt to manage all of the information at hand—drumstick creation required attention to its physical orientation in space, which required an understanding of the importance of directions in Navajo cosmology.

While the yucca-leaf drumstick would normally be disassembled after use in a ceremony, because Matthews constructed one outside of a chantway event under Laughing Singer's guidance, he was allowed to keep the drumstick. Indeed, it may have served as a reminder of the necessity of experiential training, as Matthews could not render into text the method of drumstick construction, only its related theory of orientation. During a presentation on the drumstick and its partner, the basket drum, Matthews explained:

How then, it may be asked, have I come into possession of my drumstick? It was made for my instruction by a shaman, not in the medicine-lodge, but in my own study. Such it is his privilege to do for any recognized student of the rites. I have had several drumsticks made and pulled apart for my instruction, and I have made them myself, under the observation and criticism of the shaman. This one I was allowed to retain intact. No one

⁶⁷ Matthews, 1891-92 Ceremonial Tech and Notes, WMA.

has ever sung or prayed over it. It has never been used in the rites. It was therefore unnecessary to tear apart, to release its soul and sacrifice its substance to the gods.⁶⁸

Here, Matthews interpreted Laughing Singer's permission as a rule of Navajo knowledge: when not tied to Holy People (and thus to potential misfortune from offense), knowledge could remain stabilized in its material form, separated from the mind of the singer. He could see the separation of training from the performed ritual context, for healing knowledge needed instruction and practice. And thus, he justified, he could convey this information to Anglo audiences—at least, if they were willing to watch him work: Matthews could not explain how to make the yucca-leaf drumstick, he could only demonstrate its construction. Unfortunately for his audience, he had no fresh yucca on hand:

Since the shaman cannot adequately explain in words, to the devotees who assist him, how the stick is made I shall not attempt the task for you tonight. I have learned how to make it; but I have, now, no fresh yucca leaves on hand to illustrate the process of making. So I shall say nothing more of the process. Anyone who is not satisfied with this decision may come with me to the yucca-covered deserts of Arizona and there I may show him how to make a drum-stick.⁶⁹

Here, Matthews recognized that the transfer of knowledge was typically experiential and context-dependent; given time, he may have been able to write out the construction instructions, but recognized that being there, as witness and participant to construction, was far easier to convey than language and text. This was an implicit recognition of Navajo forms of training—a pupil should learn from direct study and practice under knowledge-keepers. Yet, by training Matthews and recognizing his documentary tendencies, Laughing Singer also hinted that he was open to expanding the modes of knowledge communication, that writing and drawing might supplement teaching or aid comprehension in singers-in-training.

While prayer-sticks and drumsticks made outside of the ceremonial context could be given over to Matthews for display and study, singers did not extend this privilege to objects that had been used ceremonially. In an early meeting in New Mexico, Matthews had asked Tall Chanter to sell gourd rattles and masks used in a *Nightway*. To this, Tall Chanter explained that he could not sell because of the difficulty of obtaining the correct materials for these objects and,

⁶⁸ Washington Matthews, "The Basket Drum," *American Anthropologist* 7, no. 2 (1894): 202-208.

⁶⁹ Washington Matthews, "Some Sacred Objects of the Navajo Rites," *Archives of the International Folklore Association* 1 (1898): 227-247.

moreover, to sell them would be to “sell the properties of the greatest gods they have. They live in the mountains around here and if they found the sacred masks gone they would visit the country of the white men with fire or a great flood & destroy them all.”⁷⁰ Tall Chanter would teach Matthews how to make prayer-sticks, as would Laughing Singer, but they both took seriously the risks of conveying knowledge or parting with items that might disrupt the workings of the chantway medicine.

By attending to the minutia of orientation and sequence in the creation of the drumstick, Matthews demonstrated his commitment to ceremonial study as a trainee, as someone with more investment than a mere passerby. While he could not (or chose not to) replicate the drumstick construction, he could use his training to produce scientific knowledge about Navajo beliefs. He eventually distilled his training into an article conceptualizing the Navajo philosophy of orientation and sequence into what he called the Navajo theory of “Butts and Tips” in 1892. “A Study in Butts and Tips” detailed the “tediously numerous and minute” ceremonial observances of Navajo singers, especially regarding the orientation of objects in space relative to the cardinal directions. His Navajo teachers conveyed to Matthews the importance of understanding an object’s “base,” and its importance as a rooted orientation in contrast to where the object pointed or terminated, its “tip.” In this study, he displayed the moral imperative of patience for minutia and tedium that the scientist must endeavor: “The study of symbols in any one race of people, in any one order of priesthood or in any one ceremony may be fruitless; but let us record painstakingly what we observe, and in the course of time the comparative study of our observations may bring a solution.”⁷¹ In this rendering, he could see himself as a humble fieldworker, tirelessly working to accumulate information about the Other, which in tandem with other like-minded practitioners could someday yield an as-yet-uncomprehended insight into human ceremonial practices.

Song Study

Material paraphernalia presented a problem of inscription, rendering an object’s construction on paper, divorced from the context of its creation. Songs and prayers presented

⁷⁰ Matthews, ND Yebitcai Properties, WMA. Laughing Singer, apparently, did later sell his ceremonial bundle, or *jish*, to Stewart Culin of the Brooklyn Museum. See Faris, *The Mountainway*.

⁷¹ Washington Matthews, “A Study in Butts and Tips,” *American Anthropologist* (October 1892), 345.

another distinct problem of documentation. First of all, chantways were long events, the most significant lasting nine nights, and each of these nights consisted of dozens, even hundreds of individual songs and prayers. Moreover, many chants and prayers of the chantways featured elements that were composed in a hieratic mode, meaning they were of an ancient idiom, no longer in everyday use. Listeners could not literally translate such songs, but singers and their peers knew the required sonic contours and the meanings contained within.

Even with a handle on the Navajo language and knowledgeable translators for interpretation, the songs and prayers of the Mountainway and Nightway were generally incomprehensible to Washington Matthews's ear. But even early in his studies he recognized the importance of replicated patterns within *hataalii* song performance. Other singers also gave feedback for songs performed throughout the event, critiquing the patterns sung. "The song is long, and is mostly made up of meaningless or obsolete expressions which convey no idea to the mind of the singers, yet not a single vocable may be omitted, mispronounced, or misplaced." But the song patterns themselves carried power that allowed the chantway to proceed. Singers could not simply improvise: "A score or more of critics who know the song by heart are listening with strained attention. If the slightest error is made it is at once proclaimed, the fruitless ceremony terminates abruptly, and the disappointed multitude disperses."⁷² Such attention to detail was required in *hataalii training*. Hieratic language, like sandpainting, drew an intimate lineage of knowledge transfer from the Holy People, who first taught the ceremonial in the distant past, to the learned men who came before, to the singer-in-training.

Matthews's predecessor as army surgeon, Jonathan Letterman, had called Navajo songs "but a succession of grunts, ... anything but agreeable."⁷³ Matthews, however, "found that these ceremonials might vie in allegory, symbolism, and intricacy of ritual with the ceremonies of any people, ancient or modern. [I] found, ere long, that these heathens, pronounced godless and legendless, possessed lengthy myths and traditions—so numerous that one can never hope to collect them all, a pantheon as well stocked with gods and heroes as that of the ancient Greeks, and prayers which, for length and vain repetition, might put a Pharisee to the blush."⁷⁴

⁷² Matthews, *Navaho Legends*. Matthews, however, never recorded such a dispersal, so he probably merely reported an idealized standard, rather than an observed one.

⁷³ Letterman, *Sketch*.

⁷⁴ Matthews, *Navaho Legends*, 23.

With the aid of wax cylinders, Matthews made audio recordings of chantway songs and prayers, as well as a host of other Navajo songs. Laughing Singer became Matthews's most reliable performer for these recording sessions, as evidenced by his frequent attribution in Matthews's cylinder notebooks. With such recordings, Matthews was able to spend time transcribing the songs without having to be present. He transcribed these into notebooks and made interlinear translations, when possible. Although he need not be in the field for transcription, without a knowledgeable steward to guide the process, the audio was useless as data. To aid his interpretive efforts, Matthews regularly made notes about associated meanings or histories learned from him tutors in Navajoland, which he placed on the verso page. Although singing for the recorder was not so much formal training as indulging in performance and demonstrating mastery, Laughing Singer provided commentaries on many of the songs he sung, iteratively teaching the pantheon and Navajo history to his ethnographer-student.⁷⁵

Origin stories were often referenced in ceremonial and non-ceremonial songs. By the 1880s, origin stories and folklore were considered essential objects of ethnographic collection. As folklore became an ascending, complementary field in the anthropological scene, scholars took great interest in indigenous "origin stories" as foundational stories that could be mined for data. Matthews was no exception to this. To him, origin tales had a way of encapsulating the mentality or logic of a given people.

In November 1893, Matthews sat down with the singer Old Torlino, who taught Matthews his rendition of the Navajo creation myth, interspersed with songs, some of which he allowed to be recorded on wax cylinder. Later, he compared his notes of Old Torlino's origin story account with one by Jake the Silversmith and an account given by Tall Chanter at Fort Defiance in 1887. He published a synthetic account of the Navajo origin story, as *Navaho Legends*, in 1897.

Matthews recognized that, although he was allowed to hear and write down these tales, the Navajo storytellers considered them to be potent tales, not to be taken lightly. When Matthews asked Tall Chanter to relate the "creation myth" of the Navajo, for instance, the singer's demeanor changed from relaxed to serious and careful.

⁷⁵ Matthews's recordings are held today in Indiana University's Archives of Traditional Music.

“He arrived early; he remained late; he hastened through his meals; he showed evidence of worry at all delays and interruptions, and frequently begged me to postpone minor explanations. On being urged to explain this change of spirit he said that we were travelling in the land of the dead, in a place of evil and potent ghosts, just so long as he continued to relate those parts of the myth which recount the adventures of his ancestors in the nether world, and that we were in danger as long as our minds remained there; but that when we came to that part of the tale where the people ascent to this—the fifth and last world—we need no longer feel uneasy and could then take our time.”⁷⁶

Each night, having exerted much more energy than during the usual interviews, Tall Chanter departed. Matthews was struck by his tutor’s change in attitude. Previously, Tall Chanter and Matthews had leisurely walked through the transfer of knowledge, from elder to novice, elder brother to younger.⁷⁷

The origin story, however, appeared to drain Tall Chanter, and he worried enough about the consequences of conveying it that, after finishing for the day, he protected himself with a special song. Matthews later learned that Tall Chanter needed to recite a long prayer to counteract the effects of being too long in “the land of the dead” during the storytelling. This prayer was not within the agreement between Matthews and Tall Chanter—indeed, Matthews did not even learn of its existence until Tall Chanter had begun his relation of the story of creation.

Matthews nonetheless sought this prayer, going beyond the scope imagined by Tall Chanter to document something that had not been in view. Stealthily, Matthews followed the singer to his room and, once the prayer had begun, sat down for its completion.

...Having waited in the adjoining passage half an hour or more, I heard the voice of the old man rising in the monotonous tones of formulated prayer. Knowing that the rules of the shaman forbade the interruption of any prayer or song, I abruptly entered the room and sat down on the floor near the supplicant. He was seated tailor-fashion in front of the hearth, on which a bright fire blazed, surrounded by two or three Indian listeners.⁷⁸

Tall Chanter finished his prayer, but Matthews did not record his reaction to the ethnographer’s intrusion. This prayer Matthews later published as “The Prayer of the Navajo Shaman,” in

⁷⁶ Washington Matthews, “The Prayer of a Navajo Shaman,” *American Anthropologist* 1, No. 2 (April 1888), 6.

⁷⁷ They had regularly met together over meals and to discuss Navajo histories, legends and poems, in addition to some of the medicine practices that Matthews had recently witnessed. As Matthews related of Tall Chanter’s previous demeanor, “In none of my interviews with him had he shown any impatience with my demands for explanations as we progressed or with interruptions in our work. He lingered long over his meals, lighted many cigarettes and smoked them leisurely, got tired early in the evening, and was always willing to go to bed as early as I would let him.”

⁷⁸ Matthews, “Prayer of a Navajo Shaman,” 7.

Navajo and English translation, in 1888. In it, Matthews recognized his transgression, but also portrayed his learning of it as inadvertent: “The prayer came to my knowledge almost by accident and perhaps contrary to the wishes of the supplicant.” He presented his documentation here as dutiful ethnological accumulation. What had been unknown to him became, immediately, a new object of interest, and Matthews followed a truffle-hunter mentality, rooting out new threads that strung together the web of ceremonialism.

While the Navajo origin story gave Matthews an indication of the important Navajo deities and justification for some practices, it was merely one part of a constellation of songs and stories that explained the ways of the world to the Navajo, and often provided a means for calling for assistance from the forces of the world to ensure balance in everyday life. Over time, Matthews learned the Navajo had thousands of songs, and many were given in non-ceremonial contexts. Songs and poems, Matthews learned, were deployed by the Navajo for many aspects of life—for traveling, for coming and going, for farming and harvesting, for building and even thinking about building, for hunting, for war, for gambling.⁷⁹ These songs were not the exclusive domain of medicine men, but sung widely by all Navajo men (and, in Matthews’s time, even some women), and indicated the importance of orality in Navajo culture. He learned that many songs could be deployed spontaneously, alone or with others, but that they appeared to be guided by “prosodical laws.” When gambling songs were performed, for instance, the players conjured luck from various stealthy creatures of Navajoland. One gambler, Matthews wrote, insisted that “there was not a thing that walked or flew or crept or crawled in all the world (as known to the Navajos, of course) that had not at least one appropriate song in the game, and that many had more than one song.” Moreover, Matthews could find similar versions of these songs across Navajoland, sung in different gambling lodges. A study of Navajo songs, while daunting and perhaps impossible to exhaustively document, would give insight into how they viewed the world and its causes, and from such data the anthropologist could go about the work of analysis.⁸⁰

Documenting the Temporal and Ineffable

Even in a relatively new professional field like anthropology, Matthews could draw on guidance for his documentation strategies. Prior folklore scholarship indicated collection

⁷⁹ Matthews, *Song Notebooks*, WMA.

⁸⁰ Washington Matthews, “Navajo gambling songs,” *American Anthropologist* 2, no. 1 (1889): 1-20.

methods for songs and folklore, and linguistic circulars (as the last chapter discussed) provided a standardized mode of philological collection. And Matthews himself had sketched and collected ceremonial objects and drawn diagrams and maps of various camps across Navajoland.

Sandpaintings made during a chantway, however, presented a particular problem of documentation. They existed for only mere hours in material form, during which time they served to draw Holy People to the ceremonial and attend to the requests of the medicine singer.⁸¹ Then the sandpaintings were destroyed, their powers exhausted.

No permanent design is anywhere preserved by them and there is no final authority in the tribe. The pictures are carried away from winter to winter in the fallible memories of men. They may not be drawn in summer. The custom of destroying these pictures at the close of the ceremonies and preserving no permanent copies of them arose, no doubt, largely from a desire to preserve the secrets of the lodge from the uninitiated; but it had also perhaps a more practical reason for its existence. The Navahoes had no way of drawing permanent designs in color.⁸²

Matthews could only see sandpaintings in situ, in Navajoland, during the winter months when the Nightway and Mountainway could be performed. Moreover, Navajo singers regulated access to sandpaintings; Matthews, recall, was among the first Anglos to report on Navajo sandpaintings after personally witnessing their creation. With such strictures, informants could not simply be paid to reproduce sandpaintings. Cameras, too, were out of the question, as they would disrupt the ceremonial. Instead, Matthews followed singers who consented to let him view their practice, and perhaps sketch it, and he paid them for this opportunity.

Representation posed another issue. Sandpaintings were colored with certain pigments of earth and other materials (indeed, Matthews referred to sandpaintings as “drypaintings” because the pigments composed of charcoal, pollen, and other materials that were not strictly “sand”). His early sketches from the 1884 Mountainway included watercolors in his notebook, probably painted while away in his tent. For other sketches, Matthews noted color if he was unable to apply it in situ, an indication that Matthews understood that colors had meaningful associations in Navajo cosmology.⁸³

⁸¹ Matthews, *The Night Chant*, 36; Parezo, *Navajo Sandpainting*.

⁸² Matthews, *The Night Chant*, 36.

⁸³ Matthews, 1884 San Mateo and Hackan Notebook, WMP.

As a visible medium, sandpaintings were best sketched discretely in the medicine lodge as they were prepared. Other Navajos occasionally raised opposition to Matthews's sketching during a ceremonial, and in these instances he put away his work and focused intently on memorizing the designs as well as possible. As he told Powell in 1884, Matthews obtained "most valuable and original information that I have yet collected in the Navajo country. By the judicious use of money, and by good management, I succeeded in getting free access to the medicine-lodge with permission to note and paint ad libitum, except on one or two occasions when I was obliged to put up brush and pencil; but easily carried to my observations in memory for record outside the lodge."⁸⁴ Overwhelmed by the sheer number of elements that composed a chantway, Matthews was forced to choose between, for instance, memorizing the sandpainting's important color scheme versus the oral chants and prayers that proceeded during the construction, healing, and dispersal of the sandpainting.

Matthews worked piecemeal on sandpaintings, and in none of his notebooks did he record a single, whole image. (For reasons of sensitivity, the drawings of sandpaintings from Matthews's notebooks will not be shown here.) In his notebooks, Matthews isolated different components of the overall image, such that gods, animals, ornaments, and other facets were illustrated on subsequent pages. To re-construct his de-construction, he then included a rough diagram of the overall composition. Later, with the help of an artist, he composed these into a representation of the sandpainting design. The sandpainting plates present in Matthews's major works were all Anglo-illustrated reproductions of piecemeal sketches of sandpaintings made in the field—it was not until later that sandpainting photographs (that is, images of singer-composed sandpaintings) were circulated through the photographic work of Edward Curtis.

When a ceremonial was over, Navajo singers left no trace of their earthen images behind; a sandpainting was destroyed once its healing powers had been exhausted. The sandpainting's creation and eventual dispersion, in a sense, mirrored the event itself. Through the several days of events, singers, assistants, performers, and storytellers came together to share stories and reconnected with one another. Navajo visitors renewed their connection to relatives and recalled and passed on the worldview informed by their intellectual traditions. Then, when the nine-day

⁸⁴ Matthews to Powell, October 1884, WMA.

ceremonial concluded, participants and visitors returned to their lives in other parts of Navajoland.

Due to the ephemeral quality of sandpaintings, the memories of medicine singers also raised questions of reproducibility. Matthews had some reservations about the proclaimed ability of Navajo singers to reproduce sandpaintings exactly as they had been taught, years ago. “No permanent design is preserved for reference, and there is no final authority in the tribe. The majority of the ceremonies can be performed only during the months when the snakes are dormant. The pictures are therefore carried over, from winter to winter, in the fallible memories of men.”⁸⁵ Matthews may have been troubled by the distributed nature of Navajo knowledge authority and the singer’s claim that all Nightway practitioners reproduced the *same* set of sandpaintings. However, his two detailed studies of chantways indicated many more similarities than differences—each singer had an artfulness and style, just as Anglo painters did, but different instances of the same sandpainting shared the same depictions, including similar intricate details. In this regard, Matthews recognized a level of fluidity in the claim of exactitude that was coherent to Navajo singers among themselves. Singers understood replicability not as a technical one-to-one replication, but rather as a mnemonic, a pattern that could be passed on while subtly morphing to fit a new singer’s preferences. Singers-in-training came to possess a form of “trained judgment” regarding their representations in sandpainting, whereby their expertise with the theory and practice allowed them to “smooth” paintings to fit their own styles of practice without divorcing from their effects. For Matthews, with some instruction but not years of experience, to document sandpaintings he had to separate the parts and re-compose the images to best emulate their ideal-typical instance, whereby the pattern could be replicated but the individualized components diminished.⁸⁶

In order to understand Navajo sandpainting, his tutors undoubtedly conveyed, Matthews had to see chantways as an integrated practice, rooted in a place and produced (and dispersed) in time. But he was shown, against prevailing bias against the sophistication of the esoteric practices of indigenous peoples, that Navajo ceremonialism was a coherent, organized, even professional practice. Singer training and experiential chantway study, then, altered Matthews’s

⁸⁵ Washington Matthews, “Mythic Dry-Paintings of the Navajos,” *American Naturalist* 19 (October 1885): 932-939.

⁸⁶ Daston & Galison, *Objectivity*.

style of ethnographic documentation and, in turn, his very understanding of ethnological science and its aims. Most significantly, Matthews's mode of ethnographic notation changed under ceremonial circumstances. He took copious field notes during ceremonials, which required speed over accuracy. These notebooks captured, he hoped, at least some approximation of his sensorial experience during a complicated rite. The site before his eyes, the sounds and smells, the feeling of his skin, his placement in space and time, the movements and actions of the medicine men and his assistants and the demeanor of the patient—Matthews used all of these elements to help him capture the experience of the ceremonials as he hastily scribbled in his palm-sized notebook.

As Matthews continued to spend time among Navajo groups and visit various ceremonials, he developed a more systematic note-taking practice that was appropriate for a ritual context. After events began, he scribbled quick entries, time-stamped when he could glance at his watch in the midst of an event. Though no longer writing on a tabular schedule, a “schedule” of the events was transposed into his notes — “6.30 [PM] corral, 8.20 orchestra, 8.30 torch applied, 8.40 wand dance, 8.50 ends, 9.15 great arrow (poorly done), 9.32 whistle, 9.34 enter 8 men and a leader (some naked)... 10.15 dance of Manuelito's 5 yr old boy to the living feather in the basket.”⁸⁷ The songs, dances, and sandpaintings of the chantways presented a tricky issue for ethnographic representation because of their temporal existence. Ultimately, Matthews needed to render the ephemeral elements of a chantway permanently, to stabilize parts of the dynamic event and convey these in tandem, for to present the chantway as a series of interlocking events would allow it to be understood systematically. To prove the existence of Navajo ceremonialism was only the first step. The next was to make the sensorial and ineffable into a scientific object, to make it a stabilized contribution to ethnological knowledge.⁸⁸

These “live” notebooks stood in contrast to other documentary efforts such as Matthews's dictionaries of Navajo language—the former sought to grasp a dynamic procedure through his experience of it, the latter attempted to systematize Navajo language, stabilizing it in a two-dimensional form. Both were acts of “redimensionalization,” whereby temporal events were, by documentary necessity, rendered into chronological sequences, bound by the choices of the accumulator in moment.⁸⁹ Matthews also redimensionalized within and among his notebooks,

⁸⁷ Matthews, 1884 Ceremony and Weaving Notes, WMA.

⁸⁸ Assessing the life of an emergent category, what Ian Hacking called its historical ontology, reveals how scientific objects come to be formed and how certain assumptions are packaged in their coalescence. Hacking, *Historical Ontology*.

⁸⁹ Rheinberger, *An Epistemology of the Concrete*.

away from informants or ceremonial events. While these different forms of documentation fulfilled discrete aims, each could also inform and illuminate the other. The live notebooks, for instance, brimmed with new vocabulary words, received from Jesus, Chee Dodge, Jake, Ben Damon or another interpreter who sat nearby Matthews during the ceremonial. And while Matthews was not fully fluent in the Navajo tongue, he drew on the knowledge contained in his dictionaries to interpret the chants and prayers spoken, and later to translate recorded songs. Interpretive scribbblings moved from one notebook to another, sorting new vocabulary information into its “place” in a dictionary and bringing translation and interpretation to “live” notebooks after an event’s end.

Publishing a Total Event: Matthews’s *The Night Chant*

Throughout the 1880s and 1890s, Matthews also collected Navajo stories, accumulating a variety of myths and histories. Tall Chanter provided Matthews with several accounts of origins and items related to the Navajo pantheon. Laughing Singer and another medicine man, Old Torlino, provided Matthews with a host of additional myths and tales. Chee Dodge and Ben Damon also undoubtedly provided clarification and supplemental information when they assisted Matthews as translators or ceremonial participants.

After leaving Navajoland in the mid 1890s, Matthews turned toward an articulation of the Navajo’s medicine and belief systems from the perspective of Anglo social science. He published two lengthy works, one on *Navaho Legends* in 1897 and another on *The Night Chant* in 1902 (about the Nightway). In these two works, Matthews redimensionalized his training under Navajo singers and experiences of ceremonial practices into more synthetic and systematized accounts of Navajo cosmology, placing multi-day events and transcendent stories onto the two-dimensional space of the printed page.

The Night Chant began with “general observations and elements of the ceremony,” what amounted to a parceled list of things that set the stage for and served to execute the chantway. Matthews began by discussing the Navajo ceremonies and singers in general, and then moved to the Nightway in particular. This fragmented list offered short explanations of discrete elements that composed the Nightway, which included its seasonal limitations (after first frost), the symbolism presented, the gods referenced, the sandpaintings and ceremonial offerings, the herbs and medicines, the ceremonial spaces and medicine lodge, and the masks and dances.

The second section, “rites in detail,” featured the sequence of the Nightway. It was cast chronologically, from the first day to the ninth night, and ran almost 100 pages. Each paragraph was numbered, as it was throughout the text, and although it followed a basic narrative—a “disinterested” Matthews witnessing the Nightway events in time—the format was heavily structured with subheadings and excursuses. Because the ceremonial was so lengthy, the numbered paragraphs allowed Matthews to refer to elements that had been, or were to be, mentioned.

“Myths” composed the third section of the book, and began with the story of “The Visionary,” the original recipient of the Nightway from instruction by the Holy People. Several variants of this myth and related stories were also presented. The fourth and final part of the monograph was composed of “texts and translations” of the songs and prayers given during the course of events. When, for instance, the sequential narrative of the rites presented a song, the reader could thumb to this section for a fuller version of the hymnody. Songs and prayers came last in the text in part because, as Matthews explained, “they often allude to matters which the hearers are supposed to understand. They are not like our ballads—they tell no tales. He who would comprehend them, must know the myths and the ritual customs on which they are based.”⁹⁰ Matthews reported the presence of over 400 songs in the Nightway, noting that some diverged from one another by only several words and thus he had selected only representative songs. He presented the selected songs in interlinear Navajo and English translation, and included remarks on each song, frequently referencing his “free translations” in the main text of the ceremonial’s procession.

Eight full-color plates of sandpaintings and ceremonial paraphernalia were included in the volume. Three of these plates were by a Washington painter, Delancy W. Gill, who painted the “sandpainting” after drawings and descriptions by Matthews. These Matthews showed to several singers “for observation and comment, to meet with invariable approval.”⁹¹ Although he recorded no objection to his permanent images, singers outside of Matthews’s immediate circle presumably may have objected to these illustrations.

The Night Chant diverged widely from the format of *The Mountain Chant*, published nearly fifteen years prior. The earlier work presented the myth-history of the Mountainway’s

⁹⁰ Matthews, *The Night Chant*, 270

⁹¹ Matthews, *The Night Chant*, 316 n77.

origin at the outset, followed by the sequence of events, explication of the “drypaintings,” and the original texts and translations. *The Night Chant*, by contrast, first presented a list of the elements that composed the ceremonial program—the theory and practice of the Nightway, its objects and actions—and then moved to the sequence of events that gave it its ceremonial power.

Matthews had shifted his thinking about how to best organize the ethnographic information for the totality of a complicated event such as the Nightway. After 1890, he spent much more time with Navajo singers like Tall Chanter and Laughing Singer, and he maintained strong relationships with translators and other Navajos steeped in ceremonialism, including Chee Dodge, Ben Damon, and Jake the Silversmith. His education under these men made him appreciate the depth of knowledge and history that permeated chantways—that made chantways possible, perpetuated them, and made them powerful. By spending time among Navajo medicine singers, Matthews had opportunity to rethink an implicit question of documentation and presentation: How to document something that escaped the senses?

I have witnessed the ceremonies of the Yebetcai [Nightway] in whole or in part a dozen times or more and every time I have observed something new. And yet there are some parts I have not observed, but depend for my knowledge of them on information obtained from the shamans. Half a dozen skilled observers each watching a different part of the work could not note all that pertains to the ceremony on one occasion, nor could they, without long study of the work have a suspicion of what they might have missed. In the myths this complexity of the ceremony is well recognized, for we are told that the prophet after his first return home had to be again off for further instructions, he could not see it all at once.⁹²

Training with a medicine singer, for one, allowed the ethnographer to understand the complexity of the multi-day events. But also, as trainers and trainees well knew, a single ceremonial could not enlighten the spectator to its intricacies. Instead, its secrets had to be unlocked through diligent scholarship, practice, and the maintenance of ties with other singers.

Washington Matthews, after accumulating Navajo vocabulary, shifted to ceremonies and prayer, songs of healing aligned with his own profession as a healer. He was a scientist and believer in science, but also came to understand the value of Navajo medicine, the potency of ceremony, and sought to understand the connection between cosmology, health treatment, and

⁹² Quoted in Faris, *The Mountainway*, 49.

subjectivity. He was impressed by contrasting elements of Navajo healing—singularity and replication, instruction and destruction—and late in his life dedicated himself to capturing the knowledge and practice of Navajo medicine for posterity. While doing so, he connected himself with the emerging anthropological scene in the Southwest and in Washington, New York, and other eastern hubs. Though intimately tied to his Navajo informants—bound by friendship and tutelage—Matthews maintained allegiance with the scientific maxim of the collection and free circulation of worldly phenomena. Matthews’s own belief that he could capture the complexities of ceremonialism, including the embedded history and training that constituted the singer’s abilities, indicates that human scientists in the late nineteenth century were not constrained by aspirations to divine universal human laws, but rather could concerned themselves with an empirical cultural totality, a self-contained documentation and presentation of human life to an audience. The data produced could be used for comparison, to be sure, but the ethnographic monograph could also become a holistic body of evidence in itself, something to be filed away as a modest approximation of indigenous practices featuring sensorial complexity and a host of ineffable effects.

Not all Navajo singers approved of such reproduction and dissemination. But with the presumed permission of singers like Laughing Singer and Tall Chanter, who had transmitted their knowledge to Washington Matthews as if he were a singer-in-training, Matthews felt licensed to publish ceremonial events as a contribution to Anglo science. But, of course, his publications were not the Nightway, not the Mountainway. They amounted to something else entirely: an anthropological representation and interpretation of these events, a strategy of documentation, an experiment in the organization of complex ethnographic material as an object of scientific information—even evidence that the meaning and effects etched into chantways could not be extracted from Navajoland.

Chapter 3

Secrets of Pueblo Thought: Compartmentalization and the Contours of Knowledge Access

On a day in late September, as the community of Santo Domingo (Kewa) was bringing in the harvest and repairing the bridge and adobe walls of their houses, the local Hispano priest rode into town and introduced a stranger to the people. The stranger, Adolph Bandelier, was a white man from Illinois, and identified himself as an anthropologist, there to visit and learn from the people of Santo Domingo. After the newcomer situated himself in his room off the church, curious Santo Domingans stopped by to see and talk to the stranger. Gregorio, an outgoing man, came to introduce himself, greeting Bandelier in Spanish and speaking to him about hunting. Bandelier, at the desk, took out a notebook and blue fountain pen and scribbled in his book as Gregorio spoke. The stranger was curious; he wanted to know intricate details about the hunt: “how do you distribute the meat from a kill?” Gregorio humored his questions and taught Bandelier a few words in Keres, the native tongue of Santo Domingo. The newcomer seemed eager to learn these words and asked for more of them, but Gregorio demurred when asked about the word for “sun,” telling Bandelier that the governor insisted they not share that information with outsiders.

In the following days, the stranger asked more and more questions. The everyday banalities of Santo Domingo life seemed of great interest to Bandelier—corn-grinding on *metates*, the slaughter of a sheep—and men like Gregorio and the sacristan, Santiago Crispín, tolerated the stranger’s inquiries. In part, they tolerated his questions in order to remain close to him, watching the newcomer on behalf of the community. Over several days, some comfortability had formed between the community and their strange visitor, though the sacristan had become suspicious of his constant writing and questioning.

Then one day that fragile trust was broken. Another stranger had arrived by train, bearing heavy equipment. When Bandelier and the other Anglo man returned to the pueblo, the two immediately set to assembling a large device for taking photographs. Meanwhile, the bustle of

community life disappeared and clandestine meetings were held to discuss what to do about the stranger's use of the camera, which appeared to distress many in the pueblo. Averse to confrontation, the leadership likely implored Santo Domingans to communicate their displeasure non-verbally—a shoulder turned to block his view, a prolonged wait, unsmiling faces and crossed arms. Soon, surely, the stranger would leave and disruptions would come to an end.

In the following days, the passing of an elder meant the people of Santo Domingo needed to plan a funeral and mourn their lost loved one. Likely suspecting the stranger would attempt to witness the funeral and perhaps even try to photograph the solemn event, a member of the community asked Bandelier to stay in his assigned room attached to the church during their public grieving. As the streets cleared in anticipation of the funeral procession, however, the stranger stealthily climbed to the roof of the church to see the community in mourning.

There were still eyes on the stranger Bandelier, and news of his surreptitious viewing spread fast among the people of the pueblo. After laying the elder to rest, the people dug in and closed down, and the sacristan stopped bringing food and water. The people merely waited, patiently and without open hostility, for Bandelier to pack up his things and leave. The stranger appeared angry, and stubbornly refused to leave. But he soon tired of the waiting game. So, on October 2nd, 1880, Bandelier departed Santo Domingo, ending his 10-day stay.¹

Wandering the hills outside Santo Domingo, Bandelier was greeted by Juan José Montoya, a farmer and hunter of the nearby pueblo of Cochiti. Montoya had spent time among outsiders; he could read and write in Spanish and had dealt with Hispanos and Anglos when in his prior role as governor for his community. The Indian took the Anglo to his home. The pueblo of Cochiti, only several miles from Santo Domingo, had surely heard of the Anglo visitor, perhaps they had even received word of the trouble between Santo Domingo and the visitor. Nonetheless, the people welcomed Bandelier when he arrived with Juan José Montoya. The

¹ This account draws primarily on the field notes and correspondence of Adolph Bandelier. See Bandelier Diary, September and October 1880, Adolph Bandelier Collection, FACHL; Bandelier to Morgan, 22 October 1880, Lewis Henry Morgan Collection, URSC. See also Leslie A. White, *Pioneers in American Anthropology* (New York: AMS Press, 1978). Histories and ethnographies of the Pueblo also inform this narrative retelling. See Joe S. Sando, *Pueblo Nations: Eight Centuries of Pueblo Indian History* (Clear Light, 1992); Alfonso Ortiz, *The Tewa World: Space, Time, Being, and Becoming in a Pueblo Society* (The University of Chicago Press, 1969); Edward P. Dozier, *The Pueblo Indians of North America* (Holt, Rinehart and Winston, 1970); Brooks, *Captives & Cousins*. My narrative strategy in retelling of these events, playing with the use of both Indian and Anglo perspectives without overdetermining affect or reasoning, is informed by Daniel Richter, *Facing East from Indian Country: A Native History of Early America* (Harvard University Press, 2001). For other scholars that have employed this tactic, see Barr, *Peace Came in the Form of a Woman*; Michael J. Witgen, *An Infinity of Nations: How the Native New World Shaped Early North America* (University of Pennsylvania Press, 2012); Karl Jacoby, *Shadows at Dawn: A Borderlands Massacre and the Violence of History* (Penguin Press, 2008).

former governor rented a room to Bandelier for a modest sum and treated the newcomer amiably. He talked with the Anglo man, taught him words in Keres (occasionally Cochiti women would tease Bandelier about his pronunciation), and indulged his questions, though the white man was undoubtedly more cautious with his Indian hosts considering his recent ejection from Santo Domingo.²

One day Bandelier came to Juan José Montoya and showed the former governor some goods he had purchased from another Cochiti man. Montoya immediately recognized these as sacred objects that had once been in a ceremonial space, the *kiva*, but were now in the stranger's possession. Montoya was thunderstruck—these were objects that had been bequeathed to men of age, in confidence, objects that did not belong to any single owner but rather to the Cochiti people as a whole. Montoya urged Bandelier to restore the objects to the community. Though he appeared pleased to see Bandelier return the objects, Montoya was visibly worried about the potentials marked out by their sale. Bandelier told him he had purchased the sacred objects so they could be studied elsewhere, in Anglo-dominated parts of the country. These objects were not touristic fare; it appeared that there were Anglos—these “anthropologists”—that wanted more than well-made pottery and turquoise jewelry that Pueblos had recently begun to sell. The troubling thing was that these anthropologists wanted to buy things that could not be owned.³

By the end of 1881, all the sedentary agricultural communities known collectively as the Pueblo Indians had been visited by anthropologists such as Bandelier.⁴ From their experiences of spiritual repression under the Spanish state and Catholic missionization, the Pueblo communities of New Mexico had long understood that the less outsiders knew of their ceremonies and beliefs, the better to maintain their peaceful existence.⁵ But Anglo anthropologists were a new type of

² Adolph Francis Alphonse Bandelier, *Final Report of Investigations Among the Indians of the Southwestern United States: Volume III* (Cambridge, Mass.: J. Wilson and Son, 1890), 293.

³ Bandelier Diary, 7 November 1880, FACHL.

⁴ The Pueblo communities in this chapter each have a variety of autoethnonyms but have long recognized themselves collectively as the Pueblo Indians. In addition to many shared beliefs across several different language families, Pueblo Indians lived in “pueblos” (Spanish for village), which can be confusing to those not familiar with the region and its peoples. While I generally have tried to present communities as individual entities and limit my use of the catchall “Pueblo,” this is not always possible.

⁵ For a sense of the Southwest before its annexation by the United States in 1848, see Gutiérrez, *When Jesus Came*; Reséndez, *Changing National Identities at the Frontier*; Tracy L. Brown, *Pueblo Indians and Spanish Colonial Authority in Eighteenth-Century New Mexico* (U. of Arizona Press, 2013); Brooks, *Captives and Cousins*; Barr, *Peace Came in the Form of a Woman*; Anderson, *The Indian Southwest*; Blackhawk, *Violence over the Land*; Steven W. Hackel, *Children of Coyote, Missionaries of Saint Francis: Indian-Spanish Relations in Colonial California, 1769-1850* (University of North Carolina Press, 2005); Pekka Hämäläinen, *The Comanche Empire* (Yale University Press, 2008); John L. Kessell, *Pueblos, Spaniards, and the Kingdom of New Mexico* (University of Oklahoma Press, 2008); Weber, *The Spanish Frontier*. For the pre-Hispanic Southwest, see Sando,

visitor that differed from the familiar Anglo and Hispano missionaries, bureaucrats, soldiers, traders, and occasional tourist. These men and women did not seem primarily interested in stamping out their cherished practices, as missionaries had in the past. Instead, they were intently focused on these very practices and the inner-workings of their ways of life.

The communities of Santo Domingo and Cochiti had different reactions to the anthropologists in their midst in the early 1880s, and such differences illuminate specific conditions of the ethnological “encounter” in the late 19th century, as indigenous communities and Anglo newcomers to the Southwest sought to reconcile their respective goals in the midst of American encroachment.⁶ This chapter explores the constitutive modes of the encounter, its limits and boundaries, its negotiations and tactics—that is, how anthropologists and their inquiries affected Pueblo communities and how Pueblos set the contours of what could and could not be learned by anthropologists.⁷

In this chapter, I show that anthropological practices in the 1880s and 1890s were shaped in part by Native actors, who established the outline of ethnographic possibility and instructed anthropologists in the correct modes of comportment and decorum, even if such instruction did not stick. The ethnographic “field” has always been grounded first in Native belief systems and lifeways. As intensive sociocultural study joined schedule-based ethnology (Chapter 1), ethnographic fieldwork became a relationship-building endeavor. At times, the transfer of knowledge from informants to ethnographers was a negotiated exchange with few ripples, as in the (mostly benign) story of Washington Matthews’s study of Navajo chantways presented in the previous chapter. But ethnological fieldwork also revealed incongruous ideas of sanctity and propriety, of discretion and disclosure, between Anglos and Indians. Anthropologists learned

Pueblo Nations; Lekson, *A History of the Ancient Southwest* 2009.

⁶ Following Faier and Rofel, the ethnological “encounter” can be seen as “engagements across difference: a chance meeting, a sensory exchange, an extended confrontation, a passionate tryst.” The authors address this in contemporary ethnographies of encounter, while my project is an historical examination of engagements across difference. Lieba Faier and Lisa Rofel, “Ethnographies of Encounter,” *Annual Review of Anthropology* 43 (2014): 363-377.

⁷ I engage in this analysis as symmetrically as possible given the source material, meaning I attempt to present the circuits of influence and exchange from both Anglo and Indian perspectives. Unfortunately, the source materials which would suggest Pueblo perspectives are almost universally mediated by Anglo voices and documents. Giddens emphasized a type of symmetry in his notion of the “structuration” of scientists and subjects as engaging in mutually constitutive practices; see Anthony Giddens, *The Constitution of Society: Outline of the Theory of Structuration* (Polity Press, 1984). Callon and Latour emphasized the inclusion of materials and non-humans as agents; see Michel Callon, “Some Elements of a Sociology of Translation: Domestication of the Scallops and the Fishermen of St Briec Bay”; Latour, *Science in Action*. For an overview of the interpretation and use of the concept of symmetry in STS, see Sally Wyatt, “Technological Determinism is Dead; Long Live Technological Determinism,” in Edward J. Hackett (ed.), *The Handbook of Science and Technology Studies* (MIT Press, 2008). Raymond Williams, *Marxism and Literature* (Oxford University Press, 1977) also provides a symmetrical framework for understanding social production and agent interactions.

from their encounters—were both instructed on behavior and learned it through time among Indians—and brought this learning to subsequent fieldwork. Comportment instruction by Indian communities can be understood as an argument for the Native shaping of anthropology, to be added to histories of anthropology that have emphasized prior theoretical orientations, institutional influences, and disciplinary professionalization.⁸ However, I am more concerned with the processes of exchange and instruction that were presented in ethnographic encounters because attention to this dynamic can portray the emerging and multiply constituted subjectivities of anthropologists and their indigenous informants.⁹

Ethnography is different in every instance, of course. The practice is and was shaped by nearly limitless contingencies, not the least of them the historical cultural traditions that shaped Native communities in the Southwest; fieldwork among the Pueblo differed from fieldwork among the Navajo. Of the major conditions that shaped fieldwork within the ethnographic encounter—a people's recent and historical relations with Others and outsiders, an anthropologist's demeanor and method, the caprices of informants—the ethics of knowledge-sharing had a pronounced effect on fieldwork among the Pueblo. From the late-nineteenth-century Pueblo perspective, certain knowledge was sacrosanct and stewarded by select individuals, and thus restricted who could access it. Restrictions to knowledge access included members of their own populace in that certain forms of knowledge were distributed and guarded by social cliques and along gendered lines. This “compartmentalization” of knowledge benefited the community rather than the individual knower, and free access to information was seen to actually disrupt the social stability to which they were accustomed.¹⁰ In short, Pueblo communities possessed a knowledge economy where nobody in the community knew the totality of sacred knowledge. Given the decentered, stewarded cast of Pueblo knowledge, it is unsurprising that Pueblo ethics of knowledge complicated the anthropologist's reliance on

⁸ For professionalization of Americanist anthropology, see: Hinsley, *Savages and Scientists*; Bieder, *Science Encounters the Indian*; Stocking, *The Shaping of American Anthropology*; Darnell, *And Along Came Boas*.

⁹ One might think of this as a “contact zone” where social actors meet and interact with one another in a power-differentiated relationship. See Pratt, *Imperial Eyes*. Fernando Ortiz, *Cuban Counterpoint, Tobacco and Sugar* (Knopf, 2013 [1947]), introduced the term “transculturation” to point to the mutual cultural transformations that flowed between material, sites, and actors (in his case, between Cuba and metropolitan Europe). It also certainly resembles the notion of the “middle ground,” which spotlighted shared norms of behavior and comportment in unequal power relationships, presented in Richard White, *The Middle Ground: Indians, Empires, and Republics in the Great Lakes Region, 1650-1815* (Cambridge University Press, 2011).

¹⁰ Compartmentalization is a term used widely in Pueblo scholarship since Dozier, *The Pueblo Indians*. While the term is usually used to describe the dual mode of Pueblo spirituality as both autochthonous and Catholic, it is an appropriate term for the distribution of knowledge among religious societies. See also Brandt, “Native American Attitudes towards Literacy and Recording in the Southwest.”

informants to accumulate detailed, intimate cultural information to advance the study of human life. This disjunction between Pueblo knowledge maintenance and Anglo ethnography's public (and intrusive) inquiry allowed Pueblo communities to identify inquiring outsiders, marking them as people to be treated hospitably but with extra care, for their actions put community knowledge and knowledge-keepers at risk.

If Pueblos established boundaries of what could be known and how such knowledge could be obtained, anthropologists learned of the ways communities concealed knowledge from them and worked to glimpse across said boundaries. Through experience among Pueblo communities, anthropologists came to realize the existence of restricted areas of knowledge and the presence of a social code that regulated access to such "secret" areas. The Pueblos already had a reputation for "secrecy" before Anglo anthropologists had arrived, a historical response to spiritual repression under Spanish-backed Catholicism. "Secrets" became a proxy for "religion"; the presumed religious "survival" of pre-Catholic spiritualism could thus be sought by prying open the hidden and obscured practices Pueblos engaged in.¹¹

I suggest that the identification of secrets-to-be-uncovered, or what I also call "known unknowns," recast the role of the Indian informant, and a closer relationship developed between anthropologists and certain Pueblo informants.¹² I argue this closer relationship both enabled Pueblo informants to shape the conditions of access for anthropologists and placed them in a precarious position in which they could benefit from secretsharing as well as risk censure from their community.¹³

Knowledge Stewardship in the Puebloan Ecumene

When the people of Santo Domingo prevented Bandelier from further documenting the community, they were regulating social divisions that had been in place long before Anglos had

¹¹ The study of devotional practices and doctrine became seen as the key to a fuller understanding of divine beliefs interfaced with Pueblo society. In turn, anthropologists hoped to draw more exact classification of the social typology which Pueblos fit and accumulate a large amount of data for the benefit of comparative analysis, both with other Indian groups and among the diverse communities of the Pueblo ecumene.

¹² This phrase, the reader may notice, comes freighted with contemporary meaning. It was used by former US Secretary of Defense Donald Rumsfeld as a category of hypothetical evidence for weapons of mass destruction, which led the invasion of Iraq in 2002. By taking up his line, I want to foreshadow the perils of seeking objects that hypothetically exist, only to find that seeking them has unforeseen consequences.

¹³ Vine Deloria critiqued the orientation of anthropologists toward the extraction of knowledge for their professional benefit and begun a long-standing conversation in anthropology about the benefit of anthropology for the Other. Deloria, *Custer Died for Your Sins*. The intensification of the relationships between anthropologist and their informants is discussed in the next two chapters.

ever encountered the Pueblo ecumene. Such “Pueblo” codes, of course, differed from community to community. The Puebloan ecumene (meaning the “known” or “habitable” world from the Pueblo perspective, including communities with allied and alien traditions) fused together historically diverse people, as indicated by the several languages that were in use across the Puebloan region—Keresan dialects, Tanoan languages, Zuni, and Hopi.¹⁴ Although diverse in tongue, through exchange and intermarriage since at least 1000 CE, oral tradition and archaeological evidence indicate a common basic system of social structure and belief had continuously tied together the constellation of Pueblos in New Mexico and the Hopis and Tewa in Arizona. All of these communities practiced agriculture and some animal husbandry, built permanent rock or adobe brick homes in a tiered architectural aggregation, and developed industries of pottery, pigments, curatives, and crops, which they traded among themselves and with neighboring communities of Indians, Hispanos (after circa 1539 CE), and Anglos (after around 1800, and more steadily with the opening of the Santa Fe Trail in 1822).¹⁵ Though different in many ways, communities of the Pueblo ecumene conceptualized themselves as connected by custom and kin (see Image 4).

The cultural fabric of the Pueblo ecumene held together despite rapid transformations in the 19th century Southwest. Anglo-American settlers moved into the Southwest after the annexation of 1848 and sutured their formalized market economy to Mexican and indigenous trade networks through a suite of transportation and communication technologies. New goods and different kinds of people percolated into the region from abroad and concentrated in the growing cities of Santa Fe, Albuquerque, Phoenix, and Flagstaff. Indian communities, with their own historical and contemporary connections to these settlements, responded to the effects of such arrivals in a variety of ways. Notably, Pueblo communities contended with both Anglo, Navajo and Hispano settlers for land, their long-standing and well documented presence in the region notwithstanding. Anglo ethnologists, then, arrived in the Pueblo ecumene at a time when communities were grappling with new cross-cultural relationships.

¹⁴ Keresan dialects were spoken at Acoma, Cochiti, Laguna, San Felipe, Santa Ana, Santo Domingo, and Zia. Tanoan languages include Towa (Jemez), Tewa (Hano, San Juan, San Ildefonso, Santa Clara, Nambe, Pojoaque, Tesuque), and Tiwa (Taos, Picuris, Sandia, Isleta, Ysleta del Sur). Hopi was spoken among the Hopi villages; Zuni among the Zuni settlements. Pueblo languages are still spoken in many if not all of these communities today.

¹⁵ Ortiz, *The Tewa World*; Dozier, *The Pueblo Indians*; Alfonso Ortiz (ed.), *New Perspectives on the Pueblos* (University of New Mexico Press, 1972); Sando, *Pueblo Nations*; Bandelier, *Final Report*.

Although the fixtures of American government were slowly installed in the Southwest after the 1848 Mexican Cession, Puebloan peoples had encountered Anglos throughout the entire 19th century. All Puebloan communities understood the particular economic and military power of the United States before the arrival of American anthropologists. All were apprised of the transition of bureaucratic power from Mexican to American. Some had welcomed the regime change—many Pueblos had hoped it would neutralize squatting by non-Pueblo settlers, but Anglos had brought their own settlers and fences—or were cautiously optimistic for positive influence in their communities after the overbearing policies of the Mexicans and Spanish before them.¹⁶ In addition, all communities in the Southwest had encountered Anglo traders and soldiers, and community leaders would have thought deeply about how their community fit into the larger purview of American forces. Pueblo communities were already familiar with the systems of inquiry that arose from European bureaucratic traditions, and they understood how to navigate them.

Pueblos had deep historical memory as subjects of Spain (and between 1680-1692, as revolutionaries that had thrown off the Spanish yoke), and then as citizens of Mexico. Over the prior 200 years they had learned to police the threshold that kept safe an internal community while facilitating their participation in a broader political economy. While Pueblos were not represented in Spanish political councils, the position of *gobernador* (governor) had arisen as a political office to deal with outside influences. This position varied in degrees of seriousness depending on the strategy that each community adopted as it dealt with outside bureaucratic forces. Zuni, for instance, throughout the second half of the 1800s had selected governors who could communicate in Spanish and had broad conciliatory goals for the community while also holding high positions in the ceremonial leadership; by the late 1910s, this strategy was altered in favor of a governor who was an artist and ceremonialist who stymied the foreign business and bureaucratic interests on behalf of the community.¹⁷ In either strategy, a pueblo's governor could act as the gatekeeper of the community—if he relayed a message to “withhold the word for sun,” as Bandelier experienced at Santo Domingo, then his word was law.

¹⁶ For land politics in nineteenth century New Mexico, see Malcolm Ebricht, *Advocates for the Oppressed: Hispanos, Indians, Genízaros, and Their Land in New Mexico* (University of New Mexico, 2014). Roxanne D. Ortiz, *Roots of Resistance: Land Tenure in New Mexico* (Univ. of California, 1980). Ongoing land issues, especially from the Hispano perspective, are explored in an illuminating ethnography by Jake Kosek, *Understories: The Political Life of Forests in Northern New Mexico* (Duke University Press, 2006).

¹⁷ Suina, “Pueblo Secrecy”; Brandt, “On Secrecy and Control of Knowledge.”

By the 1870s, information about Pueblo social and material culture was available to the curious in cosmopolitan Anglophone and Hispanophone spheres, but this literature was (literally) superficial. How Pueblos thought, and the contents of their mode of worship, were not understood by outsiders. This ethnographic gap was not merely a lack of investigation. While knowledge of the foundation stories of Pueblo beliefs pervaded a community, the philosophical nuances of Pueblo cosmology were regarded as precious and sacrosanct. Anglo investigators found the details of the forces animating the world and the origin of their powers were closely guarded knowledge in the Pueblo ecumene, regulated by religious societies. Moreover, as informants readily conveyed, sacrosanct knowledge was not singularly isolated, but rather distributed among the religious societies. Each society kept safe distinct forms of knowledge that propelled Pueblo life.¹⁸

Societies with specialized knowledge were conceptualized as promoting certain universal positives, but their ability to do good came with responsibility over powerful forms of knowledge and thus could not be mishandled or widely known. “A secret order is for the benefit of the whole world, . . . not for the exclusive benefit of the few men who belong to it,” the Hopi Snake Society priest Nanaje told the ethnographer John Bourke in 1881.¹⁹ “But its privileges are not property of its members and should be preserved with jealous vigilance, since, if they became known to the whole world, they would cease to be secrets and the Order would be destroyed and its benefit to the world would pass away.” Initiated members were stewards or guardians of knowledge, and Pueblo material and symbolic culture were elaborate technologies for interacting with a world interpenetrated by powers that could be called upon, directed, or fostered for certain ends. The objects and symbols were “truly the property of those who understand them”—their power was lost if an ignorant party profaned them—or, for that matter, if they were captured on paper or photograph without proper contextual knowledge.²⁰

Within a pueblo, the regulation of secret knowledge was known and understood by all people, even those who did not have access to it. Indeed, regulation of knowledge access was a structural facet of the community, and secret-keeping was a motivating factor in self-identity and

¹⁸ Ortiz, *The Tewa World*; Ortiz, *New Perspectives on the Pueblos*; Brandt, “Native American Attitudes towards Literacy and Recording in the Southwest.”

¹⁹ Bourke Diary, 20 November 1881, John Gregory Bourke Papers (microfilm).

²⁰ Byron Harvey, “An Overview of Pueblo Religion,” in Alfonso Ortiz, *New Perspectives on the Pueblos* (University of New Mexico, 1972), 206.

comportment. An individual might come to know or see something he should not, but he would further understand that his violation must itself be secret; he could not unsee what he had seen, so he must work to keep it safe. In other words, all Pueblo adults knew what was potentially secret—they might not know the secret, but they knew the positive space that enveloped a void created by the secret: a “known unknown.”²¹

This knowledge system was accumulative and multi-polar, rather than isolated and enigmatic; it was complex, but not inscrutable; it was diversified and distributed, but not utterly inaccessible. Once a certain level of understanding was reached, other avenues of knowledge could be sought. To visiting Anglos, access to special knowledge appeared to have a flexible aspect in that proving oneself could enable a path toward knowledge. Although there were restrictions based on gender (and exceptions even then), this meritocratic principle was at times even applied to outsiders, and these conditions of access were shown to Anglo anthropologists by hospitable Pueblos, even if the ethnologists did not always take to the lesson presented.²² After Pedro José, a San Felipe governor, welcomed John Bourke among his people in 1881, he travelled with the visitor to the neighboring pueblo of Zia. There they met with Jesus, the son of the Zia governor, and Pedro José introduced Bourke in positive terms, stating the anthropologist already knew of many of the traditions of the Pueblos. Such an introduction allowed the Zia man to share information with Bourke, since the foundation of his knowledge had been vouched for. Later, Jesus contrasted Bourke’s inquiries and accumulated knowledge with the zealous and ignorant inquisition of Spanish Catholicism (using the metonym “Mexicans”): “we never talk of these things to Mexicans. We see that you are not a Mexican—that you know much of our customs and will tell you all you want to know and show you all you want to see—the estufa [kiva], the old church, and all our houses.”²³

Pedro José saw the scribe’s prior knowledge but was probably not convinced Bourke should be privy to further sacrosanct knowledge. Instead, he kept his demeanor hospitable and

²¹ See Colwell-Chanthaphonh, “Sketching Knowledge.” It is important to say that this structural condition of knowledge access did not mean that Pueblo communities were paralyzed by secrecy. Instead, it is better to think of Pueblo social structure being enlivened by traditional knowledges that required respect, responsibility, discretion and attention to access in the first place. This knowledge system was accumulative and multi-polar, rather than isolated and enigmatic. Once a certain level of understanding was reached, other avenues of knowledge could be sought.

²² See Matilda Coxe Stevenson, “The Zuni Indians: Their mythology, esoteric fraternities, and ceremonies,” *Twenty-Third Annual Report of the Bureau of American Ethnology to the Secretary of the Smithsonian Institution, 1901-02* (1904), 65, for instances of women in a Zuni fraternity.

²³ Bourke Diary, 4 November 1881, John Gregory Bourke Papers (microfilm).

amiable, and he was able to both accommodate the visiting ethnographer and keep tabs on his documentary practice. Bourke, who thought the governor overly friendly, found his access curated and packaged for him in this case. This was not odd—he did not yet know what he did not yet know. From Pedro José’s perspective, however, an introduction and tour of the kiva were not the same as revealing secret knowledge, and it appears he shared with Jesus an unspoken agreement to keep even the existence of sacred knowledge out of view.

Pueblo fraternal societies were charged with keeping knowledge and regulating its boundaries, which including watching for potential violations of secrecy by both the initiated and the uninitiated. The effect was a sort of community-wide self-discipline (and, as one might imagine, of gossip). As Nanaje had told Bourke, speaking or hearing secrets was a taboo because the good and the happy came from the efforts of the special societies, and counteracted evil in the world. This evil could emanate from within the community, and Pueblos reacted powerfully against any of their own people that profaned or exploited specialized knowledge. Internal secretseekers were frequently deemed witches, persons who circumvented the initiation of specialized knowledge or using secret knowledge solely for the benefit of the witch himself. Punishment for the violation of the boundaries of knowledge ranged from forced initiation (and thus regulation of knowledge held), to censure and communal shaming, to (in the most extreme cases) death. Anthropologists recognized early that the individuality of the witch was among the most threatening elements in Pueblo existence—he or she selfishly worked against the harmony of the social body as a whole.²⁴

Pueblo knowledge keepers were also wary of ignoramuses and skeptics in their own communities. While a witch appropriated knowledge for his or her own use, a non-believer represented the potential for knowledge to simply evaporate or diminish as smoke in the wind. Palowahtiwa of Zuni compared skeptics in his community to atheists among the (Christian) Anglos, who thought the Bible to be merely “paper, common paper which any one might roll into cigarettes and smoke away.” He related that the “foolish” in his community,

say of our ancient sacred plumes and medicines that they are nothing but old turkey feathers and eagle plumes, crumpled and broke, old cotton strings, musty and worn out, and broken bits of beads and shell stuck in a lump of clay and pitch mixed with cornmeal. But as the wise among you would say to these foolish ones of yours, ‘This ancient

²⁴ Harvey, “An Overview of Pueblo Religion.”

writing is nothing but paper, it is true, yet wrapped up in its heart, as it were, is something else, with a wise and precious meaning.' ... So, verily, the meaning and potentiality that are wrapped up in these old plumes are such that they are too precious to be bought by the largest herd of horses in the world.²⁵

Here, Palowahtiwa alluded to an even greater potential violation than mere non-belief—that the ignorant might in their carelessness give away or sell for their own benefits the sacred objects, and the meanings wrapped up within them, to outsiders.

While witches and skeptics represented potential threats inside of the community, any violation of secret knowledge was potentially hazardous. Parallel to someone who sold what could not be sold, talking of secret knowledge with an outsider posed a threat in the reproducibility of language. To outsiders, Pueblos frequently dissimulated their knowledge of sacred practices, sites, objects, or words. After repeated encounters with demurring Pueblos, John Bourke complained in his diary that the

Rio Grande Pueblos have become so shy and so timorous that duplicity and dissimulation are integral features of their character and in all conversations with strangers, especially such as bear upon their religion or their prehistoric customs and their gentile divisions [“clans”], they maintain either an absolute reserve, or, if that be broken down, take a malicious pleasure in imparting information for no other object than to mislead and confuse.²⁶

Potential informants could tell anthropologists that they simply did not know the asked-for information; at other times, informants outright misled the secret-seekers. Crafting stories to deter anthropological inquiries was also a tactic that ethnographers had noted. Nanaje admitted as much when he told a visiting anthropologist, “We tell all sorts of stories to outsiders, even in Moqui [Hopi]. Of course that is lying, but if we adopted any other course, our secrets wouldn’t be kept very long. You must not get angry at me for speaking thus to you, but I cannot tell you what you want to know and I don’t want to deceive you.”²⁷

In his explanation of constructing stories for outsiders, Nanaje stated outright that things could not be known. Making such a statement illustrated a hazy epistemological terrain wherein

²⁵ Palowahtiwa’s philosophical reflection was translated by Cushing. Frank Hamilton Cushing and Jesse Green (ed.), *Cushing at Zuni: The Correspondence and Journals of Frank Hamilton Cushing, 1879-1884* (University of New Mexico Press, 1990); Frank Hamilton Cushing and Jesse Green (ed.), *Zuñi: Selected Writings of Frank Hamilton Cushing* (University of Nebraska Press, 1979), 417-418.

²⁶ Bourke Diary, 10 July 1881, John Gregory Bourke Papers (microfilm).

²⁷ Bourke Diary, 20 November 1881, John Gregory Bourke Papers (microfilm).

anthropologists knew there was something they did not know—a known unknown—that later fieldworkers might attempt to unravel. But it also drew a boundary around the secret keeper’s knowledge that was meant to instruct an anthropologist on what could and could not be known.²⁸ While preparing for the snake ceremony in 1885, Wiki, a Hopi Antelope Society chief, allowed Alexander Stephen to watch him in some preparations, which were designed to soothe the snakes to be used in the well-known public ceremony in which Snake society men carried serpents in their mouths. Rhetorically, Wiki began his lecture to Stephen in common Pueblo idiom, noting his knowledge of secrets as a signal that what followed had gravitas and should not be taken lightly. “What I am to tell no one knows,” he began. And while he did tell Stephen of his role placing prayer-sticks, he only articulated the outline of this rite.

We are taught these things by the old people and I believe them, and no one but me has this secret of placing the *baho* [prayer-stick] for the feeding of my people’s people.... I then take these herbs of which I must not tell you the names, and I prepare the *nakwe’ nakiii* (sacred medicine-water). I sing to my ancestral uncle while I prepare it, not with the lips, but in my heart, that that which I say may not be known, for our people are thieves and liars and their breath is not good.²⁹

While it is risky to take literally the “thieves and liars” translation of Wiki’s lecture, the Antelope chief was clearly wary of witchcraft or the misuse of knowledge by a rival social clique. In fact, Pueblo knowledge stewards of the 1880s often conveyed that they feared the internal circulation of secrets much more than among Anglos, indicating their primary concern, the maintenance of the working of their social fabric. Wiki took pride in protecting sacred elements that helped bring rain to feed his people, even the “thieves and liars” among them, and such secrets he would keep into old age before passing on to a successor.

The boundaries of compartmentalized knowledge were guarded from outsiders. Surreptitious or otherwise unsanctioned viewing of internally stewarded knowledge was prohibited since it broke the regulatory circuit of society members. Even if a young Pueblo man stumbled into a lesson he wasn’t privy to, he might be compelled to join the lesson anyway, to ensure the knowledge transmission followed the correct pathways. This sort of accidental (yet

²⁸ As much poststructuralist theory and history of science scholarship has shown, the designation of knowledge authority links certain people to together through their shared knowledge and excludes others. Shapin & Schaffer, *Leviathan and the Air-Pump*. For both discursive and material sorting and boundary-making, see Bowker and Star, *Sorting Things Out*.

²⁹ Stephen Notebooks, 17 August 1885, Elsie Clews Parsons Papers, APS. Orthography in original.

now compulsory) initiation exemplified the flexibility in the knowledge maintenance system while also ensuring the power of elders over juniors.

The excuse of the accident, as it were, did not pass over to anthropologists as readily. Indeed, as elders hoped to maintain power in their communities from within, they also contended with overwhelming forces from the outside. “Mexicans” (Hispanos) were often suspect and relegated to the fringes of Pueblo settlements; Anglo Americans were typically kept at a distance as well, although many Pueblo communities had what amounted to pro-American factions in the late 19th century. Mormons, Apaches, and Navajos were other large social groups that intervened in Pueblo community autonomy in different ways, frequently through politicking with community leaders to leverage power against one or another rival group. In the 1880s, for instance, a Navajo delegation harshly criticized the Zuni governor Palowahtiwa for his American sentiments, and First Mesa Hopi traditionalists strongly resisted the influence of Nanaje and other pro-American Hopis such that Nanaje spent much of his time at the more friendly Zuni pueblo in the house of his Zuni wife. The tensions at Hopi eventually led to a split of the Oraibi (or Orayvi) village in 1906, in which conservatives against “Washington” founded a new village, Hotevilla (Hotvela).³⁰

Though dismissed by some (and upheld by others) for their “communistic” way of life, that there were factions and cliques illustrate the diversity of political thinking that persisted within a communitarian culture such as that of Pueblo communities in the 19th-century Southwest. Regardless of shared facets of cosmology, communities across the Pueblo ecumene differed on their politics regarding outsiders. Communities like Acoma were known to be less welcoming than, say, Zuni in the 1880s, as friendly communities liked to remind visitors by way of contrast. For instance, a group of Acoma elders chastised a younger man, Garcia, who arrived in town with the army surgeon and ethnologist Washington Matthews in tow in 1882.³¹ The Pueblo was beginning to draw many visitors in the late 19th century because of its storied location atop a mesa, where it had successfully resisted Spanish and Indigenous incursions of the past 300 years.

³⁰ Cushing and Green, *Cushing at Zuni*, 129-130. See also Willard Metcalf’s Journal, December 1882, NAA. For Hopi factionalism, see Peter M. Whiteley, *The Orayvi Split: A Hopi Transformation* (American Museum of Natural History, 2008). For other instances of factionalism in the Pueblo ecumene, see Omer C. Stewart, “Taos factionalism,” *American Indian Culture and Research Journal* 8, no. 1 (1984): 37-57; David Heath French, *Factionalism in Isleta Pueblo* (University of Washington Press, 1949).

³¹ Matthews notebook, 1882 Acoma Materials, WMA.

This position toward outsiders extended to Anglos, and anthropologists were vexed by the closed-lipped Acomans—as well as intrigued by what might be gained by breaking the seal.

Anthropologists might simply arrive at inopportune times. Earlier in 1882, Andrés Ortiz negotiated a stay for Adolph Bandelier at Acoma, and provided the visiting ethnologist with food and lodging. In the midst of the Anglo's stay, Ortiz's wife passed away. Mourning processions occupied his time, and the community was in no mood to treat with an inquiring Anglo who had little connection to the village otherwise. Even in the midst of tragedy, the Ortiz family hospitably received the visitor, although they were very circumspect about the existence of anything "religious." Acoma continued to peacefully demur sustained anthropological inquiry into the twentieth century.

Santo Domingo and the Rio Grande Pueblos also vacillated in their acceptance of Anglos in their midst. Although the Pueblos avoided open resistance to Anglo inquiries and regularly offered hospitality, moments of overt conflict punctuated the ebb and flow of anthropologists traveling the region. Bandelier, as illustrated above, was ejected from Santo Domingo, but he was "starved out" rather than removed by physical force. Firm requests for anthropologists to depart were used at the Hopi village of Oraibi in December of 1882, when the village leadership insisted that an ethnological party, including Frank Cushing, Willard Metcalf, and several others, leave the village. The Oraibi governing council declared a position against US influence, and they compelled Cushing to write a letter to the US President expressing dissatisfaction with their treatment by the Indian Agency and other regional powers.³² And though they threatened violence, in practice they patiently waited out the Americans, who left several days later without much to show for their ethnological ambitions.

Problems and Strategies of Ethnographic Access and Documentation

For Anglo anthropologists on their first field expeditions in the 1880s, access to indigenous communities was not a self-evident endeavor. Aside from the guidance of a few predecessors, the strategy for introducing oneself and one's purpose to a community was open to interpretation. Because professionalized anthropology was only beginning to coalesce in the 1880s, anthropologists did not have much in the way of experience in the field and had to consider the

³² Green, *Cushing at Zuni*, 257. See also, Frank Hamilton Cushing to Mindeleff, 23 December 1882, NAA; Willard Metcalf's Journal, December 1882, NAA.

routes of entry into a community that would provide the best conditions under which to study it. Southwestern anthropologists relied on the most simple and obvious method for engaging with a new community: by way of introduction from someone already known. Introductions could be obtained via traders, Indian agents, and other locals.³³ Paid translators and guides (especially town-dwelling Hispano, mestizo, or Indian guides) could also act as the proxy between a visitor and a community. In the early 1880s Southwest, however, most anthropologists drew on government and especially military contacts to facilitate an introduction to a community, particularly at more remote communities such as Zuni and Hopi.³⁴

Government liaisons provided anthropologists not only with Puebloan contacts, but at times also provided an escort to the community. Counter to the image of the singular “man on the spot,” ethnographic entourages were more common than solo ethnography in the late 19th century. John Bourke, an Army Captain, was commissioned by his military superiors to engage in ethnological work and travelled with two or three soldiers at any given time. Traveling companions filled out the skill-set of the party. A man with an able hand at drawing or skill with cumbersome photographic devices provided another pair of sensory organs to soak up the complexities of the everyday in such different communities. If a ceremonial event or something otherwise out of the ordinary loomed on the horizon, a second or third or even fourth companion would aid the ethnographic objective sought.

An entourage may have benefited an anthropologist’s standing and showed he or she commanded respect and power among Americans. Bourke and Washington Matthews were employed by the US Military, and their affiliation could be used as a mark of power that opened doors. Even those not employed by the military frequently insisted they were working for “Washington” and the “Great Father,” the President of the Americans. Anglos appreciated that working for the “Great Father” increased an anthropologist’s prestige and ability to impress indigenous communities. Captain Bourke, for instance, wrote that a Hopi man, “To-chi, early in the day, had asked me if I was from Washington and had the Great Father sent me; questions which I thought prudent to boldly answer in the affirmative: the dissemination of this answer

³³ When Bandelier reflected on his field experience in a letter to his mentor, he attributed his access to the Catholic clergy. Other anthropologists of the period were less favorably disposed toward clergy, however. Bandelier to Morgan, 22 October 1880, URSC; White, *Pioneers in American Anthropology*, 218.

³⁴ See, for instance, James Stevenson, “Illustrated catalogue of the collections obtained from the Indians of New Mexico and Arizona in 1879,” *Second Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution, 1880-81* (1883): 307–422.

among the men in the Kiba [kiva, a ceremonial space] may have helped me a little, but our strength and the deference paid me by the soldiers impressed them with the idea that I was a person of the greatest consequence in my own country.”

Matilda Coxe Stevenson regularly raised her association with the Bureau of American Ethnology, and literally did so when she hoisted the American flag at her camps adjacent to Pueblo communities.³⁵ Stevenson usually slept outside of communities she studied, dually bound by Anglo gender norms and by Pueblo ones. She distinguished her research by focusing on the lives of Pueblo women and children, an understudied aspect of Indian life in general.³⁶ For male and female anthropologists alike, once accepted into a community, they had to find somewhere to sleep. Setting up a camp reproduced some of the elements of home, a bivouac adjacent to a pueblo’s courtyard that served as a safe space for the visiting ethnographer.

Matilda Stevenson’s stay at Zuni in 1879, during an ethnological expedition led by her husband James and including the photographer Jack Hillers and aspiring ethnologist Frank Hamilton Cushing, presents a classic case of differential access strategies. Matilda, James, Hillers, and the rest of their crew set up a camp outside the Pueblo while Cushing sought a bed in the Pueblo itself, in rooms provided by Palowahtiwa, the Zuni governor. Two divergent strategies emerged: one of collecting items and trading for ethnological information while maintaining the decorum proper to a bourgeois scientific set; and another of lived experience among the people—an early example of “participant observation”—however difficult and demeaning that might be.³⁷

Regardless of the diverging strategies, late-nineteenth-century ethnological parties were data-centric and aspired to professional status. The split in the 1879 party at Zuni showed two paths forward for Anglos aspiring to *be* anthropologists but did not fundamentally recast the discipline. The orderly expedition appeared the most legitimate path for acceptance in the coalescing discipline of anthropology, as it looked to the success of geology and natural history expeditions. The benefits of “going Native” were immediately apparent to Cushing and his

³⁵ Parezo, *Hidden Scholars*.

³⁶ Matilda Coxe [Tilly E.] Stevenson, “The Religious Life of the Zuni child,” *Fifth Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution, 1883-84* (1887): 533–555; Matilda Coxe Stevenson, “The Sia,” *Eleventh Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution, 1889–90* (1894): 3–157; Stevenson, “The Zuni Indians,” 1903. See also Miller, *Matilda Coxe Stevenson*; Parezo, “Matilda Coxe Stevenson: Pioneer Ethnologist.”

³⁷ Triloki Nath Pandey, “Anthropologists at Zuni,” *Proceedings of the American Philosophical Society* 116, no. 4 (1972): 321–337; Fowler, *A Laboratory for Anthropology*; McFeely, *Zuni and the American Imagination*; Mark, *Four Anthropologists*.

superiors in Washington. By joining the Pueblos in their daily lives, Cushing was introduced to facets of the people and their culture that few Anglos had been able to see and experience.³⁸

While Cushing developed his immersive ethnographic strategy to best serve his aspiration of becoming an anthropologist, some Anglos only turned toward ethnography after living in the Southwest for some time. Alexander Stephen had been a prospector and miner in the 1860s and 70s before he planted himself at Keam's Cañon, at the foot of the Hopi mesas, in January of 1880. During the next decade, Stephen married a Navajo woman and worked alongside Thomas Varker Keam at his outpost store. Stephen began to learn Navajo, a common idiom at Keam's store, and later sought to learn the Hopi language. Both Hopi and Navajo people knew Stephen and interacted with him regularly and appeared to tolerate his ethnographic queries. Jeremiah Sullivan, a doctor at the Hopi Indian agency who later "became" Hopi, also assisted Stephen in winning the trust of the Hopi. Sullivan had ethnological aspirations that went unrealized, but he aided Stephen in preparing ceremonial notes on Hopi and facilitated the visits of other visiting Anglo ethnographers. Bourke and Matthews, both active-duty military men in the 1880s, initially had other reasons for being in the Southwest, although both had longstanding ethnological interests. Those who came to ethnography during their time in the region likely realized their experience in the field was an advantage, since their varied roles (Stephen was a prospector and trader, Matthews and Sullivan were doctors, Bourke was an aide-de-camp and scribe) structured their interactions with indigenous communities in unique ways, thus providing new angles to pursue ethnography.³⁹

For anthropologists seeking to study Pueblo communities in the 1880s, the ability to get into a community was a relatively simple endeavor, and one that could be accomplished in multiple ways. But tolerance and acceptance were different things for Pueblo communities. Anthropologists often encountered resistance to certain lines of inquiry once they were situated in a community. Pueblo hospitality provided for kind treatment and food as could be spared but did not necessarily mean that the residents were apt to speak much about themselves or their habits and traditions.

³⁸ For Cushing's later expedition, see Hinsley & Wilcox, *The Southwest in the American Imagination*; Frank Hamilton Cushing, Curtis M. Hinsley, and David R. Wilcox, *The Lost Itinerary of Frank Hamilton Cushing*. (University of Arizona Press, 2002).

³⁹ For a different assessment of Stephen and Sullivan's respective ethnological work, see Louis A. Hieb and Susan E. Diggle, "A Question of Authorship: A. M. Stephen's Catalogue of the Keam Collection [1884]," *Kiva* 69, no. 4 (2004): 401-423.

Hospitality is, after all, both an act of welcoming and an act of boundary-maintenance.⁴⁰ When an anthropologist entered a pueblo and was hospitably received, the Pueblo community was engaging in (polite) maintenance of his or her status as guest and outsider. Hospitality recognizes the guest as “special,” but this status marks the guest also as just that: an outsider temporarily invited in. This spotlighting mechanic in hospitality serves to manage the outsider’s influence or role in the host community; in Pueblo communities, as shown above, the anthropologist’s “privileged” or marked status was usually maintained by one or several “hosts,” who were charged with providing the necessary actions and items that mark the outsider as a “special guest.” Polite and special treatment, in other words, come to appear as efforts at restricting the access of the anthropologist, rather than simply facilitating it.⁴¹

Specific inquiries of Indian cosmology were frequently met with resistance and dissimulation, as we have seen. Anglos were also regularly prohibited from taking photographs or sketching sacred items. Frank Cushing, who had been repeatedly scolded for writing and sketching events even after he was an accepted member of the Zuni community, understood the Pueblo position and suggested taking some Zuni leaders on a trip to see where the items and images collected from their community were ultimately on display. “The advantages which might accrue from such a tour would be inestimable,” Cushing wrote of a proposed trip to Washington, DC, with some Zuni:

They would, moreover, understand our objects in collecting their things, how “sacredly we care for them in the Great Estufa at Washington,” and would then make no objection to our gathering not only sketches but also actual objects belonging to their esoteric orders, sacred institutions and ceremonials. Their objections thus far have been—and are—so great that any work I have done in this direction has been accomplished only by stratagem, which falls very short of complete success.⁴²

For Cushing, once Zunis saw the respectful representation of their lifeways in Washington, they might no longer see his extractive impulse as a potential threat.

⁴⁰ Matei Candea and Giovanni Da Col, “The return to hospitality,” *Journal of the Royal Anthropological Institute* 18, no. s1 (2012);

⁴¹ Benjamin Boudou, “Éléments pour une anthropologie politique de l’hospitalité,” *Revue du Mauss* 40 (Feb. 2012): 267-284; Kim Meijer-van Wijk, “Levinas, hospitality and the feminine other,” in Conrad Lashley (ed.), *The Routledge Handbook of Hospitality Studies*, (Taylor & Francis, 2016); Paul Lynch, Jennie Germann Molz, Alison McIntosh, Peter Lugosi, and Conrad Lashley, “Theorizing Hospitality,” *Hospitality & Society* 1, no. 1 (2011): 3-24.

⁴² Frank Hamilton Cushing to Pilling, 25 December 1881, NAA.

Cushing's "stratagem" for information extraction had been to ennoble himself with the Zunis and consistently push the boundaries of what could and could not be documented. If he encountered resistance—he claimed a knife was pulled on him while sketching early in his time at Zuni—he would attempt to persuade his hosts to consent to his actions. He contented himself to sketch in secret, although that too was difficult in his early time at Zuni because he was consistently under surveillance.

I was regarded as a sort of black sheep on account of my sketching and note-taking, and suspicions seemed to increase in proportion to the evident liking they began to have for me. Day after day, night after night, they followed me about the pueblo, or gathered in my room. I soon realized that they were systemically watching me. They were, however, pleasant about it, and constantly taught me Mexican and Indian words, so that I soon became able to carry on a conversation with them.⁴³

Clearly, Zuni men traveled with Frank Cushing when he journeyed out of the main village, for reasons of aid as well as surveillance. Pueblo men were not uncommon traveling companions for ethnographers, as when Pedro José accompanied Bourke on an extended tour of Pueblo villages. Juan José Montoya and his brother José Hilario, of Cochiti, guided Bandelier to various archaeological sites and helped him establish connections at other towns in the region.

Observation and misdirection duties could be held by nearly anyone in the pueblo. Young Pueblo boys frequently acted as guides for anthropologists, and they could be relied upon to direct the ethnologists *away* from the most sacred shrines and ruins in the landscape as well as inform the caciques of an anthropologist's actions while away from the main group. What Cushing saw as surveillance and misdirection, his hosts likely understood as serving to further enroll Cushing into the Zuni community. Gossip, teasing, and overt and arch criticism were not reserved for insiders alone, and across Pueblo communities these social interactions emphasized proper comportment and alignment the limits of compartmentalized knowledge.

Through Zuni suggestions and actions, Cushing was molded into an adopted member of the community. This was ultimately Cushing's most successful "stratagem" for accessing information, although the Zuni certainly controlled the conditions of his acceptance and compelled him to act in certain ways. He began to dress like a Zuni and eat only meals prepared by the tribe. "By appearing in the ancient national costume (at my Zuni friends' instigation) I

⁴³ Frank Hamilton Cushing, "My Adventures at Zuñi," *The Century Illustrated Magazine* (1882-1883).

have succeeded in lulling their newborn suspicions to such an extent that this morning they were very free with their information regarding the meaning of certain plumes and prayers.”⁴⁴ By sleeping outside to “harden his meat” and undergoing other “feats of fortitude” (as he once called it), Cushing’s reputation grew among the community, as did his access to more “secret,” private Zuni practices. But because the Zuni wanted to adopt him as their own, the only element of his “stratagem” he truly controlled was his ability to betray, or keep, their trust.

Other anthropologists in the 1880s did not attempt to integrate themselves into a host community as far as Cushing had. For the most part, Southwesternists developed strategies for accessing ethnographic information that were more direct, and often involved ignoring the protestations of Pueblo communities and simply doing as they liked. When John Bourke descended into kiva preparations for the Snake Dance at Hopi in 1881, he understood the value of playing dumb.

Knowing how important it was that some memoranda of this curious rite should be preserved, I quietly ignored all hints and when addressed by the more aggressive always made the mistake (!) of supposing that they wanted to shake hands and bestowed upon them a pump-handling sufficiently energetic to win me many votes had I been electioneering for them.⁴⁵

Ethnographers often blithely presumed that they were able to access the centers of instruction and religious performance. They did not, in other words, carry the same appreciation of the limits and conditions of access that they had in Anglo American society, molded as such were by class, race, gender, age, and educational pedigree.⁴⁶

Winking at himself in his diary (!), Bourke believed that feigned ignorance was a viable strategy. And, in many ways, it was. This common strategy for access was grounded on the notion that Anglos and Indians did things differently, and that Pueblos might simply let the Americans carry on out of a sense of politeness. Bourke’s attempt to exploit politeness, however, came up against the protective aspect of hospitality, of delimiting and managing the actions of the interloper. When Nanaje confronted Bourke about his over-eager hand-shaking, he expressed

⁴⁴ Cushing to Baird, 19 November 1879, NAA.

⁴⁵ Bourke Diary, 12 August 1881, John Gregory Bouke Papers (microfilm).

⁴⁶ For American mores around the turn of the century, see Sven Beckert, *The Monied Metropolis: New York City and the consolidation of the American bourgeoisie, 1850-1896* (Cambridge University Press, 2003). Alan Trachtenberg, *The incorporation of America: Culture and society in the gilded age* (Macmillan, 2007); T. J. Jackson Lears, *No Place of Grace: Antimodernism and the Transformation of American Culture, 1880-1920* (Pantheon Books, 1981).

both his personal reaction and the ethic of Pueblo hospitality, which in this case dictated tolerance. “We didn’t like to have you down there; no other man has ever shown so little regard for what we thought, but we knew you had come under orders and that you were only doing what you thought you ought to do to learn about our ceremonies. So we concluded to let you stay.”⁴⁷ Bourke had conveyed his charge, associated himself with the Great Father in Washington, and played the fool—in the end, the Hopis were annoyed but tolerant enough to allow him to continue, albeit with increased attention to how they managed the visitor’s experience.

Of course, anthropologists also anticipated resistance to their bold moves; feigned ignorance presupposed the potential for pushback. In response, some anthropologists waited until nightfall to sneak into the kiva to sketch an object or two. In a particularly egregious act, James Stevenson snuck into the Zuni church after sundown and stole some objects from the altar, writing later that he “got them in the dead hour of night.”⁴⁸ Years later, after James’ untimely death, his wife Matilda Stevenson likewise made off with two Zuni idols from a remote altar and deposited them in the National Museum in Washington. Although anthropologists rarely cited such acts in their own notes or reports, other material thefts transpired, and these crimes were relayed by Pueblos to later anthropologists.

Though bold moves occasionally caused ejections, anthropologists primarily acquired objects through purchase or exchange. Additionally, anthropologists realized that money could shift the system of expectations that imbued the hospitality relationship. Buying something from Indians, in other words, gave anthropologists leverage to ask questions that could otherwise be avoided. Juan Anaya, a governor of Laguna, was happy to sell items to John Bourke, only to have him use his buying as a bridge to more intimate conversation, which the seller felt obliged to continue.⁴⁹ When anthropologists opened purchasing negotiations, they also stumbled upon information about what Pueblos valued. The Zuni desire for shells, for instance, alerted fieldworkers that common shells that washed up on Eastern beaches were dearly held by Zunis. Frank Cushing wrote to his supervisor in the Smithsonian Institution, thanking him for sending white scallop shells, noting “They especially advance my influence, as they give me the reputation and standing of a holder of sacred property.”⁵⁰ The market for certain shells among the

⁴⁷ Bourke Diary, 12 August 1881, John Gregory Bouke Papers (microfilm).

⁴⁸ Fowler, *A Laboratory for Anthropology*, 107.

⁴⁹ Bourke Diary, 30 October 1881, John Gregory Bourke Papers (microfilm).

⁵⁰ Cushing to Baird, 19 Dec 1880, NAA. See also Cushing and Green, *Cushing at Zuni*, 136.

Zuni was ethnographically relevant in itself, as well as for the potential transactions enabled by the relatively “worthless” shells. Anthropologists also noted the price of indigenous goods on offer, which in connection with their consumer demands revealed potential exchanges that could be exploited for the benefit of any unscrupulous Anglo buyer.

Anglo purchases illustrated that ethnography at times veered toward an accumulation of as much “stuff” as possible for a given community—pottery, prayer bundles, masks, “trifles” and other paraphernalia were sought. But anthropologists also offered money directly for information. This transaction usually demanded an explanation as to *why* an Anglo desired such information, *why* it was valuable to them. Typically, an anthropologist presented his or her project as enabling a better relationship between Indian communities and Anglos and/or the government. Adolph Bandelier claimed that the accumulation of knowledge about Indian communities would enable better relationships between the government and indigenous groups. Sympathy, especially from the US Government, was the common explanation for asking after or purchasing information. John Bourke, for example, prepped an informant by telling him that previous disagreements between Indian peoples and the United States “had undoubtedly been aggravated, if not occasioned by a want of knowledge of [Indian] manners, customs and ideas.” By sharing information, it followed, informants could help dispel Anglo ignorance.

Buying goods and information from Pueblos was more telling, however, when a particular purchase was impossible. When an Anglo made an offer on a prayer stick or a finely carved tobacco pipe, their offer might be rebuffed and further negotiations foreclosed. When anthropologists could identify what was *not* on offer—what could not hold a price, what was not for sale—they realized that such objects must hold great symbolic value to the community that it and could not be parted from it. Unpurchaseable objects, then, pointed to additional ethnographic knots to unravel, not to mention potential items anthropologists might “accidentally” make off with when they finally left the pueblo.

The Informant’s Importance

As the century neared its end, anthropological work on Indian groups became more specialized and specific. Thinkers became fieldworkers, fieldworkers became thinkers. Anthropologists spent more time among Indian communities and began to investigate their inner or mental lives. In and through that increased duration, Indian communities altered Anglo

perceptions of what constituted Native communities and how they differed from one another. Anglos came to see that Puebloan communities were distinct but related, and that their unique historical paths could be outlined and speculated upon. In taking up a more detail-oriented ethnography, anthropologists inched ethnology away from stagist theories of universal social evolution.

Realizing differences within a broader shared worldview and cosmology, anthropologists of the Pueblo began to attend to specific cultural practices that went beyond the accumulation of language data and objects of everyday life. As the anthropological network of Southwesternists grew throughout the 1880s, and publications like the *Annual Report* published by the Bureau of Ethnology circulated among Anglo scholars in the region, a body of knowledge about Pueblos and other Southwestern communities developed a solid ground upon which further study could build. The initial field had been outlined, but many questions remained. Some of these questions had been tucked in Anglo notebooks for months or years—why wouldn't Santo Domingo tell Bandelier the word for sun? Why did Nanaje tell Bourke that they didn't want him in the kiva? Why was Matilda Stevenson's guide so terrified to show her a sacred cave?⁵¹

There appeared, in short, an object available to study directly beyond the elements that could be seen and heard during a brief stop in a community—a known unknown. To come to know what was unknown would, it was hoped, address the questions of belief systems and causal explanations, and such questions could often only be asked when anthropologists had proven themselves to the community. It took time for trust and friendship, or at least an honest tolerance, to build between Anglos and Pueblos. It took even longer to be able to ask why and receive an answer.

Tolerance at the community level provided access, but hospitality also imposed limits on the extent and depth of ethnography queries. Matilda Stevenson in 1884 had been known at Zuni for 5 years, and her acceptance allowed her to roam about Zuni with relative freedom. One night, she cautiously entered into a meeting of the Ne'wekwe (Galaxy) Fraternity, which was serving a meal at its kiva. The members were surprised to see her, but welcomed her in. She remarked on a tablet altar and asked about a bat motif presented upon it. A Ne'wekwe member replied that “if a man sees a bat when he is on his way at night to plant prayer plumes, he is happy, for he knows

⁵¹ Stevenson, “The Zuni Indians,” 154-155.

that in four days there will be much rain.” Meanwhile, a member of the society got up and walked over to an object in the room, which Stevenson knew to be a sacred carving, and covered it with a blanket. Stevenson “appeared unconscious of any objects beyond the group of men [around] the food.” Thankful the visitor had appeared to not notice the sacred object, the Zuni men sighed with relief. Later, Stevenson asked a Ne’wekwe man about what she had glimpsed. “Those whom the writer afterward questioned regarding the fetish at first denied all knowledge of it.” Undeterred, Stevenson pressed an informant for more information. In one-on-one questioning, she could apply pressure in the absence of others. Eventually, her informant offered up an explanation of the meaning of the stone carving.⁵² Stevenson’s experience in the Ne’wekwe society house was one of friendly tolerance but also of wariness about her status as an outsider. Some things could be explained freely—a bat was a good omen for rain—whereas others, such as the carving and its meaning, had to be asked about repeatedly, often outside the gaze of other community members.

Experiences like these attuned Anglo anthropologists to the benefits of intimate friendship beyond mere community acceptance. It was more difficult, after all, to ask a group of men to share information. It was better to ask a single man, alone, out of view of others. Even better was to ask a friend, someone who shared mutual respect and a desire to conceal as little as possible. Indeed, Pueblo friends could help an anthropologist obtain her desired information, not to mention provide pictures and objects that were otherwise unavailable to outsiders. During a ceremony, one of Matilda Stevenson’s friends “was untiring in her efforts to detain an old father below while the writer secured photographs on the roof, and several times released [the writer] when the father had barred the door of her room with heavy stones. The wrath and distress of the old man knew no bounds, and he declared that the writer would bring calamity not only to herself but to all the household.”⁵³ Here, friendship and a sense of duty to Stevenson allowed her informant to offer assistance even against the wishes of an elder.

Such relationships were crucial to anthropologists because they offered unique perspectives on Indian life. They also served as levers for advancing the disciplinary status of anthropology. Many of the studies that populated the pages of the *Annual Report* of the Smithsonian’s Bureau of Ethnology required knowledgeable companions, typically those who were themselves distinct

⁵² Stevenson, “The Zuni Indians,” 432.

⁵³ Stevenson, “The Zuni Indians,” FN a.

within their communities for specialized knowledge. Specialized topics distinguished ethnographers, not only from the data accumulators of schedule-based ethnography, but from their disciplinary brethren. This was especially the case in Southwesternist anthropology. The Southwest was heavily favored in the Bureau reports, and the 28 published articles on Pueblo or Navajo topics composed nearly 1/3 of all ethnographic articles. Of these, 15 articles were specialist topics that would have required an informant with unique knowledge. These texts focused on particular knowledge communities or intimate processes that could not be witnessed in passing.

Folklore and mythology were also emerging concerns, and a complementary outlet, the *Journal of American Folk-Lore*, was established in 1888. Folklore and mythology were broadly construed as narratives that a culture produced to explain its history and present practices. Folklore was instructive in its content; the recipe for a medicine might be remembered through a folk tale's plot, wherein a character collected supplies and honored certain deities while preparing a concoction. While JAFLE did not have quite the Southwestern focus as the Bureau of Ethnology's *Annual Report*, Southwesternists John Bourke, Washington Matthews, and Jesse Fewkes were frequent contributors and reviewers. The overall cast of articles in the early years of the JAFLE skewed towards "belief systems" and "behavior" of various sorts, and Native Americans were overrepresented. Studies of specialist knowledge and belief systems marked a turn toward an epistemological mode of inquiring in anthropology, toward the study of the "inner life" and reasoning and opinions of indigenous people—the branch of ethnological science that John Wesley Powell had deemed "sophiology." This shift, in turn, rendered the anthropologist-informant relationship essential, for a single informant offered the best route to intimate information. The informant became of central importance, possessing a resource to be extracted.

The importance of a single informant—or series of like informants—took on profound relevance for Matilda Coxe Stevenson's work. Focusing on women and children in Pueblo communities, Stevenson consistently presented a supplementary perspective on Pueblo life throughout her career. Her informant, We'wha, was crucial to her work. We'wha was a berdache (*lhamana* in Zuni), a man who combined the work and social roles of both men and women, who dressed in women's clothing; We'wha grew up as a Zuni boy would, and received male-inflected religious training until he displayed *lhamana* characteristics and his tutelage was transferred to

women's circles.⁵⁴ This included cooking, housekeeping, fetching wood and water, architectural upkeep, gardening, and ceramics. Stevenson, although long confused about his biological sex, saw We'wha as singular and praised him mightily in her introduction to Zuni studies, calling him "the strongest character and most intelligent" of his people.⁵⁵

Other Zunis informants such as Pedro Pino, Palowahtiwa, and Naiuchi acted as key informants to ethnographers such as Cushing, Bourke, and Fewkes. While not always the case (as shown in the following chapters), in the 1880s and 90s, these relationships were public knowledge. In addition to Zuni, Hopi informants also appeared open to information-sharing. The prominence of Zuni and Hopi was reflective of their relative size and settlement diversity to the Rio Grande Pueblos and other communities in central New Mexico. But even in the smaller Pueblo villages, evidence indicates informants such as Juan José and José Hilario Montoya of Cochiti openly maintained strong relationships with anthropologists, especially Bandelier, for years to come.

For these Indian informants, friendship with Anglo anthropologists carried both burdens and benefits. In Pueblo communities, he or she might gain prestige, a form of importance bestowed on him because this outsider considered his knowledge important. Pedro Pino, Palowahtiwa, and Naiuchi held powerful positions at Zuni; they could be criticized, and were, but they did not appear to realistically see negative consequences due to their relationships with anthropologists. But not all community members had the same privileges, and not all Pueblo communities had the tolerance of late 19th-century Zunians. Within another community, an informant might lose stature because of his association with an Anglo because of a concern he or she was revealing too much information, was seen as privileging an outsider over his own people. However, Pueblo who worked closely with anthropologists possessed a form of power *over* the visiting anthropologists—the power to shape, to some degree, the way they practiced anthropology—while also potentially imperiling themselves in their own communities. Modulating Anglo secret-seeking was thus also a task for Pueblo informants. Their social position as knowledge stewards was key to their value as informants to anthropologists, and to this role was added another, unasked-for: to instruct anthropologists how to act, how to ask questions, how to know the limits of what could and could not be written down.

⁵⁴ Will Roscoe, *The Zuni Man-Woman* (Albuquerque, N.M: University of New Mexico Press, 1996), 38.

⁵⁵ Stevenson, "The Zuni Indians," 20.

Across the Pueblo ecumene, an “anthropologist” was just another Anglo until he or she started posing probing questions beyond what the everyday soldier or trader might ask. For Pueblo communities, a mode of behavior we might call “inquiring outsider” had already come into existence through experiences with Catholic missionaries and Spanish and Mexican bureaucrats.⁵⁶ These experiences awakened suspicion and resistance when the first anthropologists arrived on the scene. Anglos were newcomers to the region, and it appears that wariness about “inquiring outsiders” lingered across the Pueblo ecumene, especially those closest to Santa Fe and Albuquerque.⁵⁷ Most troubling, perhaps, was that these new Anglos were particularly curious about the minutiae of the cosmology and social governance of their community. No doubt the Pueblos perceived the Anglo drive for knowledge as intrusive and anxiety-producing, for it disrupted the distributed and compartmentalized qualities of their knowledge system.

Pueblos learned that anthropologists asked questions—a lot of them. These questions ranged from the banal and everyday to more intimate corners of Pueblo life. Questions about basic vocabulary and belief systems became commonplace enough that some Pueblos would associate Anglos with the kind of questions they asked. Pedro Pino of Zuni, although friendly with many anthropologists including Cushing and Bourke, asked the latter in a grouchier moment, “Why is it that the Americanos always ask for such things? Whenever I meet Americanos they always say ‘Tell us when the world was born’; ‘Tell us how the Sun was made?’ and I think it very curious that with all the books the Americanos have they couldn’t . . . find out those things for themselves.”⁵⁸ Here, Pino revealed a Zuni perspective on Anglo knowledge systems, that knowledge in America was not intimately stewarded, as it was in Zuni, but rather kept on paper and in books and available to those who could read them. He also gently teased Bourke to impress upon him a notion of Zuni compartmentment, drawing on his own community’s form of social regulation.

⁵⁶ Dozier, *The Pueblo Indians*; Gutiérrez, *When Jesus Came*.

⁵⁷ For the role of Spanish Catholicism on Pueblo Secrecy, see Joseph Suina, “Pueblo Secrecy: Result of Intrusions,” *New Mexico Magazine* 70, no. 1 (1992): 60–63; Tisa Wenger, *We Have a Religion: The 1920s Pueblo Indian Dance Controversy and American Religious Freedom* (Chapel Hill: University of North Carolina Press, 2009). C.f. Brandt, “On Secrecy and Control of Knowledge”; Tracy L. Brown, *Pueblo Indians and Spanish Colonial Authority in Eighteenth-Century New Mexico* (U of Arizona Press, 2013). Both texts complicate the sole attribution of Pueblo resistance to Spanish/Catholic colonialism via secrecy.

⁵⁸ Bourke Diary, 23 November 1881, John Gregory Bourke Papers (microfilm).

Pueblo informants regularly conveyed the problem of sharing sensitive information to anthropologists, as shown in the preceding sections. Nanaje taught Bourke that keeping secrets was for the benefit of the whole world; Wiki informed Alexander Stephen that his specialized knowledge was not for everyone, and especially needed to be prevented from circulation among the cynical and ignorant of his own people. Nanaje and Wiki told anthropologists of the precarity of the knowledge they held, and no doubt hoped Anglos would desist from writing and sketching their more sacred forms of knowledge. And, in conjunction with Pedro Pino's comment about American books, these Pueblo men sought to emphasize that their knowledge system could not support reproductive documentary technologies while also recognizing that Anglo knowledge was, indeed, built upon them.

Pedro Pino's son, Palowahtiwa, also drew strong associations between Anglo knowledge systems and the written word. Palowahtiwa understood Cushing's writing as part of an Anglo tradition of "guessing." Writing gave Anglos the "gift of guessing," or the ability to tell information to another person without being physically present.⁵⁹ "Guessing" signaled Palowahtiwa's appreciation of the disembodied nature of written communication—that one could believe its contents, but such believability also risked a form of intelligent guessing.⁶⁰ Palowahtiwa's notion can be understood as more than a critique of Anglo information production, although that reading is understandable. Rather, Palowahtiwa's denotation "gift of guessing" recognized that true and untrue things could be written with the same appearance, which placed knowledge in a position where it could be decontextualized. Written knowledge could not be stewarded and responsibly presented, and the reader needed to "guess" to fill in the context of the message.

Pueblos of the 1880s and 1890s understood textual and graphic material technologies similarly: writing and sketching and photography transformed the present world and historical knowledge into a transportable object that could be deployed elsewhere. To many Pueblos, writing risked violation of the ethic of responsibility—if knowledge was extracted, one could not be sure it would be properly cared for. Photos and image-making were suspect because they

⁵⁹ Thinking about writing and scientific authority in terms of mobility and authority is a theme in History of Science and STS literature. For the classic case study, see Shapin & Schaffer, *Leviathan and the Air-Pump*.

⁶⁰ Palowahtiwa's Speech was featured in the *New Mexican Review*, 19 July 1883. See also Cushing and Green, *Cushing at Zuni*, 301-302.

captured someone's likeness, and such imitative magic could be used for nefarious purposes.⁶¹ Thus cameras and sketchpads, paints and pencils, were not contentious objects in themselves, but the transportability of the resulting media could pose a threat to the community's stability.⁶² This was not immediately and necessarily a problem of outsiders gaining knowledge; often, the most pressing worry was that someone within the community might take up such data and be enticed to manipulate it for their own ends.

It is tempting to understand the Pueblo outlook on documentary technologies as wary of the "materialization" of knowledge. Documentary "materials" could certainly identify an Anglo as an anthropologist; the itinerant John Bourke was given at least two titles that referred to his constant writing—the Sioux of Pine Ridge called him a *Minai-ho-a Man* or an "Ink Man," and the Western Apache deemed him a *Naltsus Bichidin* or "Paper Medicine Man."⁶³ But Pueblos had long encountered storekeepers, soldiers, and politicians that wrote on paper, enjoyed taking photographs, and sought goods to add to their collections. Documentation was only one element that constituted the "inquiring outsider" for Pueblo communities. Indeed, Pueblos used writing systems to their own ends, and understood that documents could help and protect them under the American system of government (even if their beliefs were proven wrong by racist courts and nefarious title-writers). Palowahtiwa and other Zunis often enrolled Cushing in making written accounts. The Santo Domingo and Cochiti people used Bandelier's writing abilities. Among the Hopi and Navajo, Keam and Stephen could be called upon to write letters. John Bourke was at times called on by Indian communities for his original training, as a scribe.

Instead, it was an additional, immaterial trait by which Pueblos identified anthropologists and presented their contrast with other Anglos: the strangers were preoccupied with intimate knowledge of community lifeways, knowledge that went beyond simply writing down words or sketching pottery or grinding-stones. And, most critically, this knowledge needed stewardship.

⁶¹ For instance, when a Zuni dressed as a Catholic priest during a Clown Society performance, for instance, he was neutralizing a priest's potential influence in the community. While photography and image-making are important to a discussion of Pueblo secret keeping, I have focused more on the politics of documentation in the more common practice of notetaking and writing. For photography and indigenous peoples, see Elizabeth Edwards, *Anthropology and Photography, 1860-1920* (New Haven: Yale University Press, 1997); Gwyneira Isaac, *Mediating Knowledges: Origins of a Zuni Tribal Museum* (Tucson: University of Arizona Press, 2007); Luke Lyon, "History of Prohibition of Photography of Southwestern Indian Ceremonies," in *Reflections: Papers on Southwestern Culture History in Honor of Charles H. Lange*, edited by Anne V. Poore (Santa Fe: Ancient City Press, 1988).

⁶² Dutton, *American Indians of the Southwest*. See also Colwell-Chanthaphonh, "Sketching Knowledge."

⁶³ "Ink Man" in Bourke Diary, 20 June 1881, John Gregory Bourke Papers (microfilm); "Paper Medicine Man" in Bourke Diary, 13 October 1882, John Gregory Bourke Papers (microfilm).

Anthropologists were a meddling sort, people who wanted to know things that didn't concern them, that weren't available to them, in the midst of a people whose community life at times was bound to the contours of concealed knowledge.

This emerging dynamic of the anthropologist and the informant opened up a new field of possibilities for both parties, although the burdens would disproportionately fall on Pueblo communities as a whole. Pueblos faced a range of options for how to best treat anthropologists: as persons to oppose at risk of alienating American support, as persons to mislead so as to protect the traditional knowledge structure, as persons to use for personal prestige and material benefit at the expense of sanctity—or as potential friends, held by bonds of trust. Anglo choices, in contrast, ranged among the ways to flip a Pueblo knowledge keeper, to compel him or her to share stewarded knowledge, through friendship or payment or deceit. In this regional context, indigenous communities helped shape what it meant to *be* an anthropologist by and through their reactions to Anglo inquiries about secret, private, or compartmentalized matters.

Through ethnographic experiences in the field, the hazy terrain of the known unknown presented an opportunity for future anthropological researchers. After having been “starved out” during his first fieldwork in Santo Domingo, Adolph Bandelier had altered his tactics and developed lasting relationships with other Pueblo communities, but also kept in mind the conservatism of the people he first encountered. Santo Domingo would best facilitate “penetrating the secrets of the Keres,” Bandelier later claimed.⁶⁴ Secretseeking was to become a major tool for anthropological inquiry, a sort of divining rod for core elements of Indian social and intellectual life.

Anglo anthropologists relied on an array of access strategies in the early years of ethnography among the Pueblo Indians. They developed contacts, made friends, and referred to their prior knowledge; they acted on assumed permission and feigned ignorance; they deployed questionnaires; they wrote detailed descriptions that framed future research; and they purchased, traded for, and sometimes stole objects and information. The accumulated data and experience among Pueblos allowed Anglo anthropologists to form a network and archive of information that grounded their nascent discipline. For anthropologists, research patterns and intellectual

⁶⁴ White, *Pioneers in American Anthropology*, 219, FN 1.

questions began to emerge, and there was a palpable sense of excitement around this thing they were creating: Anthropology.

In tandem with the actions and reactions of Pueblo people to Anglo ethnographers in their midst, the ethnographic encounters of the 1880s established patterns of interaction and thinking that persisted for some time to come. It is certainly true that anthropologists reveled in the experience of learning secret knowledge. But secrets, I argue, also came to be a fountainhead of anthropological knowledge in Southwestern communities, and thus the accumulation of secrets became a primary objective of ethnographic inquiry in the late 1880s and 1890s. And secret-seeking, as later chapters will show, persisted into well into 20th century.

For Pueblo Indians, too, the Anglo anthropologist seeking secrets became a recognizable type of person. The inquiring Anglo, it appeared, could be tutored and befriended, could be watched, could have his or her skills appropriated for the benefit of the community or the informant. While protecting their traditions from outsiders, Pueblo knowledge keepers determined the scope of Anglo access that involved a desire for secret knowledge. At the same time, as Pueblos set and continually modified the conditions of access, new opportunities arose for Indian informants. Paradoxically, an informant's contributions to anthropological knowledge placed them in a precarious position within their own communities, even while the information they provided could one day be called upon if the people forgot or otherwise lost the knowledge they held dear.

Chapter 4
The Price of Fear:
Shifting Valuations of Information and Clandestine Exchange Relationships

Elsie Clews Parsons was no stranger in Zuni. On the contrary, the Anglo woman from the East was a known entity to the people of the pueblo. She was, to the people of Zuni, one of those inquiring outsiders, who asked questions and wrote down the answers in little notebooks. This trait did not mean that the people of Zuni disliked her. She was generous and to some, like Margaret Lewis, the Zuni governor's wife, even a friend. She gave gifts to the Zuni children who followed her constantly, and occasionally Anglo-style dresses to their mothers. Perhaps, although few would admit it or do so only blushing, she may have even been crushed on.

The people of Zuni understood, however, the boundary between her and them. Like other anthropologists, Parsons asked questions that reached too far into the domains of stewarded knowledge, areas that had special meaning to the Zuni and could not be shared with outsiders. An unspoken rule to not to share sensitive information with this outsider persisted, as it did with all of the visiting anthropologists. In interactions with the friendly Anglo, Zuni men and women surely kept their guard up so as to avoid sensitive topics. Occasionally, however, to curry favor or perhaps merely in absentmindedness, they might share information that their fellow community members might take issue with.

Billí, and older man, had once spoken to Parsons about sensitive Zuni issues. Realizing his transgression in the moment, he expressed his anxiety to Parsons. But time healed his trepidation, as he experienced no consequences for sharing a bit of knowledge. A year after the aforementioned conversation, Billí went to Parsons again. The young woman had returned to the Pueblo and, though restrictions persisted, Parsons asked Zuni people in hushed tones about sensitive topics. Billí—perhaps jealous of others who were close to Parsons, perhaps feeling some economic strain, or maybe just realizing an opportunity—approached Parsons with a proposal. He would show her an old altar and writings on rocks, petroglyphs that Zuni ancestors had created long ago to convey stories to their people. That she would pay him for this

information was already understood, since Parsons had often offered money to other Zunis for sensitive information, which they publicly declined. But there was another condition for Billí to share the altar and petroglyph site with Parsons: she needed to keep this a secret between the two of them.¹

As negotiations about payment for his guidance began, Billí asked Parsons: “Quanto paga por mi miedo?”—“what will you pay me for my fear?” Here Billí showed his accounting for risks, for he would have to be sneaky about showing Parsons the altar and petroglyphs. If it was found he had done so, his reputation would suffer. His price of fear implicitly bound her to him, making her responsible for his safety.

Parsons, in her creative re-telling of his story, replied: “How do I know what to pay for your fear until I see the place we are going to? If it is a good place, a handsome place, I’ll pay you so you will be satisfied.” Retorting to Billí’s question with one of her own, the two ultimately agreed that a fixed price could not be established, but rather one based on the quality of the information. This was, of course, a criterion set by Parsons, based on her perception of authenticity and the importance of the site relative to her studies of Zuni ceremonialism up to that point. Billí, for his part, would need to view Parsons as an honest patron, and appeared to do so in this case. Parsons had, by most accounts, paid her informants fairly. Billí trusted that she would compensate him adequately.

Billí directed Parsons toward a meeting place near the glyphs; meanwhile, he took a longer and more obscure route. Then, “a mile or two beyond the reach of questioning, he descended by some step-way he knew to meet me in the narrowing canyon.” After they arrived, Parsons set to sketching and photographing the petroglyphs. A shrine was built around the petroglyphs at which Zunis still made offerings. Though petroglyphs proved interesting, Parsons was more interested in the prayerstick bundles left at the altar. These she sought to compare with bundles she had acquired from elsewhere, as she was perplexed by the role of prayer sticks in Zuni ceremonialism. Presumably, here she questioned Billí about prayerstick creation, theory, and use.

Billí grew anxious as the sun crossed overhead. They had to get back to town before long, before anyone connected Billí’s absence with Parsons’s jaunt into the canyonlands. As the two parted ways just outside the town, the time came to discuss the “price of fear.” Billí seemed to

¹ Parsons, “In the Southwest,” unpublished manuscript, Elsie Clews Papers Papers, APS. Billí appears to be a pseudonym, and thus is reproduced here.

understand Parsons might offer him a modest sum but be willing to increase it with an overture to their new, secret bond. To Parsons's offer of \$5, Billí replied, "Bueño, but a dollar more because I am your friend." Parsons, agreeing that they were indeed friends, paid Billí the requested \$6 for his risks and worries in taking her to the sacred place. The isolation from the town and community allowed Billí and Parsons to meet at a specific exchange of value: six American dollars for prayerstick comparison, some explanations and drawings of petroglyphs, and a bond between them—participants in a clandestine information exchange, sharers of a secret.

In the unpublished manuscript from which the story about Billí comes, Parsons persistently described Pueblos as anxious and superstitious about sharing secrets, such that "fear" of cosmic misfortune or from their own community members dictated much of their lives. While this was certainly overblown—Parsons penned the manuscript in a decidedly popular mode that suggests a liberal, interpretive recollection—the fact that Parsons framed Pueblo life in this manner suggests that the trope of fear colored her view of Pueblos from an early point in her career. In order to mitigate this fear, Parsons had to coax Pueblos into situations in which they could feel relief and comfort.

Billí of Zuni was given a pseudonym and anonymity in Parsons's manuscript, and it is not unreasonable to surmise that Pueblo informants commonly insisted that their identity be obscured in future publication, as part of the negotiation of information exchange. Moreover, anthropologists were obliged to protect their informants, lest a document return to the community and reveal the informant who had given sensitive information to an outsider.

This chapter looks at the work of Elsie Clews Parsons, an ethnographer who worked in the Pueblo ecumene from the early 1910s into the late 1930s. By the beginning of the 1920s, Parsons had not only established her own scholarly importance in the Southwesternist scene, but also a financial influence through her own considerable personal wealth. By looking at her fieldwork among several communities (examining specifically the settlements of Acoma and Zuni, in addition to Taos and Isleta), I illustrate her incorporation of off-site ethnographic interviews to

further a specific “ceremonial” agenda that dominated Southwesternist study in the early 20th century.²

Clandestine information exchange and off-site interviews with informants—what I also call “hotel-room ethnography”—were fostered by several factors. For one, anthropological fieldwork in New Mexico and Arizona trended toward the study of inner life, including “myths” (folklore), “traditional” education (sophiology), and “religion” or spiritualism (ceremonialism). These research foci were promoted not only by the Bureau of American Ethnology and the emerging complex of academic anthropology, but also by Parsons’s own financing vehicle, the Southwest Society. Another facet propelling clandestine research was the effect of “intimate” ethnographic publications and their circulation back to Pueblo communities. Over time, many Puebloan communities got hold of the anthropological monographs of the previous generation; through these, they arrived at a deeper understanding of ethnographic documentation. Critically, these documents at times revealed who had shared Pueblo knowledge with outsiders. From these documents, Pueblos appeared to have interpreted Anglo actions in a variety of manners, but for the most part felt aggrieved by the publication of “secrets,” especially those related to Pueblo ceremonialism and spiritualism. The revelation of shared secrets within Pueblo towns caused internal tensions about the influence of outsiders to rise to the surface. In response, Pueblo communities such as Zuni, Taos, and Acoma, among others, turned to policing the boundaries between insider and outsider in more direct ways than they had in the late 19th century.

Regional changes, notably the steady incursion of Anglo settlers into the Southwest during and after the turn of the century, also played a role in altering the conditions of ethnographic information exchange. At the local scale of a Pueblo community, new economic opportunities emerged, brought by new rail and road networks, and linked to the rising fascination with the Southwest among Anglos. A tourist economy steadily developed in the Southwest in the late 19th century and continued into the 20th, which provided Pueblo artisans with new forms of income—but which came along with increased Anglo intrusion into their communities, especially surrounding sensitive matters of spiritualism, dances, and iconography. Equally if not more transformative, however, were the boarding and day schools that Pueblo children were encouraged, and at times compelled, to attend. Anglo-based school education directly opposed

² In the next chapter, I further explore Parsons’s use of auto-ethnographic production. This chapter focuses on off-site ethnography to the virtual exclusion of auto-ethnography.

many of the “traditional” teachings of Pueblo communities, and sought to “kill the Indian, save the man.” Schools separated children from their families in rural pueblos for long periods. In addition to schools, new labor opportunities siphoned off a number of community members to larger economic centers, which exposed Puebloan laborers to new cultural, linguistic, and economic influences.

These factors intermixed, I argue, to create conditions for Anglo anthropologists and Pueblo informants to negotiate new relationships of cross-cultural exchange. These relationships were grounded on mutual access to things each respective member found valuable. For anthropologists, ethnographic information about sensitive and obscured Pueblo practices, specifically the material of Pueblo ceremonialism and cosmology, was of considerable value as novel “contributions to knowledge.” For many Pueblo informants, relationships with Anglos offered a new source for material gain, as well as access to additional economic opportunities to sell goods or gain recommendations for jobs that were typically unavailable to Indians. For both, we may speculate, these relationships also offered meaningful cross-cultural connections and learning—but where, exactly, did friendship end and exchange relationships begin? Given the forces of Americanization encroaching on the Southwest, it must be recognized that these relationships were nearly always skewed in favor of Anglos, who had access to greater resources.

Anglos and Pueblos in these relationships were “secretsharers,” a term I use to denote their mutual obligation to confidentiality, which ensured each could get what each wanted. Pueblo informants often desired their connections with Anglos to remain a secret, for fear of reprisal from fellow community members. Anglos, too, began to hide their relationships with informants in order to keep their access to ethnographic information open and unrestricted. Southwesternist ethnography took on a clandestine or secretive direction as ethnographers sought to speak to informants alone, without observation from other community members. By analyzing instances of the secretsharer relationship, I study the “value” of ethnographic information, considering the perspectives of Anglo anthropologists and Pueblo informants, and explore the different logics at work in secretsharing. I look at the motivating factors that drove the secretseeking/secretsharing relationship, and I ask why Anglo ethnographers continued to pursue secret knowledge from Pueblo informants and why Pueblo informants were compelled to share secrets under certain conditions. While my claims may have a titillating air about them—I speak of secrets and

shadow deals and spy-work—I do not mean to imply that the relationship between anthropologists and Pueblo communities in this time was inherently corrupt, but rather that two countervailing logics entered into the “ethnographic interview” (as it was no longer an “encounter”), one of Pueblo informant’s motivations to improve their lot in life through private secretsharing, and another of anthropologists airing Puebloan secrets for their professional benefit.

Elsie Clews Parsons was a novice in cross-cultural ethnography when she began fieldwork at Zuni in 1915, but she had for nearly two decades been a feminist writer-activist and sociologist, and she understood the intellectual trends of academic anthropology. Parsons was part of elite Anglo America; Elsie’s life as a young woman in the Clews family “practically demanded that she live the life of a debutante,” as one historian has mused. But Elsie Clews bucked tradition to pursue scholarly interests. Gaining a doctorate in sociology from Columbia in 1899 under Frank Giddings, she turned her own rebellion into an inquiry of the hold of traditions over the life of an individual, and specifically women in American society.³

Immediately preceding her first major field trip, to Zuni in 1915, Parsons had published three books that investigated conventions and their constraining effects, especially on American women—*Religious Chastity* and *The Old Fashioned Woman* in 1913 and *Fear and Conventionality* in 1914. Parsons’s critical examination of American gender conventions made her an apt analyst of the history of anthropological thinking up to her time. For previous anthropologists, especially those focused on kinship studies, marriage was a contractual or “legal” entity within societies—that is, an agreement between two parties to join in a union and have their union publicly recognized as such. Parsons believed anthropologists had taken the idea of marriage as self-evident and natural and thus incorrectly saw alternative forms, such as polygyny, as debased or not yet evolved forms of union.

She herself had married, albeit unconventionally, Herbert Parsons while doing her doctoral work, and later moved with him to Washington when he was elected to congress as a reform Republican. The Parsons family returned to New York in 1910, and Elsie Clews Parsons drew

³ A selection of articles written by Parsons in the early 1900s indicate her focus on the constriction of women’s roles in American life: “Penalizing Marriage and Child Bearing” (1906); “Sex Morality and the Taboo of Direct Reference” (1906); “The Supernatural Policing of Women” (1912); “Feminism and Conventionality” (1914).

strength and influence from three intellectual and political circles: the anthropological community at Columbia around Franz Boas and his students, the Heterodoxy club of Greenwich Village feminists, and the left progressives of New York, including Walter Lippman, with whom she helped found *The New Republic*.⁴ Parsons was, clearly, a well-connected person, engaged with politics and social science in a public mode. She understood that her wealth and social standing allowed her unconventional attitude, but she sought to expand opportunities for women of all social classes.⁵

During the same time, however, Parsons was exploring a new research interest in the Southwestern US, among the dispersed communities of the Pueblo Indians of New Mexico and Arizona. Parsons began her research on Southwestern Indian groups in 1912, although it appears she did not live among the Pueblo communities of the Española Valley, where she received her initial introduction to the region and its inhabitants. Instead, her initial work on Pueblo topics was centered at the ranch of an Anglo woman, Clara True, in Española. True's ranch, in this instance, became the site of ethnographic data collection; Parsons arranged for Indians from the numerous pueblos around Española—Santa Clara, San Juan (Ohkay Owingeh), San Ildefonso, Nambe, Tesuque, Pojoaque, and beyond—to come to the ranch. Over coffee or whiskey in True's kitchen, Parsons found Puebloans were willing to talk about their cultural practices more freely because they were beyond the eyes of their community.⁶ While maintaining a home base fit for her social standing in her initial journey, Parsons did later spend time in nearby Pueblo communities. She facilitated her access to these towns through a Santa Clara man named Pedro Baca, who had originally shown Parsons some ruins near Española in 1910 when she was touring the Southwest. Baca occasionally worked on Parsons's behalf for introductions and interpretation in and around the northern New Mexico pueblos.

By 1915, Parsons appeared to have decided to focus the majority of her energy on ethnological activity in the Southwest, and the Pueblo Indians became her primary objects of scholarly pursuit.⁷ She initially focused on the communities surrounding Española. In subsequent years, Parsons moved her research southward, away from the Pueblo communities on the Rio

⁴ Lamphere, "Feminist Anthropology: The Legacy of Elsie Clews Parsons," 520.

⁵ See, for example, Parsons's long (though personally fraught) relationship with Clara True. True-Parsons Correspondence, Elsie Clews Parsons Papers, APS.

⁶ Parsons, "In the Southwest," APS.

⁷ Excellent biographies of Parsons have explored her motivation for switching to anthropological fieldwork in a remote field. See Hare, *A Woman's Quest for Science*; Zumwalt, *Wealth and Rebellion*; Deacon, *Elsie Clews Parsons: Inventing Modern Life*.

Grande, to some of the so-called western Pueblos of Laguna, Acoma, Zuni, and the Hopi villages. These communities were larger and more remote from the Southwest's major Anglo and Hispano population centers, two conditions which made them better choices for ethnographic study in the 1910s, because each hypothetically possessed a greater number of potential informants who were also more "authentic" because less influenced by Hispano and Anglo interaction.

Zuni as Training Ground

Zuni, in particular, was a hotbed of ethnographic activity, and the network of anthropologists working in the town and its outlying communities drew Parsons to western New Mexico in 1915. When she arrived in Zuni in 1915, Parsons surely kept in mind some advice from a friend, the anthropologist Alfred Kroeber, for improving her experience with informants in the town. Kroeber suggested that Parsons focus on the Zuni governor and, more specifically, his wife. "You could do no better, if you propose staying for more than a day, than to lay your plans before his wife, who is an educated Cherokee, and ask her advice how to carry them out."⁸ The governor's wife was Margaret Lewis, who had moved to Zuni to teach at the local school and was seen as an resourceful intermediary for extracting information from Zuni.

While Parsons also took notes on daily life at Zuni, she engaged Margaret Lewis to facilitate information gathering. As Lewis later recalled to anthropologist Loki Pandey, "I am not a Zuni and I don't know everything about the Zunis. So if there was something which Mrs. Parsons wanted to know about them and I didn't know, I asked the people and they told me everything. My husband was an important Zuni and he helped her a lot."⁹ Margaret Lewis, it appeared, was already "in" at Zuni, by virtue of her marriage and status as Cherokee (as opposed to Anglo or "Mexican"). Moreover, she was an excellent informant for Parsons and others because of her literacy, English-speaking abilities, "progressive" attitude toward education, and position within an influential family. Even if the governor William Lewis hesitated to speak on many matters, as was often the case, he appeared to visiting anthropologists to have less trouble sharing information with his wife, who could ask freely and openly and then relate that

⁸ Parsons, "Zuni Notebook [Journal #1]," Elsie Clews Parsons Papers, APS; Kroeber to Parsons, 16 August 1915, Elsie Clews Parsons Papers, APS.

⁹ Pandey, "Anthropologists in Zuni."

information to anthropologists. After the ethnographer left the community, Lewis kept up a diary for her friend Parsons, and the two carried on correspondence both casual and professional, an informant-anthropologist connection that Parsons would later develop further.¹⁰

The loopholes of ethnographic access in the Lewis household had been mined for at least a decade before 1915. Scholars such as Alfred Kroeber, Robert Lowie, Franz Boas, and Pliny Goddard had spent time living in Zuni, and all under the hospitable roof of Margaret and William Lewis. Of course, these anthropologists had enrolled other Zuni informants, but temporary residence at the Lewis household offered access to a prominent hub of Zuni activity. In this way, the Lewis residence was akin to the Pino residence of the 1880s and 1890s, when an anthropological clique (see Chapter 2) formed around the elder Pedro Pino and his son, an influential ceremonial leader and sometime governor, Palowahtiwa (Patricio Pino). Throughout a nearly 40-year period, the Lewises and the Pinos provided a stable medium for ethnography to develop itself at Zuni. Zuni had already been a hub for the first cohort of Southwesternists; though chiefly studied by Frank Cushing and Matilda Stevenson, Zuni people encountered the likes of Washington Matthews, John Gregory Bourke, Jesse Walter Fewkes, Herman Ten Kate, Charles Lummis, and other ethnographic-minded scholars.¹¹ By the early 1900s, Zuni had become a focal point for ethnographic study, a place supposedly “hospitable” to ethnographic work but also quickly becoming colonized by Anglo scientists.

Anthropological activity at Zuni from 1880 into the 1920s, then, tended to use the Pueblo’s governor and his immediate family as the point of access. In turn, the governor (or his wife, in the case of Lewis) modulated access to Zuni materials. The outward-facing diplomacy of the governor and his circle was not a coincidence; Zunis adopted the position consciously to address the presence of outsiders. The community had long maintained the practice of obscuring the leadership in the community, instead placing a “governor” as a visible symbol of power, recognizable by outsiders. As Parsons remarked, “So invisible is their government to the

¹⁰ Parsons, Papers re: Margaret Lewis, Elsie Clews Parsons Papers, APS. Parsons developed a similar relationship with Mabel Dodge Luhan, an Anglo who had married a Taos man, and asked her to obtain ethnographic data from the husband when possible. Such a contact was useful because minor questions could be answered quickly by a reliable correspondent. See “Discreet Questions,” Parsons to Mabel Dodge Luhan, n.d., Elsie Clews Parsons Papers, APS; Zumwalt, *Wealth and Rebellion*, 230.

¹¹ Pandey, “Anthropologists in Zuni.”

outlander that its very existence is unknown. The governor and his council, the officers, are the only government known to ‘Washington’ or to the resident Whites.”¹²

Parsons and other anthropologists, of course, had learned of the hidden role of the caciques, or spiritual leaders, through previous ethnographers. Anglo traders and Indian Agents, however, could be taken by the ruse. When Parsons asked the Anglo trader at Zuni about a feud among the spiritual leaders, he had no idea that the Zuni community had any leadership besides the “good for nothing” public officer, Governor Lewis. In fact, this Anglo trader viewed Zuni as consummately peaceful; even if this man lacked curiosity for the social and spiritual structure of Zuni, it is clear the community clearly obscured the priesthood from view to outsiders.

Zuni, in other words, had maintained a level of secrecy in their dealings with outsiders; they structured the terms of access to account for visiting anthropologists, tourists, and American officials alike. The pseudonymous Billí, underscored this restrictive structure by circumventing it, reminding Parsons of the value of the restricted or “secret” information he could provide her—what he called the “price of fear.” Although it is difficult to interpret given the one-sidedness of the information available, it is likely that Zuni affairs were hidden from view for reasons of privacy (as a family might hide its troubles from its neighbors), for protection against malicious knowledge seekers or “witches,” and to preserve sanctity and sacredness. It was not that information was categorically un-knowable, but rather that the stewarded and compartmentalized distribution of Pueblo knowledge (Chapter 3) caused it to appear to Anglos as purposefully hidden and highly secret.

The Southwest Society and the Ceremonialism Focus in Southwesternist Practice

While anthropologists knew that “governors” operated as the official mediator for outsiders, the public character of the office was not of great interest to visiting scholars. The office was important to understand, surely. It appeared to scholars to be a response to prior inquiries from the bureaucracy of the Spanish state and from Catholic missionization. For Parsons, the Zuni governor was a *public* official, who could not (and would not) provide information on the deeper structures that constituted Zuni life. Instead, she sought information that the governor appeared to obscure—private issues, things out of view.

¹² Parsons, “In the Southwest,” chapter 4, APS.

As Parsons shifted from sociology to anthropology, her mode of scholarship in turn changed from generalizing conditions across society toward empirical data accumulation that fit discrete categories. Her shift toward the study of unique cultural attributes was undoubtedly influenced by Franz Boas and his students. However, it is important to note that Parsons had no need to hew to the norms of academic anthropology because she possessed sufficient self-confidence and financial security to overcome any institutional misgivings.¹³ Parsons, notably, had not drawn from the coffers of academic institutions or progressive foundations in her pursuit of Southwesternist material. She self-funded her research trips, and her wealth enabled her autonomy and flexibility in her ethnography. In fact, shortly after dedicating herself to work on Pueblo and Southwestern anthropology, she established herself as a financial gatekeeper for workers in the region.

The Southwest Society, which she founded in November of 1918, facilitated Parsons's agenda to comprehensively document the non-public elements of Southwestern Indian life, including practices of ceremonialism and mythology among the Pueblo. (Study of other Southwestern groups was also funded.) As a patron of Southwestern anthropology, Parsons could direct its research aims without the need to appease intellectually powerful—but financially neutralized—anthropologists like Boas or to institutional objectives, such as those of the School of American Archaeology and its leader, Edgar Lee Hewett.¹⁴

Officially, the Southwest Society was an incorporated entity, but it was essentially a mechanism for Parsons to formalize her funding of scholars in the field. The Navajo scholar Pliny Goddard served as the first president, with Parsons as the Secretary-Treasurer. The society's coffers were filled with \$1 donations from preeminent anthropologists of the day as well as substantial donations from a single anonymous donor. Parsons, undoubtedly the donor, here endowed the society with her wealth and leveraged it with the prestige of single-dollar donors, such as Boas, Wissler, Lowie, Hodge, Tozzer, Kroeber, Sapir, and others.¹⁵ The Southwest Society sponsored research by Boas and Ester Schiff (1920-21), Leslie Spier and Erna Gunter at Havasupai (1921), Gladys Reichard and Pliny Goddard among the Navajo (1923,

¹³ See Deacon, *Elsie Clews Parsons: Inventing Modern Life*.

¹⁴ On Hewett, see Johnson, *American Archaeology and the Conceptualization of Preservation*.

¹⁵ Fowler, *A Laboratory for Anthropology*, 328. Parsons was generous with her wealth when it came to causes she cared for; years earlier during her engagement with US politics, she was a founding donor of The New Republic with Walter Lippmann. See Lamphere, "Feminist anthropology: the legacy of Elsie Clews Parsons," 520.

1930), Ruth Benedict and Ruth Bunzel at Zuni (1924), and Leslie White at Acoma (1926), as well as others.¹⁶ Notably, the Society funded female fieldworkers during a time that institutional funding overlooked young female fieldworkers.

As the nineteenth century ended and disciplinary anthropology picked up steam, the need to train anthropologists had increased the value of secret-seeking. A student could learn about a community before arriving and come with a specific goal in mind. In the field, budding anthropologists could learn from and critique former fieldworkers while gaining field experience in short bursts of study.¹⁷ The Southwest anthropological scene became, as Don Fowler and others have often noted, a “laboratory” and training ground for budding anthropologists, a designation enshrined by the founding of the School of American Archaeology in 1907 (changed to School of American Research in 1917 to reflect ethnographic engagement in addition to archaeology) and the Laboratory of Anthropology in Santa Fe in 1930.¹⁸ The Pueblo communities of Zuni, Hopi, Laguna, and Acoma became besieged with fieldworkers—the more secretive or conservative, the more likely that a cavalier fieldworker might arrive and start asking questions. Nascent fieldworkers, directed by more senior scholars and through funding apparatuses such as the Southwest Society, were to investigate a specific area within Puebloan life while building on the archive of anthropological knowledge. To do so, however, required new tactics for uncovering and documenting novel information, to make their own contribution to ethnological science. It appeared, then, that new, clandestine relationships with informants was a path to achieving the goals of a maturing American anthropology.

An indigenous culture’s ceremonial practices—often called esoteric, obscure, complicated, idiosyncratic—presented an appealing route to novel ethnographic accumulation. For ethnographers, ceremonialism tapped into a multitude of areas for ethnological analysis such as

¹⁶ The Southwest Society also funded ethnographers and folklorists working in Spain, Haiti and Santo Domingo, Florida and Alabama, and in the greater Caribbean and West Africa (as well as an outlier in Nova Scotia). See Elsie Clews Parsons Papers, APS.

¹⁷ A continuum of fieldworkers discussed strategies for accessing Pueblo cultural knowledge. See Pandey, “Anthropologists in Zuni” for the Zuni case; for insight into the tutoring the relationship between Leslie White (the student) and Elsie Clews Parsons (the teacher), who were arguably the most deliberate secretseekers in the early twentieth century Southwest, see White-Parsons Correspondence, ELSIE CLEWS PARSONS PAPERS, APS.

¹⁸ For the School of American Research, see Johnson, *American Archaeology and the Conceptualization of Preservation*; Nancy Owen Lewis and Kay Leigh Hagan, *A Peculiar Alchemy: A Centennial History of SAR, 1907-2007* (School for Advanced Research, 2007). For the Laboratory of Anthropology, see George W. Stocking, “The Santa Fe style in American anthropology: Regional interest, academic initiative, and philanthropic policy in the first two decades of the Laboratory of Anthropology, Inc.” *Journal of the History of the Behavioral Sciences* 18, no. 1 (1982): 3-19.

religion, social governance, kinship, and folklore. Ceremonial practices, moreover, often marked out time for communities in regular intervals, and thus allowed ethnographers to understand the patterns of economic and social practice that ebbed and flowed in Pueblo communities.

Parsons and others in the Southwest Society's orbit were also interested in the degree of outside influence exerted on the Pueblo and other Southwestern Indian communities, what came to be called acculturation. In her own work, Parsons studied Southwestern social and spiritual organizations through linguistic and ethnographic observation in order to analyze the extent to which various communities and ceremonial forms had been influenced by outside forces. One point of analysis concerned the historical connections of European, specifically hispanophone, colonialism, as the duration of Spanish-Indian cross-cultural exchange had been lasting and clear Catholic and Spanish influences could be seen Pueblo communities. In a related vein, Anglo influence was also of interest, although this appeared simpler to subtract or account for in cultural analysis, especially for Anglo ethnographers steeped in their own culture. Cross-indigenous influence, as between Navajo and Pueblo groups, was also of interest. To articulate this constellation of cultural exchanges relationships would provide ethnology with fine-grained, autochthonous culture data, as well as provide information for an analysis of different forms of intellectual and material transfer. Through the Southwest Society, Parsons and allied scholars presented a series of goals for Southwesternist anthropology.¹⁹

The anthropological subfield of folklore studies was particularly in vogue at the turn of the century, and its popularity continued well into the 20th century. Parsons's collaborators Pliny Goddard and Franz Boas were committed to gathering material for the American Folk-Lore Society and its publishing outlet. "Folklore" was a broadly encompassing name, but the work conceptualized and published under the sign of folklore was intimately connected with spiritual or ceremonial practices, especially with regard to studies of the non-West and of the Southwestern United States specifically.²⁰

Parsons admired and supported scholarship on folklore. From 1916 to 1940, Parsons donated \$42,000 to the American Folklore Society to aid its publication efforts; a similar sum is presumed to have gone to ethnographic publications as well.²¹ But an analysis of Parsons's work

¹⁹ Zumwalt, *Wealth and Rebellion*, 210; Fowler, *A Laboratory for Anthropology*, 328.

²⁰ Regina Bendix, *In search of authenticity: The formation of folklore studies* (Univ. of Wisconsin Press, 2009); Rosemary Lévy Zumwalt, *American Folklore Scholarship: A Dialogue of Dissent* (Indiana University Press, 1988).

²¹ Fowler, *A Laboratory for Anthropology*, 328.

and patronage indicates that “folklore” was not a broad enough category to describe her ethnographic aims in the Southwest. Ceremonialism, instead, better captures Parsons’s interest in Pueblo societies, and more generally in Indian “religion.” Parsons’s articles from 1915, when she began to work on Southwestern topics, to 1939, when she published her major two-volume work, *Pueblo Indian Religion*, were scattered with numerous references to ceremonials and ceremonialism. While the term “folklore” appears in her overall bibliography, Parsons conceptualized folklore as an ideational component of ceremonialism, the theory to ceremonialism’s practice.²²

Ceremonialism, of course, was an area of sensitivity among Pueblo communities, as I have shown in the previous chapter. Indeed, anthropological books of their “religion” or ceremonial materials were the chief documents about which Pueblo peoples were concerned. Pueblo communities held ceremonialism as sacred in its constituting, epistemological function. These processes evoked stewarded knowledge that worked toward various ends—for rain, for a good growing season, for health and healing, for easy birthing, for boys and girls in maturational rites of passage. Due to their importance for the maintenance and perpetuation of Pueblo lifeways, ceremonial processes were often private events, for Pueblo eyes alone.

Given the private, sensitive nature of ceremonialism, the inquisitiveness of ethnographic fieldworkers in the Southwest presented a complicated dynamic. After the initial incursion of anthropological inquiry in the 1880s and 1890s, Pueblo communities appeared to understand the goals of ethnography as basically exhaustive: anthropologists would eventually ask about nearly every nook and cranny of their community life, however obscure. In tension with the anthropologist’s goal, Pueblo communities sought to maintain some elements as their own, reserved for their own people exclusively. By 1920, for instance, when Boas and Parsons joined forces at San Felipe Pueblo, they found the people of the town utterly unwilling to speak with them. Similar resistance came from Santa Ana and Santo Domingo, and the anthropologists

²² Note that the AFL, with “folklore” in its very name, published the first bibliography upon Parsons’s passing. Parsons saw Pueblo ceremonialism as a social force that structured and regulated Pueblo life. In this way, her scholarship and patronage can be seen as concerned, in addition to the minutia of religious life, with the life history of an individual in Pueblo communities (this is dealt with in the next chapter). This fit with her former scholarship on women and the family in American society. See Lamphere, “Feminist anthropology: the legacy of Elsie Clews Parsons; Catherine Jane Lavender, *Scientists and Storytellers: Feminist Anthropologists and the Construction of the American Southwest* (University of New Mexico Press, 2006); Deacon, *Elsie Clews Parsons: Inventing Modern Life*; Zumwalt, *Wealth and Rebellion*.

gained a sense that these pueblos in the northern part of the ecumene were “closed” to anthropologists.²³ Understandably, it seems that Pueblos were frustrated that these private or reserved elements became the targets for accumulation within the goals of an exhaustive ethnography.

A Changing Economic and Demographic Landscape

Pueblo resistance to documentation and a “conservative” turn toward privacy were, of course, due to more than just the presence of anthropologists in their communities. The years 1880-1920 were a time of rapid Anglo-American settlement in the region. Anglo settlers brought cultural, political, economic, and even geographic change as they built homes and businesses, roads and rails, and began the incorporation of New Mexico and Arizona territories into the fabric of the United States. The region was linked to the East and West with the railroad in the 1888 and later via roadways, culminating in 1926 with Route 66 highway. Both infrastructural developments brought economic development to communities through which they passed. The major towns near Pueblo settlements in 1900, Santa Fe and Albuquerque, grew in population and drew labor from nearby Indian communities for emerging timber, cattle, and sheep industries.²⁴ Meanwhile, conflicts over water rights and land use emerged as high-desert resources were stretched by increased Anglo settlement.²⁵

The railroad had also brought new supplies to previously remote communities. New household goods and farm tools were hawked by traders who set up trading posts near communities such as Laguna, Acoma, and Zuni. While Anglo and Hispano traders had long been present in these communities, the availability of goods increased markedly. An increased goods market redrew some alliances and sentiments in Pueblo communities. In some cases, foreign traders married into the communities they served. This included the remote community of Acoma; Solomon Bibo, an Anglophone Jewish trader from the East, married the Acoma governor’s daughter in the 1880s and exerted financial advantage to control land and trade

²³ Fowler, *A Laboratory for Anthropology*, 334.

²⁴ Edward Holland Spicer, *Cycles of Conquest: The Impact of Spain, Mexico, and the United States on the Indians of the Southwest, 1533-1960* (University of Arizona Press, 1962); Chris Wilson, *The Myth of Santa Fe: Creating a modern regional tradition* (Univ. of New Mexico Press, 1997); Dilworth, *Imagining Indians in the Southwest*. Though not in focus here, it is important to note that Hispano residents of New Mexico, Texas, and Arizona were similarly affected by Anglo settler colonialism. See Pablo Mitchell, *Coyote Nation Sexuality, Race, and Conquest in Modernizing New Mexico, 1880-1920*. (University of Chicago Press, 2005); Mora, *Border Dilemmas*; Ortiz, *Roots of Resistance*.

²⁵ Pandey, “Anthropologists in Zuni”; Ebright, *Advocates for the Oppressed*; Kosek, *Understories*.

opportunities in these towns. For a time, the non-Indian Solomon Bibo even served as governor of Acoma, before eventually falling out with the community and relocating his family to Santa Fe.²⁶

Anglo influence was particularly felt through education, including forced education through the relocation of children from pueblos to distant schools. For many Pueblos, Anglo education was a fraught political terrain, and certainly not universally supported within Indian communities. Although only select material evidence remains, it appears likely that every Pueblo community found itself debating the merits and pitfalls of schooling, weighing the benefits of increased earning capacity amidst the new markets seeping into the Southwest against the negatives of diminished attention to ceremonial concerns and the responsibilities of every Pueblo man and woman to their kin and clan.²⁷

Increased intercultural exchange with Anglos, especially through formal Anglo-style education, created new opportunities for anthropological data collection through culturally heterogeneous, literate informants. Anglo-educated Pueblo Indians simultaneously represented an intriguing prospect for novel data collection as well as a (potentially sympathetic) ally in the face of increased resistance to ethnographic inquiry. Since attempts at Indian schooling began in the late nineteenth century anthropologists had relished the opportunity to enroll a Pueblo informant who demonstrated at least one of the hallmarks of Anglo education—English fluency and the ability to read and write—and was willing to help document their lives and the ceremonial events in their communities.²⁸ Literacy was seen by turn-of-the-century Anglo reformers as a route to “civilizing” Indian communities, as a way of dispelling Indian “opinions” (sophiology’s area of study) and replacing them with a reasonable, “modern” perspective. In the

²⁶ Peter Nabokov, *How the World Moves: The Odyssey of an American Indian Family* (Viking Press, 2015). The Anglo family of the Marmons also married into Laguna Pueblo, with effects that were recast by a Laguna-Marmon descendent, Leslie Marmon Silko in 1977. Again, intermarriage did not only occur between Anglos and Indians. For a long history of Hispano-Indian intermarriage in the Southwest, see Gutiérrez, *When Jesus Came*. See also Brooks, *Captives & Cousins*.

²⁷ Adrea Lawrence, *Lessons from an Indian Day School: Negotiating colonization in northern New Mexico, 1902-1907* (University Press of Kansas, 2011). For education, a valuable case study (Hopi) can be found in Matthew Sakiestewa Gilbert, *Education beyond the Mesas: Hopi students at Sherman Institute, 1902-1929* (Univ. of Nebraska Press, 2010). Education was also at issue during the “Orayvi split,” as documented by Whiteley, *Orayvi Split*.

²⁸ There is an extensive and sophisticated subfield on the history of Native American education and assimilation. For Southwestern communities and schools for indigenous education, see Lawrence, *Lessons from an Indian Day School*; , Jacqueline Fear-Segal, *White Man’s Club: Schools, Race, and the Struggle of Indian Acculturation* (University of Nebraska Press, 2007); Sally Hyer, *One House, One Voice, One Heart: Native American Education at the Santa Fe Indian School* (Museum of New Mexico Press, 1990); Diana Meyers Bahr, *The Students of Sherman Indian School: Education and Native Identity Since 1892* (University of Oklahoma Press, 2014); Myriam Vučković, *Voices from Haskell: Indian students between two worlds, 1884-1928* (University Press of Kansas, 2008).

Southwestern context, this desire was explicitly announced in texts such as *Stiya: A Carlisle Indian Girl at Home*, written by Marianna Burgess (under the pseudonym Embe) in 1891.²⁹ In this romanticized, biographical story, a Pueblo girl, the eponymous Stiya, returned to her Pueblo community from Carlisle Indian School with the goal of introducing Anglo literature, and by extension American “civilization,” to her brethren. Such works traded on the notion that literacy would enable the assimilation of Indian groups into the larger American fold.

Literacy, however, was not simply a one-way street to assimilation and amenable ethnographic informants. Anglo-educated Pueblos came to understand Anglo literary technologies and their potential for reproduction and circulation, especially as forms of reproduction that diminished knowledge’s power as it circulated in printed form. Recall Palowahtiwa’s suggestion that Anglo writing systems relied on a “gift of guessing”; Anglo media decontextualized information and could present both true and untrue things to the reader, who had to “guess” at validity. Palowahtiwa here contrasted decontextualized written data with the Pueblo notion of knowledge stewardship, which insisted that important knowledge be carefully managed by fraternal societies or other social cliques and passed on with deliberate care. An education in letters surely gave select Pueblos a sense of the reasons that Anglos valued literacy and its material articulations in books, notebooks, and other media. It seems reasonable, even likely, that literate Pueblos recognized the archival and scientific motivations of data accumulation presented by anthropologists as a form of collecting information for reference and distribution, a cycle of replication that multiplied the number of people that could potentially know a form of knowledge. At times, literate Pueblos decided to help anthropologists to this end, in recognition of the preservative quality of history and ethnographic literature. But, at other moments, the characteristics of Anglo literary technology’s perceived violation of the situated, stewarded troves of knowledge that interwove Pueblo communities could stop anthropological inquiry in its tracks. Anglo education, in short, could highlight the potential benefits as well as the perils of documentation for communities that adhered to compartmentalized knowledge systems.³⁰

²⁹ Burgess 1891. See also Dean 2011.

³⁰ See Lawrence, *Lessons from an Indian Day School*, for an extended discussion of Pueblo “education” and the limits typically placed on studies of learning and schooling.

As some Pueblo people were brought into the emerging Anglo sphere for economic and educational reasons, Anglos also penetrated the Pueblo ecumene in increasing numbers. New Anglo towns such as Gallup, Grants, and Flagstaff had popped up along the railroad corridor heading west from Albuquerque in the 1880s and 90s. In addition to serving as nodes for industry and railroad, these towns also provided lodging to tourists from elsewhere in the United States, who could now travel to New Mexico and Arizona and visit the famous Pueblo Indians. Tourists swarmed to Pueblo Dances—especially “exotic” performances such as the storied Hopi “Snake Dance.” In conjunction with dance tourism, a new market emerged for Indian crafts, and Pueblo pottery in particular became a hot commodity.³¹ Anglo artists and writers sought inspiration in Santa Fe and Taos and other areas in New Mexico, lending the region the nickname the “Land of Enchantment.” Some sought to capitalize on the cultural interest New Mexico captured, and boosters in Santa Fe recast the capital as a tri-cultural city, a “city different.”³² As historical and literary scholars have often noted, the Southwest after the turn of the century became imbued with a regional aura that conjured romanticized cultural and historical practices of Indians and Hispanos. These Anglo appropriations of Southwestern peoples’ cultural icons aligned, to some extent, with the novelty that anthropologists sought from Puebloan communities during the same time. The “secrets” and esotericism of the Snake Dance were alluring, in other words, for tourists, merchants, and anthropologists alike.

Politeness, Dissimulation, and Hospitality at Acoma

When Elsie Clews Parsons arrived in Acoma in 1917, a white woman atop the mesa asking questions of the people, she was hardly a novel site to Acomans. The town was experiencing the trouble of popularity; after the railroad and burgeoning of nearby towns, tourists regularly arrived at the foot of the mesa expecting to be shown the way up. The people of the town had experienced competing external forces, tugging in various ways at the fabric of their former lives. By the time Parsons made her own appearance, Acoma had begun to charge a photography

³¹ Erika Marie Bsumek, *Indian-Made: Navajo Culture in the Marketplace, 1868-1940* (University Press of Kansas, 2008); Dilworth, *Imagining Indians in the Southwest*; Margaret D. Jacobs, *Engendered Encounters: Feminism and Pueblo Cultures, 1879-1934*. (University of Nebraska Press, 1999).

³² Wilson, *The Myth of Santa Fe*; Marta Weigle, “From Desert to Disney World: The Santa Fe Railway and the Fred Harvey Company Display the Indian Southwest,” *Journal of Anthropological Research* 45, no. 1 (1989): 115-137; Hal Rothman, *Devil’s Bargains: Tourism in the Twentieth-Century American West* (University Press of Kansas, 1998). Early Southwestern boosterism notably came from Charles Lummis, himself an ethnographer of sorts, and his publication *Land of Sunshine* (called *Out West* after 1902).

fee for visitors—\$1 for the town and \$5 for Acoma’s famous San Esteban Del Rey Mission Church, built by Spanish friars with Acoma labor in the 17th century.³³ In addition to its history, tourists were charmed by the romantic setting of Acoma, a town situated 400 feet above the valley floor atop a mesa, a perch captured by its touristic name, “Sky City.” Though perceived to be a tourist, Parsons was determined to get some information for her developing project on Pueblo ceremonialism.

At Acoma, Parsons arrived with presents for her hosts and a Zuni person of some standing who would vouch for her. She still encountered resistance to her inquiries, though she believed that, had she been able to communicate her intention to benignly study Acoma culture, they might have given her easier access. “Although I was most hospitably entertained by the Acoma household who took us in and who kept me on after my Zuni friends left, I was unable to overcome the distrust altogether and much of my time was squandered in merely trying to differentiate myself from the picture-taking tourist or from the Washington representative from whom every ceremonial or intimate detail of life is to be hidden.”³⁴ Parsons had not yet realized that while Acoma may not have a nuanced understanding of the difference between camera-wielding tourists, Washington politicians, and anthropologists, they recognized that information flowed between these parties; information given to one, in other words, could find its way to another. Anglos, after all, were known as documenters, people who snapped photos or wrote down information so that another could later draw on it, outside of the context of its creation or the bounds of its stewardship.

The visiting anthropologist had intended to see a dance that her Zuni contacts had told her about, and during her stay Parsons revealed her explicit objective to see the ceremonial. She was later told, however, that the ceremonial calendar had been altered to account for her departure, and the ceremony would commence once she had left the mesa. So, with Acoma’s reputation as a secretive and deceptive place, unyielding to outsiders, Parsons adopted a strategy to simply remain atop the town’s mesa and rely on Pueblo politeness, slowly working her connections with friendly families of the community. Speaking of her time at Acoma Parsons explained, “before a

³³ Major scholarship on the history and ethnography of Acoma begin with Parsons and her acolyte Leslie White. For reference, those are Elsie Clew Parsons, *Pueblo Indian Religion* (2 vol.)(University of Chicago Press, 1939); Leslie White, “The Acoma Indians,” *47th Annual Report of the Bureau of American Ethnology* (1932). Both are, however, sensitive items to the Acoma people today; both Parsons and White are controversial figures who are not well remembered. See also Ward Alan Minge and Simon J. Ortiz, *Ácoma: Pueblo in the Sky*. (University of New Mexico Press, 2002).

³⁴ Elsie Clew Parsons, “Notes on Acoma and Laguna,” *American Anthropologist* 20, no. 2 (1918): 162-186.

dance Acoma stations a guard, no doubt a war captain, on the trail below to preclude ascent by Whites. But here I was, already on top, a guest of the aunts of one of the war captains, and of a kinsman of the cacique.” For Parsons, this was “an opportunity not to be missed,” for she was lucky to have been atop the mesa before the proceedings were to begin and associated with important members of the pueblo that could assent to her viewing.

Parsons had identified two men familiar with Acoma’s ceremonial events but who were ceremonially agnostic or, as she put it, “commercialized,” enough to be willing to provide the visiting anthropologist with information. One informant, Edward Hunt, ran the store at Acoma. In the interest of her continued patronage,

He did not want to offend me and he would engage in talk, yet he had no scruples against giving me any answers that came into his head as a means to evasion, and he would stay in the store in self-protection, for he knew I would not question with customers about or people passing by. Only once had he to warn me, my back was to the door. “Hide your paper,” he said, “I see the governor come.”³⁵

Hunt, Parsons wrote, was supposed to have had a Mexican father, or else was “Mexican” enough to “to be indifferent in a measure to the ceremonial life.”³⁶ But as shown above, Parsons also felt she could not fully trust his information, as she understood his economic interest in answering her questions and had experienced Puebloan dissimulation before.³⁷ Still, Hunt demonstrated to Parsons the potential fruitfulness of one-on-one conversations, away from observation by others.

Her other informant was Frank Johnson, who acted as her host in town. From the Johnson household, Parsons attempted to convey that she was not a tourist but a scholar of Pueblo peoples. This did not sway Hunt nor Johnson much; they seemed to merely tolerate Parsons—a sentiment we glean from her notes about their comportment. Johnson, for instance, stood with Parsons during the ceremonial dance, answering some of Parsons’s questions in a “cautious voice.” Johnson also sought to pass her off to a woman watching from a viewpoint further from the performance area. “One time, as much to put an end to the questions as to make me comfortable, he conducted me to the rooks which served as seats for the officers, and where I had sat a while the day before with the sister of the teniente until she was summoned by her family

³⁵ Parsons, “In the Southwest,” APS.

³⁶ Parsons, “In the Southwest,” APS.

³⁷ Unfortunately, there is not sufficient space for a discussion of Edward Hunt and his remarkable life. But see Nabokov, *How the World Moves*.

and I knew from her embarrassed excuses that they did not want her to be seen with me in public.”³⁸

Yet more anxiety could be seen in her host Frank Johnson as the dance was nearing its conclusion. Johnson told Parsons that her car was waiting at the foot of the mesa, ready to take her to Laguna for the evening. But Parsons was intent on staying until the end. Johnson returned once more with a message from the driver, saying Parsons was to hurry. Still, she watched on. Johnson came a third time, “saying that the driver sent word I must come down, he could not wait any longer.” She finally relented and made her way to the car.

There was a fresh burst of song as we started down the trail... On the way to Laguna I asked my driver why he had sent those urgent messages. He had sent no messages, he said, he'd have waited there all night, if I'd wanted. It made no difference to him.³⁹

According to Parsons, Johnson had fabricated the driver's anxiety, presumably under pressure from Acoma leaders. And this anxiety was directly linked to her appearance at the dance and his responsibility to deal with his guest amidst concerns from other members of the community.

Most remarkable were the conditions under which Johnson had hosted Parsons, in contrast to his profound discomfort with her presence at the dance. When Parsons had arrived to stay in the Johnson household, a young girl in the family was very sick, and she passed away while Parsons was staying in their home. Parsons was struck that their hospitality persisted almost unchanged—they mourned the loss of a young child in her presence yet kept up their efforts to make her stay as easy as possible. Until, that is, the day of the dance, when Johnson felt compelled to hurry Parsons off the mesa. Here, Parsons witnessed that Acoma hospitality dictated that she be provided for and maintained in her stay at Acoma, even in the midst of a family tragedy.

Pueblo hospitality perhaps reflected a unique “politeness” and reserved character (anthropologists have often described Pueblos as reticent but peace-loving) but did not differ from the general contours of hospitality relationships from diverse cultures around the world. Pueblo hosts provided hospitality to structure the reception and presentation of the stranger within the community. As demonstrated in Parsons's experience at Acoma, hospitality structured

³⁸ Parsons, “In the Southwest.”

³⁹ Parsons, “In the Southwest.”

her “encounter,” and indicated to Acomans the limits of her privileges in the town. A young girl’s death, and the continued hospitality offered to Parsons in the midst of this tragic event, surely confused Parsons but also alerted her to strong group loyalty in Pueblo public culture, as the Johnson household was not merely taking care of Parsons but protecting the entire community from unrestrained ethnographic inquiry.⁴⁰

Clandestine Meetings and Hotel Room Hospitality

By 1926, Parsons’s experiences during fieldwork in the Pueblo ecumene had led her to conclude that ethnographic research about Pueblo communities was often best advanced *away* from the Pueblo itself, through individuals isolated from their milieu. As Parsons wrote to ethnographer Leslie White, who was new to the Southwestern field in the mid 1920s, “‘Making contacts’ is not a help but a hindrance among the Pueblos. Most of us have got our material for any town [pueblo] from a single informant, and away from his town. They watch and terrorize one another.”⁴¹ She viewed Pueblo community life as dictated by constant surveillance, which paradoxically meant that, in contrast to the experiences of anthropologists twenty years previous (Chapter 3), knowing more people in a pueblo would actually yield less information.

The traditional layout of most pueblos—buildings centered around an open courtyard—ensured that anthropologists and other outsiders were immediately known to the community. The isolating and spotlighting function of the architecture of pueblos was especially apparent at Acoma, a pueblo that sat atop a mesa, 400 feet from the valley floor. To get to Acoma, everyone took a single path up the cliff-walls to the top of the mesa. In the 1920s, outsiders visited Acoma regularly, but they paid for the privilege and were not welcome to stay long. Even today, to visit Acoma requires a guided tour of a set duration.

These hurdles had made working with Acoma informants and developing an ethnographic profile of the community quite difficult for anthropologists like Parsons. “We have always found it exceedingly difficult to get Acoma informants. If I were trying it again I would go to Grants on the RR & try to get an informant to come there away from observation. There are certain [p]ueblos where it is impossible to work *in* the town.”⁴² Her solution was to lure Pueblo

⁴⁰ On hospitality, see Boudou, “Éléments pour une anthropologie politique de l’hospitalité”; Meijer-van Wijk, “Levinas, hospitality and the feminine other”; Lynch et al., “Theorizing hospitality”; Candea & Da Col, “The return to hospitality.”

⁴¹ Parsons to White, 27 May 1926, LWP.

⁴² Parsons to White, 17 May 1926, LWP. Parsons had noted in 1920, moreover, that an Acoma man had been friendly with her

informants away from town. Elsewhere, they could be paid and provided for outside of the context of their community, away from surveillance and gossip, which tended to close them off to giving nuanced, detailed information.

While staying at a residence outside of Laguna Pueblo, Parsons weighed the ethnographic positives and negatives of detached contact with the community:

Observation of the general life of the pueblo was necessarily limited and my circle of acquaintances comparatively restricted. On the other hand, interrogation was unhandicapped by embarrassing visitors and the disposition of my informants were rendered comparatively frank and responsive.⁴³

Documentation via participant observation could not be conducted, but a new intimacy with informants could be fostered. Moving away from the pueblo observations of John Gregory Bourke, Frank Hamilton Cushing, and Matilda Coxe Stevenson of the late 19th century, ethnographic emphasis had here shifted, sensorially speaking, from looking to listening, from participating to hosting.

In another instance, Parsons had made a brief visit to Isleta and established contact with an informant there. But it was when she brought this contact to Albuquerque, “in a hotel room, safe from observation,” that she gained better and more substantial information. In a later article based on this Isleta informant’s information, Parsons reminded her scholarly audience that Pueblo secrecy was designated for cultural elements she deemed exclusively and autochthonously Pueblo—her female informant “resisted all endeavors to learn from her not only the words of ceremonial import but clan names and the [indigenous] name for the town.” It was only Pueblo-specific information that her informant withheld: “on all things Mexican or Catholic she was communicative and glad to be helpful.”⁴⁴

Again, writing itself was not the only issue, but rather the practice intensified concern regarding the sharing of secrets with outsiders. Parsons related an encounter with a leader of Isleta who had been schooled in Santa Fe and had produced a book of his own about his people. When asked if it was available, he dissimulated, conveying the book was a keepsake for his

but, “as I had expected, he was more communicative away from Acoma than he had been in Acoma.” Elsie Clews Parsons, “Notes on Isleta, Santa Ana, and Acoma,” *American Anthropologist* 22, no. 1 (1920): 56-69.

⁴³ Elsie Clews Parsons, “Notes on Ceremonialism at Laguna,” *Anthropological Papers of the American Museum of Natural History* XIX, part IV (1920).

⁴⁴ Elsie Clews Parsons, “Further notes on Isleta,” *American Anthropologist* 23, no. 2 (1921): 149-169.

people after he passed away. As Parsons put it, “He would have no dealings with a stray scientist—he was afraid to, said a neighbor, citing his fears as justification for her own” silence on ceremonial matters.⁴⁵

Off-site and hotel-room meetings could provide a private space for conversation, as well provide informants access to new goods and services that were not on offer in the pueblos. For a time, Parsons also used a ranch in Alcalde, New Mexico, near the town of Española, to host Pueblos from the northern ecumene. Just like Clara True’s property in Española, which Parsons had made use of in 1915, the Alcalde ranch was a “quiet” place managed by Anglos, removed from the eyes and ears of Pueblo communities.⁴⁶ She used this ranch on several occasions for interviews, and also made it available to ethnographers of the Pueblo, such as Leslie White.⁴⁷

By hosting informants in an environment that she managed and putting money in their pockets and food in their bellies, Parsons enacted an inverted hospitality relationship that promoted information sharing. If Pueblo hospitality had structured Parsons’s experience at Acoma, as described above, Anglos offered up hospitality to Pueblo informants and structured their experiences in off-site ranches and hotel rooms. Obligations that the anthropologist had as a visitor in a pueblo—to publicly conform to the norms of the host—were flipped in off-site interviews. Pueblos, now, could be more easily compelled to fall in line with the desires of their hosts—which in these cases involved sharing sensitive information. Even if initially reticent, informant “guests” could be coerced into by particularly generous “hosts,” who could entice them with money or groceries.

Anglo anthropologists playing “host” probably conceptualized their treatment of Pueblo informants as hospitable, but their coercion could also be understood (cynically) as bribery or (forgivingly) as a mutual exchange relationship. As Parsons’s friend Ralph Beals recalled, Parsons secured ethnographic data in many cases “by getting Indians into hotels in Santa Fe or preferably Albuquerque, and bribing them into giving her information under the pledge of secrecy.” Parsons presented incentives and a promise of protection from harm from their community. While money could clearly be major motivating factor for secretsharing, other quality-of-life improvements—a day in the booming Anglo town, a new dress or a hat, novel

⁴⁵ Parsons, “Futher notes on Isleta.”

⁴⁶ White to Parsons, 21 October 1930, ECP Papers, APS

⁴⁷ Zumwalt, *Wealth and Rebellion*, 243-44.

goods from American grocers—also came into play. The key, it seemed, was to negotiate an exchange between value systems: Anglo-American goods and services in exchange for information about the most obscure and private aspects of Pueblo knowledge and belief.

Off-site ethnography was not an innovation by Parsons alone, but one that was perhaps conditioned by another constriction of modern life—that she was a woman in world where the female sphere was limited. Another notable practitioner of off-site ethnographic documentation was Matilda Coxe Stevenson, who after her husband passed away spent much time *near* the field, at a ranch and orchard she purchased near Española, New Mexico. Stevenson imagined her ranch as “an ideal place to work for I can always avoid interruptions when studying or I can hide my interpreter from view.”⁴⁸ She hired in secret and, in the privacy of her ranch house, put Indian visitors at sufficient ease to coax information from them.⁴⁹

Within the privacy of ranch kitchens and hotel rooms, anthropologists also had the opportunity to provide taboo items, notably alcohol, to individuals who were wary to drink in public in the pueblo. While Beals insisted that Parsons did not use alcohol to coax Pueblo informants into talking—an accusation that he leveled at Parsons’s predecessor, Matilda Coxe Stevenson⁵⁰—Parsons likely offered alcohol on occasion, even if only as an amenable host. Stevenson’s approach, the records show, was more direct. Stevenson ordered cases of scotch and put them in her expense accounts for the Smithsonian Institution (which denied payment). While some scholars speculate that the widow ethnographer wanted the scotch for her own use given her developing alcoholism—a diagnosis leveled by some of her contemporaneous male colleagues who clearly had a low estimation of her character—Stevenson herself claimed that her liquor orders were “necessary in her work, since nothing else would induce the Indians to give out their more secret information.”⁵¹

Regardless of the presence of alcohol, off-site interviews offered privacy to informants and an opportunity for clandestine cross-cultural exchange, which would be written down in a permanent and transmissible form. For anthropologists, the privacy of the meeting was necessary because Pueblo community “secrets” were valuable to them, and they hoped that private elements could be made ethnographically “public,” in support of their career and their

⁴⁸ Matilda Coxe Stevenson to Frederick Webb Hodge, NAA

⁴⁹ Parezo, *Hidden Scholars*, 51

⁵⁰ Peter Hare Papers, APS

⁵¹ Parezo, *Hidden Scholars*.

contributions to the discipline. And, it follows, in these exchange relationships the private information shared by informants became public, literally and legally, in the Anglo American system of exchange. In exchange for individual informant anonymity and payment, community secrets were revealed. Given that sensitive information became publically available through ethnographic monographs, then, Parsons and her Southwesternists colleagues knew it was essential to keep monographs out of the hands of the communities portrayed.

This complicated switching system in the terms of knowledge-access can be simplified in a statement: Individual informant privacy trumped community privacy for anthropologists. Because of the risk of infamy and an informant's "fear" of hurting their social standing (or their health), informant identities were kept secret from their communities. Puebloan secretsharers were themselves secrets; they had passed their secrets on to ethnographers, who through the documentation and publication of sensitive information became the public secretsharers of Puebloan life.

The value of secrets—the price of Pueblo fear—went beyond monetary terms for Anglo anthropologists. Indeed, secrets obtained took on a new form of value, an intangible value that could circulate in professional, scientific circles. This scholarly value, what we sometimes call coin-of-the-realm, calls to mind a short story of Jorge Luis Borges, "El Etnógrafo," in which Borges presents the assumption of professional status in advice given to a budding ethnographer:

At the university, an adviser had interested him in Amerindian languages. Certain esoteric rites still survived in certain tribes out West; one of his professors, an older man, suggested that he go live on a reservation, observe the rites, and discover the secret revealed by the medicine men [los brujos] to the initiates. When he came back, he would have his dissertation, and the university authorities would see that it was published.⁵²

Secrets are presented here as valuable materials that advance a career and are collected on behalf of a scholarly apparatus, situated in the university, drawing on its rendering of the conditions and purpose of information acquisition. Secrets are to be collected to advance knowledge within the social sphere of the anthropologists; and as such, secrets can be considered an object of extraction, a resource moved from one context into another—as Parsons did at by paying Billí at

⁵² Jorge Luis Borges, "The Ethnographer," *Collected Fictions* (1998), 162.

Zuni, and as countless other anthropologist had done among the indigenous communities of the United States and around the world.

The documentation and publication of “secret” knowledge contributed to anthropological science, and through such a contribution, the ethnographer (el etnógrafo) gained scientific authority. By developing relationships with informants, acquiring difficult-to-extract knowledge, and transforming their cultural experiences into scientific knowledge, anthropologists engaged in “boundary work.” This deep, sensitive knowledge was—or was imagined to be—the ground which anthropology held apart from other disciplines, an area of intellectual inquiry unique to itself.⁵³ This disciplinary constitution was indeed the product of their own labors—of learned comportment and linguistic study and interviews and observation—but it was also the product of a clandestine relationships for the exchange of ethnographic information.

This was not the first instance of what I am calling ethnographic exchange relationships—George Catlin and others established certain conditions of information trade in Sioux communities, as did Boas and his colleagues among the Tlingit, to name only two precursors. But by the early 20th century, the Pueblo ecumene had become a place to cut one’s teeth, anthropologically speaking. As university programs popped up around the United States, budding ethnographers, archaeologists, and linguists were frequently sent to Zuni and other pueblos for cursory training. In the pueblo itself, visiting anthropologists accumulated data that served predominantly as a form of training; while the value of ethnological data was present, the deluge of anthropologists seemed to be directed by the logic of experience-building. As shown in previous chapters, Southwesternists moved away from routine surveys to accumulate standardized ethnographic data, and Pueblo people imparted what it meant to be an anthropologist in general. The Pueblo played the part of the indigenous Other who held esoteric information, and who decided what could and could not be taught to outsiders; the Anglo negotiated the social niceties of inquiry and analysis among the “uncivilized.”

Zuni, Acoma, and other pueblos sometimes benefitted materially from the interest of anthropologists, but its people were also constantly on display, pitted against one another, causing strife within the interior of community life. And as anthropology developed more fine-grained questions about Pueblo intellectual and spiritual life, resources began to pool around

⁵³ Boundary work is an important concept in the history of science. See Thomas F. Gieryn, *Cultural Boundaries of Science: Credibility on the Line* (University of Chicago Press, 1999).

individuals with access to knowledge and without the reservations to share it. If the community itself had been a sort of a training ground, a mature anthropologist might later seek a much different setting for knowledge-sharing and documentation—the individual informant isolated from view, off-site, in the hotel-room, the private ranch kitchen, at the remote altar. The secretsharer relationship provided unique information that distinguished anthropologists from among themselves.

In Borges' "El Etnógrafo," when the protagonist returns from time among the Natives he does not share the secrets he learned there. The ethnographer instead tells his advisor, upon his return from the field, "Now that I possess the secret, I could enunciate it in a hundred distinct and even contradictory ways. I do not know very well how to say it, only that the secret is precious and that science, our science, seems to me but a mere frivolity now." Borges' ethnographer returned with new knowledge but did not publish or publicize this resource he had acquired from fieldwork among a Native tribe. Rather, he chose to orient his own life according to the non-revealed "secret."

Such was not the case in Southwesternist research in the early 20th century; Parsons and her colleagues published secrets, revealed them, as products of information that advanced "science." For Anglo anthropologists, ethnographic data, especially secrets, constituted the project they all engaged in—science, "our science."

Chapter 5

Remote and Assisted Ethnography: New Models of Ethnological Data Production

As a developing science in the first half of the 20th century, American anthropology established its authority to speak about Others via thorough records of publication, “contributions to knowledge” that underscored the discipline’s accumulative efforts in the field and its intellectual labor in the library and university. But while ethnographic publications stimulated the discipline of anthropology, they also produced a measure of anxiety, at least for Southwesternist anthropologists, that monographs and offprints would end up in the hands of their subjects, especially Pueblo Indians. As Elsie Clews Parsons wrote to a correspondent about a monograph about the Tewa-speaking Pueblos, “You have with you, of course, a copy of Tewa Social Organization. When you are in San Ildefonso or in Pajarito Ranch, keep it under lock and key, for if any notion gets out of what you are doing, your room will probably be searched. I do not think this publication has reached the Tewa or any of the local white people.”¹

Of course, ethnographic information—extracted from host communities, analyzed and formatted to disciplinary standards, printed and distributed to Anglo scholars in the form of specialized ethnographic studies—at times did travel a trajectory of a different sort, a circuit that brought books back to their “source” in host communities. Though guarded against, a major volume of Puebloan ethnography landed in Isleta, New Mexico, in early 1936. The volume, which was a collection of articles printed as the 47th *Annual Report of the Bureau of American Ethnology* (1932), included a long and detailed examination of “Isleta, New Mexico,” by Elsie Clews Parsons. When Felipe, an Isleta man, opened the volume and perused its contents, he was understandably curious about what had been written by the Anglo anthropologist about his people.²

¹ 22 June 1936, Parsons to Whitman, APS.

² This man we will call “Felipe” to continue to protect his identity even though, as we will see, his real name is part of the published record; still, it will also become apparent that we should continue to obscure Felipe’s real name because that was his wish, a wish that was ignored by later editors.

Felipe read through “Isleta, New Mexico” carefully. He found that the written content was accurate for the most part, but that the images paired with the article were lacking—“you are missing the picture so it would be completed,” Felipe later wrote of one of Parsons’s ceremonial descriptions.³ The paper’s shortcomings he listed down, and noted where he might amend or improve them. Then, Felipe began to write a letter to the author of “Isleta, New Mexico.” As proof of his ability to improve Parsons’s knowledge on Isleta topics, he included an image depicting medicine men costumes and roles. And, to underscore both the value he could provide and the risk he was taking, he wrote in his letter that the drawings were “most secret,” illustrations of events that “no one can see but Indians that believe.”⁴

Felipe’s letter eventually found its way to Parsons via the Bureau of American Ethnology. In it, he wrote that he was hoping to collaborate with Parsons, helping her improve her information about Isleta society and ceremonialism. Felipe was, in a sense, enrolling himself as her informant, an exchange relationship that he hoped would provide him with money in return for images of secretive Isleta events. After proposing an arrangement wherein he would be paid for each sensitive image, he signed his letter and, seemingly anxious about the risk he was putting himself in, added a postscript: “If I had some way to get help in this world I would never [have] done this but I expect to get good help.”⁵

Elsie Parsons initiated their agreement with a \$10 payment for six illustrations.⁶ She had stepped away from Southwesternist fieldwork by 1936 but was not one to pass up data about Pueblo ceremonialism. The material must have been especially compelling because the informant was an able artist who not only had insider access to information and events but could also render them in detailed drawings. Moreover, the informant had presented himself to her and made an offer to supplement her ethnographic information with more detail. She merely had to pay him for his time, his expertise, and (just as important) his risk—something she had done in her relationships with Pueblo informants for decades—to acquire remotely produced ethnographic documentation.

³ “Felipe” to Dorsey, 1 May 1936, Elsie Clews Parsons Papers, APS. Felipe first wrote to Dorsey, who later forward the letter to Parsons.

⁴ “Felipe” to Dorsey, 1 May 1936, Elsie Clews Parsons Papers, APS.

⁵ “Felipe” to Dorsey, 1 May 1936, Elsie Clews Parsons Papers, APS.

⁶ “Felipe” to Parsons, 15 June 1936, Elsie Clews Parsons Papers, APS.

As depicted in the last chapter, some Pueblo informants in the first third of the 20th century had been “pumped” for information in isolation, away from the eyes and ears of their people. Southwesternist anthropologists increasingly had to supplement a Pueblo informant’s “price of fear,” accounting for his or her risk in sharing hidden information with outsiders. In so doing, anthropologists and informants became “secretsharers,” bound by a clandestine relationship that facilitated the exchange relationship and protected both the informant and anthropologist from scrutiny from by the host community. The secretsharer relationship gained importance from the confluence of increased anthropological activity in the region (and need for novel ethnographic material), resistances to Anglo inquiry throughout the Pueblo ecumene, and regional developments such as new labor markets and Anglo education of Pueblo children. While clearly affecting Native communities, anthropological fieldwork was also altered in the midst of social change in the region, as the secretsharer bond was in direct contrast to the method of “participant observation” used by Southwesternists around the turn of the century (a counterintuitive notion, retrospectively, since the regular use of participant observation is often pinned at Malinowski’s *Argonauts of the Western Pacific* (1922)).

This chapter builds on the prior chapter’s examination of anthropologist-informant exchange relationships and the conditions of their clandestine, intimate meetings. By seeking out an exchange relationship with an anthropologist, Felipe, Parsons’s epistolary informant from Isleta, demonstrated his understanding of the situation that Pueblo communities faced as Anglo-Americans settled in the region: he was a recipient of an Anglo education in “letters,” while he also felt anxiety and guilt that he had to sell “secrets,” documentation of his people’s ceremonialism, for his livelihood. Felipe’s remote, assisted ethnography was possible because of his unique and hybrid standing, an ability to understand two different systems of knowledge production and dissemination, not to mention his skill at translating between these two systems via letters and drawings.

Felipe was certainly not the only informant to directly facilitate ethnographic data collection. As we will see, other informants aided Southwesternist anthropologists with ethnographic documentation, sometimes through direct oral testimony, sometimes through written descriptions and diary-keeping. For instance, a young Tewa-Hopi man named Crow-Wing kept a diary of daily life among his people, a project prompted by a meeting with Parsons in 1920. Parsons later published Crow-Wing’s diary with appended ethnographic notes, crafting

a grounded depiction of the ebbs and flows of daily life from an Indian—and not an anthropologist’s—perspective. Informant participation in such assisted ethnography aided the discipline of anthropology as it branched out into new areas of inquiry that sought to understand the persistence of “traditional” elements in individual behavior and indigenous communities in the midst of modernizing America.

Daily life from an Indian perspective was complemented by another style of ethnographic research, the life history or biographical portrait. Life history accounts, such as those recorded by Walter Dyk of the Navajo men Left Handed (*Son of Old Man Hat*, 1938) and Old Mexican (*A Navaho Autobiography*, 1947), spotlighted the individuality of an informant. The informant would tell the story of himself (and less often, herself), an “autobiographical” account that reflected something of their community and its history. While extensive in scope and potentially rich in ethnographic data, this form of ethnography did not necessitate an anthropologist spending extensive time among a Native community. The trend in life histories can be traced to the rising influence of culture and personality studies in anthropology, which, while less a theoretical school of anthropology than a field of inquiry linking psychological study to ethnographic work, emphasized the “cultural patterning” of (adult) personalities within a distinct culture and their reciprocal influence of dominant personality types on the formation of culture characteristics.⁷

The intimate character of these anthropologist-informant relationships was mutually meaningful, although Pueblo and other Indian informants carried much more risk in maintaining these relationships. While some relationships turned into friendships, most are better described as power-differentiated relationships, in which informants came to depend on anthropologists for subsistence and circumspection, relationships in which secretsharing had alienated informants from their communities to some degree. In cases such as Felipe’s, which we will shortly explore in more depth, the conditions of the secretsharer relationship were complicated. Felipe had, after all, sought out Parsons as a patron with a comprehension of the risk he was taking with his standing in Isleta. Parsons, while sympathetic to Felipe’s risk, clearly knew that a relationship of

⁷ For culture and personality, see Robert A. LeVine, “Culture and personality studies, 1918–1960: Myth and history,” *Journal of Personality* 69, no. 6 (2001): 803-818; George W. Stocking (ed.), *Malinowski, Rivers, Benedict and others: Essays on culture and personality*. (Univ. of Wisconsin Press, 1987). For major divisions within culture and personality anthropology, especially those of Benedict (culture is personality writ large) and Edward Sapir (culture is realized differently in its individuals), see Darnell, *And Along Came Boas*.

dependence could facilitate a novel ethnographic project. Parsons, after all, depended on Felipe for unique ethnographic information, including detailed images of ceremonial matters that were impossible to obtain elsewhere. She, in return, paid him for his information, and assured his anonymity in future publications.

Parsons kept her personal pledge to provide for Felipe and protect his identity. But, as detailed in the final section of this chapter, the secretsharer bond could be broken by those outsider of the secretsharer relationship because the preservation of ethnographic documentation on paper (and other media). After the respective deaths of Parsons and Felipe, their clandestine correspondence was released to the world—a world that, naturally, included Isleta.

Felipe's correspondence with Parsons had been ignited by the return of published ethnographic information to Isleta. Published in 1932 in the *47th Annual Report of the Bureau of American Ethnology*, the article detailed Isleta's demographics, practices around major life events, rituals and material paraphernalia, the calendar system, folk stories and mythology, as well as a number of "tales of personal experience" gathered from interviewees. The data that Parsons used in "Isleta, New Mexico," had been accumulated years earlier. Aside from a handful of smaller studies, the catalyst for Parsons's study of Isleta actually came from a younger woman she had mentored, Esther Schiff Goldfrank. In the 1920s, when asked where a budding ethnographer might cut her teeth in the Southwestern field, Parsons suggested that Goldfrank try to "crack" Isleta.

Goldfrank had formerly been to the Southwest with Franz Boas in 1919–1922, acting as the latter's secretary and assistant during fieldwork in Cochiti and Laguna pueblos. She set out on her own in 1924 to Isleta to record folk tales, with funding from the Southwest Society and thus from Parsons, its main benefactor. At Isleta, Goldfrank encountered a resistant ethnographic environment, just as other ethnographers had before her. While she had found an Isleta man who seemed a promising informant, the greater community made work difficult. "I found one man willing to talk," Goldfrank wrote, but our meetings were made so difficult by members of the community that we were forced to continue them in Albuquerque, 13 miles away."⁸ Like Parsons

⁸ See Goldfrank's introduction to Elsie Clews Parsons, *Isleta paintings* (Smithsonian Institution, 1962).

before her, Goldfrank found the tactic of hotel-room interviews better suited for ethnographic data accumulation among reticent Pueblos.

Away from his home community, Goldfrank's informant, "Norman," proved to be a garrulous and excitable man. Parsons later called Norman "exceedingly credulous," for he seemed to truly believe he had seen things supernatural—he claimed to have witnessed a medicine man transform into an eagle and alight to the sky, for example. While the apparent credulity of Norman's belief diminished his authority as an informant, his willingness to talk gave him the anthropologist's ear. In the Albuquerque hotel room, Norman related nearly a dozen folk tales to Goldfrank, who captured them on paper.⁹

Goldfrank left Isleta after her meetings with Norman, but she passed on the informant to her benefactor. Parsons, too, had asked Norman for folk tales, which she later sent on to Goldfrank for comparison. The informant had repeated ten stories to both scholars, and Goldfrank used the subtle differences in the stories to produce a short analytical article for the *Journal of American Folklore*, "Isleta Variants: A Study in Flexibility."

Parsons collected more than just folk tales from Norman, however. Once again isolated in a hotel-room in Albuquerque, Norman willingly provided information on all manner of Isleta life. Through their conversations, he provided Parsons information on the social structure, ceremonial practices, and kinship relations. To check his account, Parsons employed another informant, the pseudonymous Lucinda, who was stricter with Isleta social codes that restricted the free-flow of knowledge, particularly ceremonial knowledge.¹⁰ Parsons compared the information from the two Isleta informants with her amassing archive of documentation on Pueblo communities, and collated her research notes into a long article, "Isleta, New Mexico."

"Isleta, New Mexico" ran over 250 large format pages. Even at such a length, it was not the central feature of that report. It was combined with a similarly long article by Leslie White, "The Acoma Indians," and four articles by Ruth Bunzel (over 500-pages in length) on Zuni ceremonialism, mythology, ritual poetry, and katchinas (deities) by Ruth Bunzel. The 47th *Annual Report* was thus a trove of Pueblo data, a presentation of over 1000 pages of information on Puebloan groups that covered many of the language-families within the Pueblo ecumene—Keres

⁹ Though Goldfrank did not anonymize her informant, I have done so here.

¹⁰ Parsons provided Lucinda a pseudonym in her text but did not do so for "Norman." While this may mean that Norman allowed his name to be printed, I persist in keeping his identity hidden for the sake of his extended family still living.

(Acoma), Tanoan (Isleta), and Zuni. Though the BAE had dedicated volumes exclusively to Puebloan topics in the past, the 1932 publication was a crucial ethnographic resource for anyone interested in Puebloan ethnography. Certainly, for aspiring Southwesternists, the *47th Annual Report* would become an indispensable resource for the field.

Because the *47th Annual Report* exhaustively detailed several Pueblo communities, Parsons and her co-contributors wished to ensure that the volume not be seen by Puebloans for fear of sparking community uproar, which in turn might prevent future work in the region. Both Goldfrank and Parsons had hosted Norman and other informants off-site during their respective visits to the pueblo to avoid any potential controversy with other community members. Tensions between Isleta community members stretched back into the early 1900s, when the journalist and folklorist Charles Lummis had lived in the pueblo, collecting stories from two informants. While Lummis was long gone by the 1920s, in 1927 Isleta's council had obtained his book, *Pueblo Indian Folk-Tales* (1910), and were scandalized by the fact that information had been given over to the Anglo journalist. While one informant had been excused from wrongdoing, another was punished by the community. It was clear, then, that some people in Isleta still resented the past sharing of secrets.

When Felipe happened across the *47th Annual* in 1936, he surely had a clear memory of the uproar over Lummis's book a decade prior; it appears that Felipe did not share his copy of "Isleta, New Mexico" with fellow Isletans. By his own account, Felipe appeared to be in a precarious position. He had been employed with the railroad but had left the job due to an undisclosed health issue. Now, he sought some other form of support, and likely had reasoned that Anglo scholars occasionally wandered into Isleta and other Pueblo towns and offered money or goods in exchange for information. The author of "Isleta, New Mexico" immediately presented the most likely buyer of such information. So, he appealed to her for help. He explained his situation: "I am Indian and [have] no way of making a living, no farm." In exchange for her support, he proposed that he could make more pictures of obscure, secretive aspects of Pueblo ceremonialism.

In offering his paintings to Parsons in exchange for financial assistance, Felipe conveyed his understanding of a particular constellation of values that Anglo anthropologists held: that they would pay for information about hidden facets of Indian life. And, given the limited options

for obtaining secret information from insular communities such as Isleta, Felipe recognized his leverage. Felipe understood that his images would eventually be published—he was, after all, selling his work as a supplement to Parsons’s own. In response, Felipe underscored, early and often, one simple request: “don’t you tell who did this.”

Felipe held the potential to earn money based solely on his experience as an Isleta man with access to, if not mastery of, ceremonial matters. Anthropologists had created the conditions of possibility for this exchange relationship, and it differed from the common economic means (farming, touristic craftwork, manual labor) that Isletans in the 1920s and 1930s had before them. Indeed, this economic position was not based on the exchange of his labor-time, but rather on what we might call his accumulated situational time, on the experience of growing up in a particular social and gendered position in Isleta—on his life history as a Pueblo.

“Being Indian” was a paradoxical, pigeonholed position for economic wellbeing in the early 20th century. Indian-made goods accumulated a certain form of romantic value, but this same romantic value also diminished Indian opportunities in other forms of labor. While the stereotype of an Indian as rural, culturally conservative and closer to nature gave a sort of authentic sheen to handicrafts and art objects, this representation negatively impacted an Indian’s ability to find consistent work in other venues, owing to employer’s fear that he would vanish from the worksite the moment he was called back to home for some reason. Other misinformed images of Indians as uncivilized, unintelligent, lazy, and a “government burden” also contributed to their economic isolation.¹¹

Felipe certainly experienced this economic isolation, as he frequently reminded Parsons. But he had an idea about how to make the most of his position as an Isletan, and he consistently raised his unique access and history in his letters to Parsons. In early correspondence, Felipe was keen to remind Parsons that he could depict things that were “not public” and had never been shown to Anglos because other Isletans were afraid that “if they ever tell they will die or will go in poor health.”¹² Felipe underscored his risk both to increase the potential value of his paintings as well as to remind Parsons, constantly, to protect him from the potential wrath of his fellow community members.

¹¹ Though Felipe did not play up his Indianness outside of the context of Isleta, his labor still derived value on perceived relationship with the past, as best examined by Philip J. Deloria, *Indians in Unexpected Places* (University Press of Kansas, 2004); Deloria, *Playing Indian*. See also Conn, *History’s Shadow*; Bsumek, *Indian-Made*.

¹² “Felipe” to Parsons, 15 June 1936; 20 August 1936, Elsie Clews Parsons Papers, APS

At times, it appeared that Felipe relished his role as a secret ethnographic illustrator. Felipe wrote to Parsons in November to ask for an advance on payment because “I want to make a little trip to Zuni pueblo and I will see some of their secret fire dance and I will draw them.” He then assured her that he aimed to help her and continue to document and send her secrets that she had not seen.¹³ Perhaps, in this case, he also sought to mitigate his continued risk in the clandestine exchange by shifting his focus to another pueblo. The desired advance, of course, may have prompted Felipe to convey enthusiasm for his task, as anyone might do to a prospective employer. But that his enthusiasm could eclipse his misgivings shows Felipe as an interested participant in a form of ethnographic documentation, a mode of collection that further articulated ethnological data and preserved it for posterity.

While Felipe reminded Parsons that his work was, ethnologically speaking, valuable and unique because he was an Isleta Indian who had access to things even other Isletans did not see, he also put forward his experiential knowledge as a source of authority. By lineage and experience, Felipe could know things others could not. In one letter to Parsons, Felipe pushed back on a suggestion that he was not giving the whole truth on a matter that Parsons had raised in a previous letter. “I can not draw a picture of war in Poland,” he wrote, “because I don’t know what is going on there. If I were there and I learned, I might, but I can not do it [draw] without seeing. I have to be there a long time before I learn about it. It’s the same here. I was here and raised here.”¹⁴ Here, Felipe explained something of his epistemology and philosophy of representation in this letter to Parsons. He asserted a link between the lived experience and the contents of truthful representation, a link that excluded informed but distant representation modes such as imagination or fancy.¹⁵ Felipe cast himself in the role of a “modest” witness to Isleta ceremonialism, the point of view of one who understood the practices he witnessed, someone that offered expert corroboration.¹⁶

¹³ “Felipe” to Parsons, 27 November 1936, Elsie Clews Parsons Papers, APS.

¹⁴ “Felipe” to Parsons, 13 October 1939, Elsie Clews Parsons Papers, APS. Parsons filled in some gaps in Felipe’s grammar in this and other letters, some of which has been retained for the reader’s benefit, without significantly altering the feeling of his message. I try to avoid the use of the editorial tools (brackets, sic) that indicate usage “errors” and omissions as often as possible because I find they produce misunderstandings as often as they clarify. I generally follow the Chicago Manual of Style on issues of silent correction (16th edition, 13.7).

¹⁵ In a letter from May 1941, Felipe wrote everything he drew he had seen and learned from his elders, and that he could not possibly fabricate his images. “If I don’t [know] nothing I can’t make it just to make it because I wouldn’t [know] what to say.” “Felipe” to Parsons, 5 May 1941, Elsie Clews Parsons Papers, APS.

¹⁶ Shapin & Schaffer, *Leviathan and the Air-Pump*.

At the same time, Felipe accepted many parts of Parsons's profile of Isleta presented in the 1932 article. Felipe appeared to have read the 1932 Isleta paper over and over and decided which images to illustrate based on her textual explanations. He often referenced the page numbers of her text in one of his images so that she could see what element he was elaborating upon. In a series about childbirth, for instance, Felipe painted scenes of events surrounding pregnancy and childbirth, and referred to the individual pages as they were printed in the *47th Annual Report*.¹⁷ Tapping into scientific conventions of credible witnessing and citation, Felipe engaged the ethnological discourse on Isleta that Parsons and others had fomented and insisted on his ability to do so with authority.

At times, Felipe contrasted his insider status with other ethnographers, including the journalist Charles Lummis. To Parsons, Felipe wrote, "You are getting more real stuff than Mr. Lummis did because he just learned from one old man, [redacted], but he never learned about all this work because they would not tell anyone, not even an Indian unless he belonged to a medicine society. Even [some] Indians of this pueblo don't get to see this." Felipe then underscored the unique, "secret" aspect of his painting and the risk he put himself in: "You are getting most of [the] secret things that no one else will ever get. I hope they will never get me for doing this."¹⁸ After he sent the aforementioned letter, Felipe perhaps dwelled on his assertion that he gave accurate information and, two days later, wrote to Parsons a brief letter with additional ethnographic information to be appended to his previous one. Felipe could by turns vacillate in his concern for his place in Isleta's "traditional" milieu or his desire to engage in a beneficial relationship with Parsons that aided him in the Anglo sphere of social and economic activity.

Implicitly, too, he also questioned Parsons's ability to judge as a non-Pueblo. She had not seen the things he had, and as such was limited in her ability to assess them as truthful or not. Felipe positioned his work as authoritative ethnographic representations in a way that an anthropologist could not hope to obtain. And because of this situated position, Parsons would have to present the images not merely as illustration, but as locally-sourced ethnographic documentation—she would have to present Felipe's images as coming from an Isleta informant. Paradoxically, even though Felipe wanted his identity hidden, he at times seemed proud of his

¹⁷ While Parsons arranged Felipe's paintings thematically and according to the Isleta ceremonial calendar (following her organization scheme for nearly all of her professional writing on Puebloan societies), Felipe had sent images at various times. "Childbirth" themes, for example, were present in 9 images sent between 1936 and 1941.

¹⁸ "Felipe" to Parsons, 20 June 1939, Elsie Clews Parsons Papers, APS

ethnographic role, as in a 1939 letter in which he wrote, “I don’t want to do this just to get money,” he wrote. “You will be surprised some day that you found someone that did you a big favor.”¹⁹

Felipe’s sense of pride about sharing secret information was more often overwhelmed by a sense of guilt, however. Felipe often wrote that he felt a pang of uncertainty about his agreement with Parsons. “I don’t know if I am doing right to tell all this or not. Sometimes I feel funny. Everything is secret. Too much work for me.”²⁰ While he appeared to trust Parsons, across the years Felipe more often than not ended his letters with remarks on the risky nature of his work. Over the course of his correspondence with Parsons, he directed his mail intermittently to Albuquerque when he felt particularly at risk, and back to Isleta when he needed income quickly and thus would take the risk of exposure. Felipe was clearly being pulled on several sides: he needed money to help him along, but he also needed to keep his access to secret information secure, not to mention ensure his own well-being among his people.²¹

Felipe’s worries were not without reason. Isleta, like other communities across the Pueblo ecumene, at times experienced tension over the influence that outsiders, particularly influences such as Anglo education and economic forces, had in the town.²² In addition to controversies stirred by Charles Lummis’s book of Pueblo folk-tales, Felipe was at risk of becoming known when outsiders came to Isleta for ethnographic research. He was almost “outed” in January of 1937; according to Felipe, scholars from Columbia University had written to a trading post in Albuquerque asking for him by name; they “want me to work for them,” he told Parsons. When he was questioned about this inquiry by a fellow Isletan, Felipe made a strong denial: “I told them I don’t know how to draw, I am not an artist so I don’t [know] nothing.”²³ While Parsons appeared to have kept his name away from Isleta’s orbit, she had not been strict enough in her silence among her New York circle, and Felipe’s name had circulated around Columbia as a prospective informant.

¹⁹ “Felipe” to Parsons, 13 October 1939, Elsie Clews Parsons Papers, APS

²⁰ “Felipe” to Parsons, 15 Jan 1940, Elsie Clews Parsons Papers, APS

²¹ It is also possible to consider Felipe’s position in light of W.E.B. Du Bois’ notion of “double consciousness,” the internal conflict of recognition experience by subordinated people. Here, however, Felipe flipped Du Bois’ lens, for he was plagued by the burden of thinking about his actions from the perspective of his own people.

²² For an account of factionalism in Isleta up to the late 1940s, see French, *Factionalism in Isleta Pueblo*.

²³ “Felipe” to Parsons, 10 January 1937, Elsie Clews Parsons Papers, APS.

By the summer of 1937, Felipe seems to have come to trust Parsons's continued patronage enough to write longer explanations to her follow-up inquiries. He began to offer extended explication of some aspects of his paintings and the unrepresentable aspects of ceremonial positions, such as the lineage of war chiefs or midwife specialists. Parsons asked for clarifications about certain cultural components—of, for instance, childbirth practices, war chief roles, sweat baths—and Felipe's letters from 1937-1940 were primarily responses to her questions. With trust established, Parsons was able to conduct a more robust form of ethnography-at-a-distance.

Benefits of Remote Ethnography

Felipe, of course, was not Parsons's first ethnographic correspondent. She had an epistolary relationship with many Native Southwesterners, Pueblo friends and acquaintances to whom she would send letters as well as money and presents of clothes and hats. Aside from her secretsharer relationship with Felipe, Parsons's other strong relationships with informants had been established in the field. And, if the informant was "lettered," he or she could continue to provide her with useful ethnographic information (as well as news and tidings from the town) long after Parsons returned to her home in the East.

The accumulation of ethnographic documentation remotely, through the mail and (unlike the linguistic schedules of the 19th century) directly from the informant, proved to be a relatively safe and discrete form of ethnological knowledge production. While Southwesternist anthropologists could typically observe "daily life" of their informants, it was more difficult to understand the mentality of the Pueblo individual without meeting with them in isolation (and the risks of this were shown in the previous chapter). Anglo presence invariably altered an informant's behavior, and "daily life" was hardly possible when an informant was attempting to mitigate the ramifications of engaging with an outsider. One solution to this problem was to ask informants to do work autonomously, gathering ethnographic information while the anthropologist was away from the pueblo or settlement. An account of everyday life in an Indian community that was written in a diary or journal *by* an Indian informant would provide a lens into the mundane everydayness that an informant may not convey to an inquiring anthropologist.

Parsons, remarking in 1925 on the numerous studies of the Hopi towns of the past forty years, lamented that "in all this volume of record, for all the towns, there is one striking gap:

there is little or no record of the life of the people from within, so to speak.” Parsons’s plan for capturing the “life of the people from within” was to convince a literate informant, a Tewa man named Crow-Wing (aka George Cochise) to keep a diary. Here, she hoped, Crow-Wing would record his thoughts and feelings, and perhaps give insight into the thought-processes that structured his existence.

Crow-Wing was born in Hano, the Tewa-speaking town nestled among the mesa-top Hopi villages, and later moved to his wife’s house in Sichumovi. Crow-Wing was an ideal diary-keeper for several reasons. He had been educated at a school in nearby Keam’s Canyon and had spent time working in Denver, but had returned to Hopiland without, as Parsons put it, becoming fully “Americanized.” In Crow-Wing, Parsons had found someone who was

“enough of a traveller and observer of disparate cultures to understand the interests of the ethnologist, enough of an Indian to respect the town life, and to acquire and cherish knowledge of ceremony. In other words he belongs to that choice class of informants, middle-aged men for the most part, who are “Americanized” sufficiently for ethnological work, and who appreciate, without disclaiming, their own culture.”²⁴

Crow-Wing thus held a specific set of tools that enabled him to capture his “traditional” Tewa/Hopi experiences on paper, in a style unique to his situation and upbringing. Crow-Wing agreed to keep a journal of his daily life on the mesas, which he would later share with the anthropologist. Crow-Wing began his diary just after Parsons left the Hopi mesas in December 1920. He wrote in the journal fairly regularly, continuing for a year. He also occasionally wrote letters to Parsons, detailing his progress as well as major events in town.

Parsons considered Crow-Wing an honorable man, willing to do almost anything to make their time together pleasant. But she also recognized that, in terms of secretsharing, there was a line that Crow-Wing would not cross. He was particularly mum about ceremonial issues and objects: “No White person, he has assured me, will ever see the ancient Tewa mask of which he is trustee, and in view of public opinion or gossip he would not say a word, I have reason to surmise, to facilitate admission for any White to esoteric ceremonial.”²⁵ Crow-Wing would act on behalf of an Anglo such as Parsons, but would not allow an Anglo to violate any stricture against seeing or learning about sensitive matters.

²⁴ Parsons, “A Pueblo Indian Journal, 1920-1921,” *Memoirs of the American Anthropological Association* 32 (1925), 6-7.

²⁵ Parsons, “A Pueblo Indian Journal.”

From an ethnographic standpoint, Crow-Wing's journal was both interesting and ordinary—or, rather, it was interesting because it was ordinary. In her introduction, Parsons pointed out that while the everydayness of the journal may have resembled American newspapers, Crow-Wing omitted any mention of births, marriages, divorces, illnesses, and deaths—an omission Parsons interpreted as indicative of the social function of privacy. In such a compact village, of course, Crow-Wing would have been apprised of every birth and death, every marriage and marital spat. Yet he did not consider it proper, or of interest, to note these major life events of people in his community. While this frustrated Parsons, Crow-Wing was expressing a Tewa/Hopi perspective on ethnographic documentation, crafting his entries in line with an understanding of Anglo modes of documentation and his own conception about what information was appropriate to write down.²⁶

Crow-Wing concluded his record-keeping in December 1921, when he mailed his journal to Parsons and reflected on some omissions in his entries. Crow-Wing expressed some hesitation about his phonetic rendering of Tewa and Hopi names. He was unsure of his spelling at many points in the journal, and in the moment of writing a given entry had been unsure of how to remedy this. At many points in the journal, Crow-Wing left out Tewa words, inking in a long blank after putting down an initial letter: “Powamu chief was now going to make p_____; his partner will go and make _____ with him and every body getting ready for to dance.”²⁷ In this case, as he explained, he simply wasn't sure of how to write out “prayersticks” in English (hardly surprising given non-American concepts were not taught in Anglo schools). Later, he wrote to Parsons, “you [can] tell where I can't spell the words. If we meet together then I will tell you what the word is.”²⁸ Parsons and Crow-Wing did meet together again, and in their meeting Crow-Wing elaborated on several entries. Parsons later incorporated some material from their follow-up interview into Crow-Wing's entries and rendered his whole diary into an annotated manuscript.

Tewa/Hopi spelling issues aside, omitted words were not always a matter of unsure spelling. Crow-Wing's journal was virtually devoid of proper names. Instead, Crow-Wing wrote phrases such as “Two boys have gone” or “The girl grinds corn all day,” illustrative of some

²⁶ For another example of “indigenous” influence on ethnological data accumulation, see Schumaker, *Africanizing Anthropology*.

²⁷ Diary of Crow-Wing, 7 February 1921, Elsie Clews Parsons Papers, APS

²⁸ “Crow-Wing” to Parsons, 22 May 1922 [Cochise/Cochisi to Parsons], Elsie Clews Parsons Papers, APS.

wariness of putting names on paper. Parsons interpreted this in line with a common cultural practice of name-avoidance that characterized the Puebloan ecumene: “Among all Pueblo Indians as among other Indian peoples, the use of terms of relationship is preferred to the use of personal names, and there is definite avoidance by juniors, such as exists among us towards our parents.”²⁹ Here, the scribe probably leaned on his knowledge of whom he was writing, relying on the contextual experience that informed oral community communication.

The finished document served as an example for a novel tactic for ethnographic data accumulation, not to mention a signal of Parsons’s continued creativity in ethnological pursuits. With Crow-Wing, Parsons engaged in a form of remote, assisted ethnography—documentation that did not require her presence but was done on her behalf. The journal offered concise statements on arrivals or departures from the town, on Crow-Wing’s work and on community-wide projects, and on games and ceremonial events. Paired with photographs of the Hopi villages as well as drawings of katchinas (deities), Parsons published “A Pueblo Indian Journal, 1920-1921” in the *Memoirs of the American Anthropological Association* in 1925. In her presentation of the text, Parsons thoroughly edited Crow-Wing’s writing to comply with formal English grammatical rules and added copious expository footnotes throughout the work. As for his omissions, where possible she inserted a community member’s proper name, such as those of Crow-Wing’s family members, that the author had originally avoided direct reference to; Parsons also inserted the Tewa words that Crow-Wing had been unsure how to spell. Everywhere else, she maintained Crow-Wing’s account and voice, and incorporated his additional notes from their later collaboration to elaborate certain details.³⁰

Parsons’s publication of Crow-Wing’s journal came in the midst of a growing interest in the individualism of indigenous subjects, a focus that still attended to Indian communities but sought to understand how such communities shaped the “personality” of its singular members (and vice versa). Parsons’s biographical studies influenced culture and personality anthropology, as it came to be known, which adopted elements of psychoanalysis, particularly the analysis of

²⁹ Parsons, “A Pueblo Indian Journal,” 73, fn 116.

³⁰ It appears that Parsons interviewed Crow-Wing after she received the publication (or talked to someone who had intimate knowledge), because there are instances that she adds language to his text. For instance, she inserted an explanation of why a man was “not a good chief” in parentheses, maintaining Crow-Wing’s voice, on 1 August 1921. The original does not have his explanation. It is not clear why she did not put this information in footnotes, but it suggests that Crow-Wing expanded on information in the journal in a follow-up interview with Parsons. This would also explain how Parsons was able to insert proper names in the text.

individual case studies.³¹ Though divided on many points, the study of the individual-within-culture largely eschewed racial/biological determinism and, by the study of individuals within cultural groups and not merely the groups themselves, culture and personality studies expanded the range of study subjects available. Regardless of whether a scholar believed personality constituted culture or culture produced a set of variable personalities, anthropologists could turn their attention to individual lives (and their stories) and, in turn, illuminate the social world of their kinfolk.³²

Life History Ethnography

Parsons had made a biographical turn even before her employment of Crow-Wing for the daily life journal. During time at Zuni in the late 1910s, Parsons had become fascinated with a young woman, Waiyautitsa, and collected some information about her life as child and young adult in the Zuni community. In 1919, Parsons published a biography of Waiyautitsa in *The Scientific Monthly*. Through the story of Waiyautitsa's childhood, Parsons discussed Zuni girls' clothing, linguistic terms of address, and social positions, in contrast to boys. Parsons followed this contrastive view throughout; telling Waiyautitsa's story was akin to giving information on Zuni girlhood and transition to womanhood, supplemented by the boy/man version as contrast. Parsons's work here contributed to an understanding of women's roles in ceremonialism and social order.³³ Three years later, in 1922, Parsons published a revised version of the biography in a volume, *American Indian Life*, that she edited and organized. Parsons, it seems, had received encouragement for her original biographical sketch of Waiyautitsa and she subsequently asked other anthropologists to craft their own Indian biographies—or, as they were often called, “life histories”—for *American Indian Life*.³⁴

Biographical sketches of American Indians did not, of course, originate with Parsons; the formal aspects of the genre stretched far back in the Anglo-American canon. Nearly 80 years before, in 1833, John Patterson had published an “autobiographical” account of the Sauk leader Black Hawk.³⁵ Biographical details of informants could come out in longer ethnographic

³¹ See Stocking (ed.), *Malinowski, Rivers, Benedict and others*.

³² LeVine, “Culture and Personality Studies.”

³³ See also Stevenson, “The Religious Life of the Zuni Child.”

³⁴ Elsie Worthington Clews Parsons (ed.), *American Indian Life* (University of Nebraska Press, 1922).

³⁵ Black Hawk, *Black Hawk: An Autobiography* (University of Illinois Press, 1833).

depictions, and Southwesternists, too, had provided snippets of some of their key informants' life histories. Matilda Coxe Stevenson, for example, wrote about her two-spirit informant We'wha, from whom she gained much valuable information about Zuni gender roles and material-cultural practices. John Gregory Bourke's military histories also included ethnographic details of allied and hostile Indians, especially impressive leaders; Bourke drew upon his prior ethnographic work to bolster his biographical portraits. Non-ethnologists also provided biographical—and at times romantic, overblown—depictions of notable Indian chiefs or warriors such as Sitting Bull (Lakota), Geronimo (Apache), or Manuelito (Navajo). These prior models, however, did not attempt to situate the biographical subject as the narrator of this information—Geronimo, for instance, did not narrate his own life in a single sitting. He was rather the object of study by journalists, travelers, or other aspiring biographers, who used articles and past interviews to collage together a biographical account.

Life histories, while borrowing some of the narrative conventions of biography, was focused specifically on “culturally-significant behaviors,” that is, actions that fit within the scope of the “traditional” or were unique to a given community.³⁶ The goal was to depict an individual informant's personality—and a distinctly “Indian” personality—through their recorded autobiography. Unlike biographies, life histories presented an informant's perspective directly. Personality data came from the contours of the telling—the story structure, use of chronology, or how the informant presented actions and responses and interactions with others, for example. From the raw material provided by the informant, the anthropologist could then analyze general cultural attributes that influenced the informant's personality.³⁷

While Parsons's early work in the life history genre did put a spotlight on a Pueblo individual's life—Waiyautitsa and Crow-Wing, respectively—her focus did not ignite a trend for a new method for studying the Pueblo ecumene. Incidentally, though Pueblo life history did not take off, the nearby Navajo communities became an often-used source for life history study in the region. Navajo Indians were seen as much more individualistic than their neighbors, the Pueblo peoples. While this contrastive historical belief (which survives to this day in

³⁶ On the perceived non-modernity of Indians in Anglo-American culture, see Conn, *History's Shadow*. For Indian resistance to their relegation to stereotypes of the non-modern, see Deloria, *Indians in Unexpected Places*.

³⁷ Sidney W. Mintz, “The anthropological interview and the life history,” *The Oral History Review* 7 (1979): 18-26; Vincent Crapanzano, “The life history in anthropological field work,” *Anthropology and Humanism* 2, no. 2-3 (1977): 3-7; H. David Brumble, *American Indian Autobiography* (Univ. of Nebraska Press, 1988).

anthropological thought) cannot wholly explain the precedence of first-person life histories from Navajo individuals, Navajo autobiographies became prominent examples of the genre in the 1930s.

Among the most notable life histories of the early 20th century was a record of the life a Navajo man named Left Handed, a biography captured by Walter Dyk in the 1930s.³⁸ Walter and his wife Ruth Dyk spent 1933-1935 engaging in fieldwork among the Navajo. They began their biographical interviews with Left Handed in 1934, aided by the Navajo translator Philip Davis (and often meeting in his house near the Lukachukai mountains). Left Handed's account of his own life was full of emotional intensity, and "at times he became so disturbed and worn out that he would take four or five days off, going away to his home or to visit relatives."³⁹

Their conversations with Left Handed were published as *Son of Old Man Hat* in 1938 (a later volume of the remaining material from Dyk's record of Left Handed's life history was printed as *Left Handed, a Navajo Autobiography* in 1980). As culture and personality scholar Edward Sapir said of Dyk's the landmark *Son of Old Man Hat*, "one discovers that a "primitive" can talk, often prefers to talk, about his personal memories even where they do not seem to give the ethnologist chapter and verse for some important rubric in his filing cabinet."⁴⁰ In other words, the informant provided loads of data, but it was not formatted for ethnological analysis, nor did it even fit with the reigning ethnological questions of the day.

As Dyk's fieldwork experience showed, however, recording a life history was not as simple as copying the informant's meandering recollection of his or her life. Preceding his time with Left Handed, Dyk had worked with another Navajo, Old Mexican, with the interpreter Dan Warren. Dyk's recording of the life history of Old Mexican had been frustrating: Dyk saw Old Mexican as an egomaniacal and vindictive man. To Dyk, Old Mexican told a version of his life history "which cast him, so he believed, in his proper role—that of a magnanimous and virtuous man, abused, imposed upon, and frustrated by an unappreciative world. Consequently there is little in his story that is not intended to incite either admiration or pity."⁴¹ Dyk's perspective on

³⁸ Left Handed is not to be confused with Sir Left Handed, or Hosteen Klah, a prominent medicine singer and weaver around the turn of the century. Navajo public names were (and are) often descriptions, Left Handed being a public name given to left-handed people. Other common names, as seen in Chapter 2, involve height or demeanor, such as Tall Chanter (literally, a tall man who is a medicine singer) and Laughing Singer.

³⁹ Old Mexican and Walter Dyk. *A Navaho Autobiography* (Viking, 1947), 6.

⁴⁰ Left Handed and Walter Dyk, *Son of Old Man Hat* (Harcourt, Brace, 1938).

⁴¹ Old Mexican & Dyk, *A Navaho Autobiography*, 6.

Old Mexican perhaps indicates why the biography, under the unimaginative title *A Navaho Autobiography* (1947), was published nearly a decade after *Son of Old Man Hat*, whose central figure was much more to Dyk's liking.

Navajo life history took off—at least as much as an ethnographic genre and its application to a distinct Indian group can be said to take off. As might be expected, life histories of Puebloan peoples were slower to launch, save Parsons's proto-biographies of Waiyautitsa and Crow-Wing. By the 1930s, however, anthropologists had increased success enrolling Pueblo informants to give autobiographical accounts. Don Talayesva, a Hopi from Oraibi, told his story to Leo Simmons, an account that became an important document in Southwesternist circles when Simmons published it as *Sun Chief: The Autobiography of a Hopi Indian* in 1942.⁴²

Talayesva's relationship with Simmons, who collected and edited Talayesva's life history, aroused some controversy within the Hopi towns. Some accused him of selling secrets; others, that he was receiving money from Washington for what amounted to a serious offense: selling dead Hopi bodies. But for the anthropologist focused on a single informant, the cares of the town only affected his work only insofar as the singular informant became anxious, cautious, or otherwise disinclined to share information. Such tensions did not appear to have significantly altered Talayesva and Simmons' relationship. Simmons respected Talayesva and let him speak about nearly anything he wanted. Simmons recognized that this free-form style and its corollary inductive data accumulation would produce unbiased life histories. According to Simmons, "an astute investigator with a theory to prove or disprove may possibly extract from a naïve informant an approximately desirable answer."⁴³ That did not, of course, mean that Simmons' own framing of the project lacked bias. But his approach left much to be decided by Talayesva.

Simmons was working to fill gaps in Hopi data for the Cross-Cultural Survey at the Institute of Human Relations.⁴⁴ Talayesva had initially occupied a more traditional informant role and received 35 cents per hour for his work. Eventually, Talayesva's own life story interested Simmons enough to alter the project to focus on Talayesva and his specific life experiences. To continue his work with Talayesva, Simmons was adopted by Chief Tewaquaptewa and Don

⁴² Another Hopi biography of note, *Truth of a Hopi* (1936), was that of Edmund Nequatewa, who had worked with Mary-Russell Colton in the 1930s.

⁴³ Leo W. Simmons, *Sun Chief* (Yale University Press, 1942).

⁴⁴ For the Institute for Human Relations, see Jill G. Morawski, "Organizing knowledge and behavior at Yale's Institute of Human Relations," *Isis* 77, no. 2 (1986): 219-242.

Talayesva, as “son” and “brother” respectively, an initiation that appeared to have come from a genuine desire by these Hopi men for Simmons to be incorporated into the community. As a new member of the Sun Clan, Simmons had received some permission to learn subjects which were regarded as clan and Hopi secrets.

For Talayesva’s life history project, Simmons also managed to enroll Talayesva as a “participating observer” in Hopi life, “taught to report on the events of the day, together with his mental and emotional reactions to them.” Simmons initially scribbled these reports into diaries, and later Talayesva took over this task. The diaries kept by Talayesva were later incorporated by Simmons into the monograph. Here, following in the footsteps of Parsons, Simmons relied on an educated Hopi to produce ethnographic information in written form, documentation which provided both direct information on daily life practices, as well as the formal written account that could offer a window into the narration choices, inclusions and omissions, and emphases that came from an Indian mind.

For Simmons, allowing Talayesva to tell and write his life history without significant constraints did have its shortcomings. By the time Talayesva had penned a remarkable 3000 pages of his diary of daily life, Simmons decided to try a more directed approach. In his initial entries, beginning in 1938, Talayesva reported his everyday activities. Eventually, his excessive detail—descriptions of each menu item in his three daily meals, for example—prompted Simmons to request that Talayesva steer his writing toward more significant or unique happenings in his daily life. Specifically, Simmons hoped to know more about a ceremonial called the Soyal. Some information about the Soyal had been presented in a 1901 work, *The Oraibi Soyal Ceremony*, recorded by George Dorsey and H.R. Voth. Knowing that Talayesva’s clan associations gave him a position in the Soyal ceremony, Simmons asked Talayesva about the ceremony and showed him Dorsey and Voth’s book. Talayesva had (and likely reminded Simmons at this junction) originally stipulated that during their life history work he would not describe ceremonial contents or processes, recognizing that other Hopis may be angry with him for sharing such information. “What I do in the Soyal is secret. When you ask me about that it sets the people against me.”⁴⁵ Though Simmons argued that the account had been published for

⁴⁵ Simmons, *Sun Chief*, 6. Talayesva had agreed, however, to relate his experiences with ceremonials about which there was already published historical records—a compromise that Simmons recognized as shortsighted, because “he did not know what had been published” about prior events of which Talayesva had been a part.

40 years and thus was widely known, Talayesva had strong reservations about sharing information about the ceremonial.

Though the extent of their working relationship is obscure, Simmons and Talayesva eventually repaired the fracture in their bond that the Soyal book presented. In fact, Simmons convinced Talayesva to give his opinion about the 1901 book's representation of the Soyal ceremonial. Taking the Dorsey and Voth book in hand, "Don examined the pictures and drawings of the altars and seemed distressed, making such remarks as, 'This is awful. It makes me unhappy. That man Voth was a thief. The secrets are all exposed.'"⁴⁶ Here, Simmons recognized the long-standing reluctance to share ceremonial information with outsiders; Talayesva, for his part, presumed the anthropologist had secretly engaged in his documentation, including surreptitious sketching of altarpieces and other paraphernalia. But Talayesva also offered Simmons his own explanation of the ceremony, cementing their relationship as secretsharers. This connection could not inspire Talayesva to speculate beyond his knowledge, of course. Simmons subsequently asked Talayesva to inspect another Dorsey and Voth volume on a different ceremonial altogether. In this incident, Talayesva refused to even inspect the book or hear its description of the competing ceremonial. Talayesva, as a steward of the Soyal "secrets," avoided learning other secrets he should not know.

While biographical details of Indians had entered anthropological field notes and the diaries of travelers and traders since the early nineteenth century, Elsie Parsons seemed to find the leading edge of a trend in life histories that emerged in the 1920s and continued after her death in 1941.⁴⁷ Paul Radin's 1926 *Crashing Thunder* was particularly influential, showing that the documents of fieldwork—in this case, notes on a Winnebago man's life—could be rendered into credible ethnographic data. Over the next two decades, life histories came to be understood as documents that possessed interwoven parts that more aptly captured the "essence" of a given culture, an ineffable thing that charts, tables, and descriptions failed to convey.

⁴⁶ Simmons, *Sun Chief*, 7.

⁴⁷ Kluckhohn provides an exhaustive bibliography of life history ethnography up to the time of his publication. Clyde Kluckhohn, "The Personal Document in Anthropological Science" in Gottschalk et al, *The Use of Personal Documents in History, Anthropology, and Sociology* (1945). See also Brumble, *American Indian Autobiography*.

Life history ethnography drew on the authority of an individual's experiences to present information from their perspective. Autobiographical information gave a specific perspective, and regardless of the presumed truth or falsehood of the story, the life history was seen to be an important ethnographic document precisely because it conveyed a *perspective* which could be analyzed.⁴⁸ As literary scholar Brumble asserts in his study of *American Indian Autobiography*, "The as-told-to Indian autobiographies are evidence of two personalities and two cultures. In such narratives we often find two different sets of narrative assumptions at work, two different sets of aims, and two very different senses of what it means to tell the story of one's life."⁴⁹ Anglo narrative structures were cinched onto Indian recollections and indigenous modes of representing themselves through a story of life.

Critical voices have long noted the skewing of autobiographical ethnographies toward the religious, the different, the extreme experiences of the Other.⁵⁰ While some amanuenses tolerated free-writing about daily events or meandering life stories, the desirable content was certainly "traditional" elements that differentiated Indians from "modern" Americans. Anthropologists, too, recognized this issue. In a 1945 review essay on "personal documents" in anthropology, Clyde Kluckhohn overviewed the history of the "life history" genre.⁵¹ He identified the problem of the amanuensis or interviewer in Indian autobiographies, since the anthropologist was invariably the document-maker. The ethnographer could thus direct storytellers toward areas he or she was most interested in; Kluckhohn wondered, for instance, whether Walter Dyk had a "nontraditional" informant in *Left Handed*, or whether he steered *Left Handed* to disproportionately discuss material (here, the predominance of sexual exploits) that were "nontraditional" conversation topics (that is, they are not found in other Navajo autobiographies). For Kluckhohn, Don Talayesva's story represented "the most satisfactorily published example of a personal document study from a nonliterate society."⁵² Kluckhohn overlooked, however, that Talayesva was absolutely literate; he documented daily events in a diary for Leo Simmons, presenting a "lettered" perspective on his "non-literate" community. Of course, the anthropologist's role in bringing the text to its final stage of production could, consciously or

⁴⁸ On the "life history" genre, see also Regna Darnell, *Invisible Genealogies: A History of Americanist Anthropology* (University of Nebraska Press, 2001).

⁴⁹ Brumble, *American Indian Autobiography*, 12.

⁵⁰ Vine Deloria, *God is Red: A Native View of Religion* (Fulcrum Publishing, 2003); Deloria, *Custer Died for Your Sins*.

⁵¹ Kluckhohn, "The Personal Document in Anthropological Science."

⁵² Kluckhohn, "The Personal Document in Anthropological Science," 95.

not, render Indian subjects in normative Anglo-American life-trajectories—as in, for instance, the chronological ordering of a life history even though the telling may have been anything but linearly chronological.

Life histories portrayed an Indian perspective on life. They also served, at least in some instances, to increase the visibility of the person profiled, which could have consequences in their home communities. Such increased recognition did not always have negative consequences, to be sure: a life history profile certainly provided some informants with increased material wealth and prestige, while others opened themselves up to criticism by allowing an Anglo to capture and publish an account of their and their community's life. Moreover, these varied experiences were not mutually exclusive, as the case of a Taos man, Lorenzo Martinez, conveys.

Lorenzo Martinez had, by 1930, considered using his education to write up something ethnographic—an autobiography or a more systematic explanation of his home community of Taos, New Mexico. He worked as a printer and farmer and was reminded of his literary motivations when the anthropologist Leslie White came to Taos in 1930. Lorenzo Martinez became friendly enough with the visitor, eventually answering some of his myriad questions about life at Taos, about their clan and kin relations, and about their government and leadership. The anthropologist asked a lot, and for the most part Martinez was forthcoming. But when the Anglo man asked after the kiva ceremonials, Martinez flatly told him that he could not provide this information.

For the visitor White, Martinez's reluctance was not surprising. White's strategy was to press on with men like Lorenzo Martinez who were willing to talk a bit. White would listen and open up to Martinez to build mutual confidence and maybe, just maybe, the Taos man would reveal something of his kiva experiences. But, Martinez told the visiting anthropologist, there was tension in the Taos community, and to share ceremonial information would only stir the pot. Indeed, though Martinez and White had been discreet in their conversations, Martinez's interactions with the Anglo became known to the community, and he faced renewed criticism from a group centered on Juan Mirabal (aka John Gold Tooth), a Taos cacique with the power to make Martinez's life unpleasant in the tight-knit high-desert town. To Martinez, sharing information, especially if it was put into print, could be the tipping point against his standing in

Taos, and he might have to leave his family, friends, and source of income if Mirabal pushed him further.⁵³

To Taoseños like the Mirabals, printed materials and other outside influences were objectionable additions to Taos life. Martinez in all likelihood represented a hybrid form of Anglo and pan-Indian lifestyle that they stood against. Lorenzo Martinez had been sent to Carlisle Indian School as a boy in the 1890s; not only did he bring back various Anglo beliefs and dress, but at Carlisle he had been introduced to an emerging pan-Indian peyote “cult,” which he brought back to Taos in 1896.⁵⁴ Lorenzo was sometimes called “Carlyle Martinez” because of his time at the Carlisle Indian School, where he learned Anglo letters and a system of belief that some feared would displace or otherwise corrupt the present (and historical) ways of life.⁵⁵ Martinez had weathered the troubles in his home community through the years, and he knew well the sore spots that, if pressed, would send Taos into a frenzy. Thus when Leslie White asked him to act as an informant, Martinez was hesitant to stir up trouble.

To circumvent the surveillance of their relationship, White tried to prompt Martinez to write about Taos in his own words, since “Carlyle Martinez” could read and write. To inspire the Taos man, White showed Martinez a recently published book by the Lakota leader Luther Standing Bear, *My People, The Sioux* (1928). As it happened, Martinez had been friends with Standing Bear at Carlisle, and White suspected that Standing Bear’s book might influence his old Taos friend. As White later described his strategy,

I encouraged him in his ambition to write the book rather than asking him to help me. I felt that this was the wise move at that time; I wanted to draw him out, to expose himself as much as he would. And he did. He told me enough to convince me (as much as one

⁵³ Stewart, “Taos Factionalism”; Parsons, *Pueblo Indian Religion*; Elsie Clew Parsons, “Taos Pueblo.” *General Series in Anthropology* 2 (1936): 74-96.

⁵⁴ Peyotism, a spiritual and healing movement that often used the hallucinogenic peyote cactus for devotional events, was probably a major source of the rift between Martinez and the Mirabal family. The Mirabals, who held secular and religious leadership positions, in time came to oppose the Peyote cult, as it stood to diminish their own positions within the social and spiritual hierarchy. Stewart, “Taos Factionalism”; Parsons, *Pueblo Indian Religion*.

⁵⁵ For many Puebloan people, the problem with Anglo education was that it opened up tensions about social influence in their tight-knit communities. Anglo education was understood as influence from a foreign, outside source. In 1906, tension in the Hopi villages over the issue of Anglo education resulted in a community split. “Traditionalists” from the various towns decamped to found a new town, Hotevilla. While the “Oraibi Split” was the starkest rupture over the issue of Indian education, other Pueblo towns experienced factionalism that broke over differing opinions about Anglo schooling for local children. See Whiteley, *Oraibi Split*.

ever is convinced by what these people tell you) that he would be willing to talk about anything.⁵⁶

While White believed Martinez would take the bait, Martinez told the visiting anthropologist about his previous notion to write about Taos. But Martinez also conveyed that he had had second thoughts when he recalled another Taos man who had written something about their community and had passed away early in life.⁵⁷ The man's writings had been found by Taoseños and brought before the tribal council, and there the texts had been translated into Tiwa, the language of Taos. As he later told White, "They were furious, old and young, women and girls, all were just like wolves, mad at that [man]. They all said what they would have done to him had he not died."⁵⁸

Even with this historical warning, Martinez gave serious consideration to White's suggestion that he pen an account of Taos. If he were to "spill the beans," Martinez would need a place to write, away from the surveillance of his home community. At this, White offered him a place at a San Gabriel Ranch, a rented home-base for ethnographic work away from the pueblos, where informants could be brought for comfortable—and private—conversations about sensitive materials.

More than just seclusion, Martinez also asked for leverage against the faction led by the Mirabals. He knew that Juan Mirabal, although now a strict follower of a "traditionalist" spiritualism, had helped a visiting anthropologist over three decades prior. Martinez recalled the anthropologist was named Miller, but he had never seen his book that had surely come from the ethnographer's time with Mirabal.⁵⁹ So he asked White to get Miller's book for him—that way, if Lorenzo published something about his people and Mirabal took issue, Martinez could counter that Mirabal had also been given secrets to an anthropologist.⁶⁰ Martinez, of course, may have been "drawing out" the anthropologist for his own ends. If he led the anthropologist to believe he

⁵⁶ White to Parsons, 21 October 1930, Elsie Clews Parsons Papers, APS. White also met an unnamed man at Taos who was studying with the "old timers" in the traditions of Taos, but one day might write a book about all the secrets. "He told me that he was "in training", that he was letting his hair grow, that he was going to become thoroughly trained in all the esoteric "Indian doings" of the pueblo, become a prominent officer, "rule the pueblo". Then, finally, the whole pueblo would collapse, "because the government isn't going to take care of us much longer; they're going to let us go", he was going to capitalize his knowledge by writing books about their secrets!"

⁵⁷ White to Parsons, 22 July 1930, Elsie Clews Parsons Papers, APS.

⁵⁸ White to Parsons, 22 July 1930, Elsie Clews Parsons Papers, APS.

⁵⁹ Merton Leland Miller, *A Preliminary Study of the Pueblo of Taos, New Mexico* (University of Chicago, 1898). Miller never published more ethnographic work.

⁶⁰ White to Parsons, 21 October 1930, Elsie Clews Parsons Papers, APS.

would write down his own account of Taos life and reveal some of its obscure and hidden practices, he might obtain leverage of his own against a rival.

Lorenzo Martinez, in the end, never authored a book or gave his life history to an anthropologist. But his pondering over the possibility and negotiation with Leslie White was retained in White's field notebooks and correspondence, and the politics of documentation in the midst of Taos' negotiation with "outsider" influences is telling. In their brief negotiation for information exchange, the conditions of increased, intimate ethnographic access centered on political economy of documentation and information sharing—Lorenzo Martinez would withhold information from White unless another document could be provided to him, a document that supposedly showed that another man, a rival, had shared information with an anthropologist thirty years prior. Martinez considered writing information himself and having it published, especially with leverage against a rival who had also shared information. But to do such writing, he would need to be away from the community about which he was to write, away from the potential questioning he would receive about his actions. This impulse to publish ethnographic information was bolstered by another document, a book by Luther Standing Bear, which showed that indigenous writing could be given the weight of the Anglo press, that an Indian story could be told by an Indian, that different forms of prestige and fame could be accessed by Indians.

In parallel to Taos' politics of information sharing, anthropologists had established a shared understanding about the circulation of ethnographic material back to its source. Southwesternist anthropologists like White and Parsons knew to keep their reference books and personal notebooks away from Taos and other Pueblo communities, for otherwise communities might erect new barriers to ethnographic research. Thus, although Leslie White promised to find and bring to Lorenzo Martinez the aforementioned book, this was a hollow promise. As White later wrote to his mentor Elsie Parsons, "I told him that I would try to locate a copy of Miller's book—although of course I am not so simple as find one and give it to him."⁶¹

Ethnographic documentation continued to be a controversial practice in Pueblo communities into the 1930s and beyond, up to the present day. But ethnography was not simply

⁶¹ White to Parsons, 21 October 1930, Elsie Clews Parsons Papers, APS.

an issue of “superstition” about documentation and did not merely chart the boundary between “insider” and “outsider” in Pueblo communities. Pueblo communities could be divided over the potential relations with Anglo anthropologists. Some argued that they should make a permanent record of cultural practices that may someday disappear; others insisted that traditional Pueblo divisions around distributed stewardship of information remain and that outsiders take no part in the continual transmission of knowledge.

As Talayesva and Martinez’s stories show, information-sharing by indigenous informants (in the Southwest, but elsewhere, too) could be complicated by dueling logics within the minds of different people within a community. On the one hand, Puebloans generally desired the basic maintenance of community sanctity, and viewed secret-sharing as a violation of community norms of knowledge stewardship. On the other, Anglo-educated Pueblos (and others who lived or worked in the Anglo-American sphere) may have been inclined to think about cultural preservation in a manner different from others in their home community and could perceive value in making a record of their lifeways and unique traditions—a perpetuation of practices that subsequent generations might let fall into disuse. The dueling logics could be seen in the life history of Don Talayesva, for instance, which bridged both tensions but did not resolve them, and the *Sun Chief* appeared a man caught between two worlds. Finally, life history writing seemed to animate Indian culture in a way that tables, charts, and topical descriptions could not. Drawing on the move from schedule-based inquiry to long-term fieldwork of decades prior, anthropology shifted toward “thick” descriptions (long before the term was given by Geertz), a rich new area of ethnographic inquiry that probed deeper than numbers or linguistic tables could represent.

Problems with Publications

Of course, Pueblo communities did acquire documents written by anthropologists, as demonstrated by Felipe’s story and tensions over received publications indicate. Some Indians from Jemez pueblo complained that Elsie Parsons had caused internal conflict when they had learned about her 1925 publication, *The Pueblo of Jemez*. One set of correspondence indicated some people in Jemez wanted the names of her informants, asserting that it was an issue of fairness for the secretsharer to have his or her identity revealed.⁶² In another instance, Parsons

⁶² Hare, *A Woman’s Quest for Science*.

was angered when a government employee read excerpts of her study of Taos to leaders of that community. “A copy reached the Indians over a year ago through a land conservation man.... Of course he was ignorant of the trouble he was starting.” For Parsons, part of Taos’ aggravation would come from mistranslation across multiple interlocutors; two interpreters had translated English into Tiwa, Taos’ language, as an Anglo reader read the book aloud. In this case, since Taos already had several copies of the book, Parsons hoped that someone (such as a considerate Anglo) would provide a more positive interpretation of the work and explain its respectful, preservative values to the leaders. This hoped-for consequence did not come to pass. In fact, the results appeared much more serious: Taos leadership eventually identified a recently-deceased man as Parsons’s informant, and his son was forced to divest his share in communally held lands.⁶³

As Parsons told her mentee, the budding ethnographer Leslie White, in the 1920s, “I suppose you are aware that for the sake of fellow workers [i.e., ethnographers] one must never show Pueblo Indians any publications.”⁶⁴ While a publication would surely cause a stir within communities, anthropologists sought to protect their access to communities, and the revelation of publications could seriously hinder such access. Even as Parsons reminded White to withhold publications from his interlocutors, she sent him and other Southwesternists books, articles, and other scholarly materials that would aid their work. “Do not fail to keep these publications under lock and key. It is most important to our work that no Pueblo Indian see them and they will take a book with pictures of masks when they won’t take anything else from your belongings.” Anthropologists recognized the important link between information access and information control. Parsons and White, among others, sought to close down any potential return of ethnographic material to communities from which the information was extracted. Ethnographic documents, to protect the continued accumulation of ethnographic data, had to be kept secret from Pueblos.

This practice of disciplinary secrecy, Parsons stated, should be also be adhered to with the local Anglo priest and “any other White.”⁶⁵ Here, Parsons indicated that *knowing* the sensitivity of another culture—even when such practices did not conform to one’s own notion of the

⁶³ Zumwalt, *Wealth and Rebellion*, 256.

⁶⁴ Parsons to White, 17 May 1926, Leslie White Papers, BHL.

⁶⁵ Parsons to White, 3 June 1926, Leslie White Papers, BHL.

sensitive and private—was crucial to being able to continue to gain information. Other Anglos could not be trusted to keep a book secret because they may not have adequately appreciated that Pueblos would find the work scandalous.

When books did make it into a Southwestern community, as Parsons's book on Taos found its way to that community in the 1930s, trouble was sure to follow. One major problem, overlooked by anthropologists in the field, was the lack of anonymity of non-informants, especially in contrast to an anonymized source. In many cases, an anthropologist's informant was simply dropped from view, referred to in footnotes as "my informant." To Pueblos, who of course had not learned the conventions of a budding discipline that was only just then establishing fieldwork norms, if a text described a person engaged in a private ceremonial activity, it was not a far leap to presume that the described person had also revealed the information. This had the unfortunate, unintended effect of causing confusion over whom was the secretsharer. Tony Mirabal of Taos wrote to Parsons accusing her of misrepresenting him in her monograph on the mountain town. "Now when you use my name in your book about all kinds of things without any hesitation that makes people think I have been telling everything you have got."⁶⁶

Books about Pueblo Indians were controversial topics within these communities. To be sure, Pueblos saw some value in such works—for historical and celebratory reasons—but it appeared they were also frustrated that they no longer held and stewarded forms of knowledge themselves. Ultimately, this meant that ethnographic monographs, and especially the anthropologists that produced them, should be considered warily by Pueblo communities.

Given the situation, anthropologists had to contemplate the ethnographic publication process. Even the publication outlet should be considered, at least from Parsons's and White's views. White wrote to Parsons in 1933 about the University of Chicago Press, which he deemed would be too public an outlet. Books by the press could end up in a bookstore or be provided for tourists at a prominent Santa Fe hotel, such as the La Fonda. "They would surely have it in the lobby of the La Fonda in Santa Fe. Within 48 hours after it entered the La Fonda, they would be

⁶⁶ Mirabal to Parsons, Elsie Clews Parsons Papers, APS. Quoted in Lavender, *Scientists and Storytellers*, 63. Less often, informants would be given pseudonyms, but in a world like the Pueblo ecumene, where names could change and people knew intimate details about one another's lives, it was not difficult to ascertain the identity of an informant by details beyond a fake name.

holding all-night meetings in [Santo] Domingo to discover the culprits. There would be hell to pay all up and down the valley. It might be better to have it come out in some non-commercial medium where the likelihood of discovery is reduced to a minimum. I have never seen any memoir of the Amer. Anthrop. Assn., e.g., on sale in Santa Fe.”⁶⁷

In part, Puebloans viewed secretsharing as a selfish burden on the community, a form of dissension that prevented social cohesion. Not only were informants paid (or they were perceived to be paid by their associates), but their revelations were seen as diminishing the potency of traditional Pueblo powers. Concepts of fairness and labor-equivalency circulated among Pueblos. If an informant heard that an ethnographer was paying another storyteller \$12 per story (an immense amount for a single story in the early 20th century), he or she might stop sharing altogether on grounds of exploitation or unfairness. Such a confrontation happened to Parsons as she was collecting stories at the Laguna village of Paguete, though she insisted to her informant that she paid all storytellers the same, \$1 per story.⁶⁸ Exchange relations, when public, could cause strife within Pueblo communities. Anthropologists could exploit this strife—what amounted to jealousy—to leverage new contacts who sought material gain from information-sharing. Factions within a single Pueblo could even be played against one another, as was the case in Taos. Just as Pueblo sacred knowledge was decentralized and distributed, the idea that an individual community member could exploit the system of compartmentalized knowledge for personal gain appears to have been a major concern for early 20th century Pueblo towns.

When Felipe of Isleta presented himself to Elsie Parsons as an informant, as one willing to use his unique position to aid her ethnographic project in return for compensation, he was a rare breed of informant, willing to circumvent the normal paths of knowledge stewardship. Felipe’s precarious position, moreover, cast him as a unique informant in Parsons’s address book. He was willing to provide her with more sensitive, secret information than any previous informant, but appeared especially motivated to do so because of his financial situation and other personal troubles, just out of view. From Parsons’s perspective, his apprehensiveness was “wholly sincere, I think, even when it was translated into pecuniary terms. Fear has to be compensated for, as

⁶⁷ White to Parsons, 26 October 1933, Elsie Clews Parsons Papers, APS.

⁶⁸ Louis A. Hieb, “Elsie Clews Parsons in the Southwest,” in Nancy J. Parezo (ed.), *Hidden Scholars: Women Anthropologists and the Native American Southwest* (University of New Mexico Press, 1993).; Elsie Clews Parsons and Franz Boas, “Spanish Tales from Laguna and Zuñi, N. Mex.,” *Journal of American Folklore* (1920): 47-72.

another Pueblo Indian once put it when he asked me ‘How much will you pay me for my fear?’”⁶⁹ His precarity and anxiety, additionally, offered Parsons a look at contemporary social politics in Isleta, a view of intra-group issues that were seldom available to outsiders.

Felipe’s commentary provided some information in the politics of knowledge maintenance and access, as when he referred to a sweat-lodge that had been left to ruin after the medicine man who practiced its curative rite had passed away. “They [Isleta medicine practitioners] still know how, but since White people have begun to come around to watch, they hide from them.” Presumably Felipe meant that the sweating chamber, formerly in public view, had been relegated elsewhere or, at the very least, the proceedings were strictly kept from outside view. Here Parsons learned something from Felipe that would have otherwise been difficult, since as a white woman the medicine practitioners would not have revealed themselves to her.

The politics of documentation, too, came out in the letters. In some paintings Felipe had named the people he depicted in the images, which was certainly a risky move on top of his secret production of ceremonial paintings. One image depicted the burial preparation of the recently deceased White Corn Chief and included his given name on the page. Later, Felipe apparently thought better of this, and wrote additional notes on the image’s surface directing Parsons not to mention the chief’s name when she eventually published the material, for he was a powerful leader and to name him in death could cause misfortune. His letter went even further: “I don’t want to mention his name. You can guess easy. Don’t you write his name.” Even beyond the view of his people, the psychological power of name-avoidance held Felipe. Surely, he figured, better safe than sorry, even in his secretsharing.

Parsons understood that Felipe’s drawings conveyed aesthetic and ceremonial details that could not be described by informants. As she wrote in her draft of *Isleta Paintings*, “Many details are given that could hardly be brought out in verbal descriptions or in photographs, which, in any case, are taboo in Isleta.”⁷⁰ Parsons herself was foreclosed from direct experience regardless, for Felipe portrayed many aspects that were specific to an exclusively male social setting. Parsons had asked for images of women’s work, but these “did not appeal to the artist as subjects for portrayal—and this in itself is significant.” While she could not wholly control his ethnographic depictions, because Felipe’s ethnographic information came in a hybrid form, in illustration and

⁶⁹ Parsons, *Isleta Paintings*, 2.

⁷⁰ Parsons, *Isleta Paintings*, 3.

textual description, his data gave a new dimension to her anthropological study of Pueblo ceremonialism.

Parsons was struck by Felipe's drawings and commentary because they offered an absolutely unique perspective. She could forget hotel-room interviews; if Felipe could simply explain a ritual through drawing, including pertinent details and commentary, he would give not only a formal representation of an event and its role-players, but also impart the elements that Pueblos considered to be most important. As for his truthfulness in secretsharing, Parsons saw Felipe as trustworthy. She wrote that she particularly appreciated that he did not "paraphrase his information in order to make the Indian aspect intelligible to a White person"; rather, Felipe understood Parsons's demonstrated experience with Pueblo social and ceremonial systems and gave her an insider's perspective.⁷¹

Even though anxiety plagued him, Felipe continued to develop Parsons as a patron, elaborating new areas of interest that would provide him with a financial return. As Felipe drew a spectrum of events and practices at Isleta, he recalled things that Parson's "history" did not have. Beginning in the summer of 1940, Felipe inserted additional statements about the unique perspective he offered the anthropological world. Around this time, Parsons seems to have suggested their agreement would soon come to an end. In response, Felipe decided to expand his range of paintings to topics outside of Parsons's 1932 work. After explaining some images he had included in a letter, Felipe underscored his knowledge with an aside—"(see how much I know?)"—and then rhetorically questioned the correctness of him "tell[ing] you too much which you don't know, ... even the people of this village don't know."⁷² If Parsons had indeed intended to wind down the relationship, her mind had changed by the new year, and Felipe continued to send paintings and long letters providing novel ethnographic data for another year.

At this time, Felipe seemed to be in a more difficult position than in previous years. There had been worrisome moments for Felipe in the past, but his letters to Parsons in 1941 indicated financial trouble and increasingly difficulty in making accurate drawings without attracting attention.⁷³ But by the Fall and early Winter, Felipe's tone had returned to eager. Parsons,

⁷¹ Parsons, *Isleta Paintings*.

⁷² "Felipe" to Parsons, 9 October 1940, Elsie Clews Parsons Papers, APS.

⁷³ "Felipe" to Parsons, 1 February 1941, Elsie Clews Parsons Papers, APS.

however, had also suffered for her health. On December 4th, surely without realizing it, Felipe wished Parsons a happy new year for 1942. She would not live to see that commence, and Parsons died on the 19th of December. Felipe and Parsons had sent many letters over their decade of correspondence. And yet, the two secretsharers never met in person.

At the time of Parsons's passing, Felipe had illustrated and sold 189 watercolor and ink drawings and had sent at least 56 letters to Parsons explaining his work and situating it ethnographically. Before her death, Parsons had begun a manuscript draft, incorporating 140 images from her acquisitions. She included notes on the ceremonial or social practices depicted, weaving together data from Felipe and her own ethnographic notes. She also provided a lengthy introduction, a scene-setting section of text that provided a justification for the publication of such sensitive images. Most notably, the clandestine source of the paintings—from a member of Isleta's most exclusive fraternal societies—had “considerable ethnographic value.... Many details are given [by Felipe] that could hardly be brought out in verbal description or even by photography, taboo at any rate in Isleta.”⁷⁴

Parsons's drafted introduction noted the reticence to secretsharing held by the Pueblos, which in this case was grounded on “the characteristic and deep-rooted Indian attitude that knowledge or practice when divulged to the non-initiated loses its potency.”⁷⁵ Parsons reasoned that Felipe justified his secretsharing because, as he put it, “you know it anyway”—that is, she had published on Isleta in 1932 and had already displayed her knowledge of its history and practices. He thus was able to get around the sanction against sharing information with the uninitiated lest the powers at work be diminished. To be sure, Felipe struggled with this circumvention, but he further justified his actions as a desire to correct and supplement Parsons's previous scholarly publication. Perhaps Felipe even encountered moments of fatalism, for if he passed on the opportunity, someone else would provide information that would encompass the information void anthropology sought to fill.

The Publication of *Isleta Paintings*

Two decades after Parsons's death, *Isleta Paintings* was published by the Bureau of American Ethnology. The book's publication in 1962 came through the efforts of Parsons's

⁷⁴ *Isleta Paintings* Manuscript, Elsie Clews Parsons Papers, APS. See also, Parsons 1962.

⁷⁵ *Isleta Paintings* Manuscript, Elsie Clews Parsons Papers, APS.

friend, mentee, and fellow ethnographer, Esther S. Goldfrank, the same scholar whom Parsons had encouraged to “crack” Isleta back in the early 1920s. For years after Parsons’s death, Goldfrank had been attempting to secure support for the publication that Parsons had envisioned, chiefly by putting the impressive images Felipe had prepared on public display in various lecture halls and anthropology departments around the country.

The Smithsonian Institution published the work in a large-format volume of nearly 300 pages and filed the monograph as a *Bulletin of the Bureau of American Ethnology* (#181). Of the 189 images Felipe sent to Parsons, 140 were published, each image individually placed on the recto page and paired, on the verso page, with a brief commentary from Parsons. 10 of the 140 paintings were published in color, with the remaining 130 images presented in black and white.

Edward P. Dozier, a Puebloan from Santa Clara and an anthropologist, reviewed *Isleta Paintings*, offering a situated perspective on the publication. Dozier assessed Parsons’s work, as well as Goldfrank’s assembly of the content, as a positive contribution to Pueblo anthropology. He was impressed by Felipe’s ability to keep the work secret, an ability that he predicted would be more limited in the aftermath of the publication.

Perhaps most unlikely is the possibility that another native Pueblo artist will have the courage to venture on a project of painting ceremonial activities. Pueblos zealously guard the religious aspects of their culture and the strictures that befall the informer are so stringent that few dare to reveal ceremonial secrets. Something as graphically realistic as water color paintings of one’s townspeople in ceremonial activities, and so cleverly executed that identification of the individuals portrayed is possible, takes extreme courage or else foolhardiness.⁷⁶

Here, Dozier recognized that Felipe’s actions were sure to cause problems in Isleta and that, for better or worse, the publication had probably foreclosed future secretsharing relationships between Indians and anthropologists.

Dozier stressed that Parsons’s dedication to ethnological pursuits may have blinded her to a basic sympathy for Felipe. In his review, Dozier mused about the moral implications of the anthropologist-informant relationship when it came to secretive materials—“Pueblo Indians, and ethnologists as well, have long wrestled with their consciences on this problem—should Pueblo Indians reveal age-old ceremonial secrets and should ethnologists freely publish the information

⁷⁶ Edward P. Dozier, “Review of *Isleta Paintings* by Esther S. Goldfrank,” *American Anthropologist* 65, no. 4 (August 1963): 936-937.

they have gained, often in confidence?” Ultimately, Dozier, perhaps in light of his review in an anthropological outlet, concluded that pragmatic Puebloan informants “become sincerely interested in putting on record a passing, rich culture, even though in the process they may risk Pueblo censure and abuses”—a justification that Dozier ascribed to Felipe.⁷⁷

The relationship between Parsons and Felipe—two people with vastly different life situations, whom never met in person—can be understood as a relationship of secretsharers, as people bound by the sharing of secrets. This secretsharer dynamic—notably found in Joseph Conrad’s *The Secret Sharer* (1909), in which a novice sea captain is burdened by and yet willingly keeps a strange stowaway’s secret presence⁷⁸—is relational: A “secret sharer” is not simply someone who “spills the beans” but rather one of the holders of a common, hidden bond—a partner in a partnership in which things are held in confidence.

Parsons held Felipe’s secret drawings and correspondence, and in order to protect Felipe and continue their mutually beneficial relationship, she was inclined to keep his identity and actions a secret. Parsons, to be sure, courted virtually no risk in their relationship; she kept Felipe’s secrets in order to further her accumulation of information. But, in her manuscript for *Isleta Paintings*, she also stipulated that Felipe’s true identity not be made public. In draft and in print, Parsons quoted directly from one of his letters in which Felipe had her, “I will complete all the secret drawings. It will be all right for you to publish them some day, but don’t tell who did this, it [would be] hard on me.”⁷⁹

***The Artist of Isleta Paintings* (1967)**

Felipe’s anonymity was not to last. Five years after the publication of *Isleta Paintings*, the editor of the work, Esther Goldfrank, parlayed the ethnographic data Parsons had acquired into another volume—a monograph on the *author* of the *Isleta Paintings*. This work bore a straightforward title—*The Artist of “Isleta Paintings” in Pueblo Society*—and provided

⁷⁷ Dozier, “Review of *Isleta Paintings*.”

⁷⁸ In Conrad’s book, the stowaway had murdered an insubordinate crewman on another ship in order to save everyone else on board from a coming storm, and arrived on the captain’s ship with a plea to save him. If he were to reveal the stowaway, the captain would put his authority into question—and betray a man with whom the young captain identified, the stowaway that he continually called his “double.” In Conrad’s work, secrets are in multitude—the reasons for murder, the stowaway and his presence on the ship, the captain’s plans to help the man escape—and the tension is built around the duo who share the secrets.

⁷⁹ Parsons, *Isleta Paintings*, 2.

photographic reproductions of the letters of the artist to Parsons, full of his constant pleas for her to maintain his anonymity.

While Goldfrank did not include letters from Parsons to Felipe (a few of which do exist in the same paper collection at the American Philosophical Society that housed Felipe's letters to Parsons), she did include Parsons's notes on "Who's Who in Isleta" and any background information on Felipe that she could obtain. The "Who's Who" section was a list originally written up by Parsons, featuring 48 individuals of Isleta whom Felipe mentioned or whom Parsons knew from her own experience in the pueblo. Goldfrank cross-referenced the named people in Parsons 1932 "Isleta, New Mexico" text as well as in the 1962 posthumous volume. With the "Who's Who," Goldfrank initiated a second revelation, having once revealed the paintings to the public, she next revealed the artist's name, community association, and his fraught correspondence with Parsons for all who wanted to see.

In publishing the artist's name and his side of the correspondence with Parsons, Goldfrank felt she had a "duty—scientific and artistic—of identifying the highly gifted Pueblo Indian who had given us this memorable record of life in his native village." Moreover, the author's letters and his clear emotional hand-wringing about his sale of sacred imagery provided a glimpse into Pueblo "personality and the institutions that shape it." Goldfrank asserted that anthropology could benefit from study of "the artist's recurring reference to his hopes, his fears, and his work. Anyone who has ever tried to probe beneath the seemingly calm surface of Pueblo society and elicit intimate personal reactions, especially in the Rio Grande villages, will realize how precious these statements are."⁸⁰ For Goldfrank, enough time had passed since the Isleta artist's death (a decade prior), and other Anglo scholars had studied Isleta with consenting, public informants. What harm could it do?

Because Felipe had desired anonymity, Parsons did not (and could not) elaborate her informant's life history. In her introduction to *Isleta Paintings*, Parsons also noted that Felipe did not paint pictures of daily, "secular" life, although she had asked for them. "Activities, such as cooking, eating, sleeping, or the merely economic aspects of farming, hunting, and handicraft, did not appeal to the artist as subjects for portrayal—and this in itself is significant. Certainly it

⁸⁰ Esther Schiff Goldfrank, *The Artist of "Isleta Paintings" in Pueblo Society* (Smithsonian, 1967).

underlines the well-known difficulty of securing Pueblo autobiographies.”⁸¹ Goldfrank took up this missing life history element for her introduction to *The Artist of “Isleta Paintings.”*

To bolster her ethnographic celebration the artist, Goldfrank drew on the influence of the life history genre and culture and personality anthropology broadly. Though waning by the 1960s and fractured in its theoretical orientation, culture and personality anthropology had maintained the Boasian lesson of expressive “genius” and the fruitfulness of psychological inquiry; practitioners, such as Goldfrank, who cut their teeth in the early decades of the 20th century continued to focus on individuals and their accomplishments to root out the “unconscious patterning of behavior” in their given society.⁸²

Goldfrank’s 1967 volume celebrated “scientific and artistic” axioms of liberal individualism. For her, the artist should be known in order to be credited, celebrated, and (importantly) studied. His work, after all, provided a glimpse at Isleta life that virtually no Anglos could comprehend or even access. His unique access, his view of hidden elements given through his illustrations, suggested a parallel with the autobiographies such as those of *Sun Chief* or *Son of Old Man Hat*. If Don Talayesva and Left Handed could be known and remembered as notable contributors to anthropology, then why not Felipe? Moreover, for Goldfrank, knowing the identity of a creator or discoverer—and he must be an individual and not a group—was important as an historical record and as the result of certain structural conditions that had produced such a personality.

This revelation and focus on the individual, of course, was contrary to the Isletan view, shared by other Pueblos, that emphasized the community over the individual.⁸³ Without doubt, Goldfrank understood that the publication of *Isleta Paintings* had caused a stir at the pueblo. The ethnographer felt some justification for revealing Felipe’s identity because Isleta had learned of the 1962 publication; the community had already identified the artist, internally. That Isleta had been astonished by the information’s revelation—and not by its inadequacy or falseness—signaled to Goldfrank that the “very intensity of their reactions seems to guarantee that the

⁸¹ Parsons, *Isleta Paintings*, 3.

⁸² Edward Sapir, “The Unconscious Patterning of Behavior in Society,” in *Selected Writings of Edward Sapir in Language, Culture and Personality*, edited by David E. Mandelbaum (University of California Press, 1949).

⁸³ Goldfrank herself understood this, as she demonstrated in her “hydraulic” thesis for Pueblo society. Esther S. Goldfrank, “Socialization, personality, and the structure of Pueblo Society (with particular reference to Hopi and Zuni),” *American Anthropologist* 47, no. 4 (1945): 516-539.

representations are valid.”⁸⁴ Goldfrank’s reaction to Isleta’s displeasure was removed and ethnographic; she called the artist a “rebel,” part of “a society where, as one anthropologist put it, “disobedience is a sacrilege and heresy as well as treason” (White, 1932, p. 11).” For Goldfrank, Felipe “was not attracted by the Pueblo road to recognition and power—a priestly vocation.” By interpreting his personality as that of a “rebel,” Goldfrank opened up an imagined potentiality that the Artist could disrupt the conventions of his community to the benefit of the anthropologist, who was merely present to collect information. Rebels could be informants and celebrated for their iconoclasm and ingenuity in the face of oppressive and stagnant conservatism.

Although the artist did break a social code of Isleta, his rebellion was probably linked to his socioeconomic position. Goldfrank directly pointed to Felipe’s occasional moral lapses that punctuated his obedience to Isleta mores, even while calling him a rebel. “The wonder is that while he breached a basic principle of his society, that while his anxiety over this action never abated, he nevertheless chose to remain in Isleta, outwardly conforming, except when he was drunk, to its authoritarian mode of life.”⁸⁵ Here, the “rebel” that was celebrated a few lines previous was cast as a drunk, uncredited with decisive agency, and provided with another justification for his secretsharing. Here, Goldfrank appeared to deem the artist a rebel insofar as it made him an agent in his own actions. Here, he was also a drunk, a delusional rebel, even a selfish one—a quixotic figure that provided data for anthropologists against the wishes of his people.

For Felipe’s ghost, this would have been a confusing portrait of the artist. He was cast as many things: talented, rebellious, given to drink; outwardly conforming, inwardly desirous. He became a sort of martyr for anthropological knowledge—that he had revealed secret information and aided anthropology would seem to justify Goldfrank’s revelation of his name and place within Isleta, to be celebrated as a talented artist and contributor to knowledge. Goldfrank enacted two transformations for Felipe: one, into a hero-rebel for anthropology; another, into a duplicitous and complicated figure in the town and community where he lived and died.

⁸⁴ This quote was provided by an anonymous anthropologist at Isleta, quoted in Goldfrank 1967, 16. It is ironic that Goldfrank sought to protect the Anglo anthropologists who provided her with follow-up information from Isleta. As she noted at the beginning of her introduction, seemingly referring to Anglo anthropologists or other non-Indian informants, “I omit the names of all who have given me information as I do not wish in any way to jeopardize their relations with the people of Isleta” (1).

⁸⁵ Goldfrank, *The Artist of “Isleta Paintings,”* 16.

As Dozier had commented on Parsons's *Isleta Paintings*, so another Pueblo anthropologist reviewed Goldfrank's *The Artist of "Isleta Paintings."* Alfonso Ortiz, a noted ethnologist from San Juan Pueblo (Ohkay Owingeh), reviewed Goldfrank's second publication of Isleta material. "A few questions arise with respect to what Goldfrank regards as her foremost duty in preparing the book, that of identifying the artist,"⁸⁶ wrote Ortiz, and he continued to gently question if Goldfrank had thoroughly ensured the protection of Felipe's extended family (Felipe, recall, had died shortly after Parsons). Ortiz also lamented that it was difficult to assess the motivations of Felipe, who lived in constant fear of death for his secretsharing during his correspondence with Parsons, due to the lack of thorough biographical information on the artist. These criticisms aside, Ortiz nonetheless praised Goldfrank's contribution to Pueblo anthropology, for her work added yet more data to the archive of Pueblo ceremonial knowledge.⁸⁷

Both works, Parsons's posthumous *Isleta Paintings* and Goldfrank's *The Artist of "Isleta Paintings" in Pueblo Society*, dealt with the primary aim in human science: the production of "knowledge" through the accumulation and analysis of novel, interconnected cultural documentation. The archival, accumulative impulse that characterized ethnography was in these cases extremely important. Both Parsons and Goldfrank aimed to increase the information about Isleta available to outsiders, first at the level of the community and greater Pueblo ecumene, and later at the level of the individual and his personality.

The documentation of such ethnographic facets—or their capture, if you like—was each scholar's ultimate aim, a concern that superseded the relative communicative value of specific information in each monograph. Often, the act of revelation is implicitly justified in anthropological writing. Readers of Parsons and Goldfrank may have learned more about Isleta ceremonialism, but what did one do with the information after that? As a document—as a piece of paper with words and images on it that referred directly to private rites of the Isleta people—

⁸⁶ Alfonso Ortiz, "Review of *The Artist of "Isleta Paintings" in Pueblo Society* by Esther S. Goldfrank," *American Anthropologist* 70, no. 4 (August 1968): 838-839.

⁸⁷ That two Pueblo anthropologists, Dozier and Ortiz, publicly supported both *Isleta Paintings* and *The Artist of "Isleta Paintings"* was probably unsurprising to other anthropologists working the 1960s. Though anthropology had always fostered constructive criticism (and its harsher varieties), the Southwestern scene was much more aligned in its theoretical and practical aims. Even an outlier like White, with his neo-evolutionist thought, was accepted by this crowd, and the group appeared to insulate itself against dominant professional personalities such as the students who had come up around Franz Boas.

the work was a record of cultural practice. In the anthropology of the middle 20th century, this documentary work could be construed as a service for the community in question.

The conditions of Felipe's "supplement" were complicated, allowing various interpretations. On the one hand, it can be said that Felipe wanted to reveal to Parsons the "truth" of some of her representations, to correct errors in her history and ethnography. On the other hand, it can also be said that Felipe wanted monetary remuneration, since he had lost a job with the railroad and was not a farmer, the typical way to forge a livelihood in Isleta in the 1930s and 40s. Regardless of the interpretation one chooses, Felipe firmly established his desire to remain anonymous for his own protection and for his extended family (he himself was a bachelor).

In contrast to the autoethnographic diary of Crow-Wing and the autobiographies of Left Handed and Don Talayesva, Felipe did not provide his "life history" as an ethnographic document or case. Remote and clandestine ethnography such as Parsons's collection of the *Isleta Paintings* did not conform to the emerging field of culture and personality studies. In this field of anthropological analysis and fieldwork, informants could be seen as proto-authors, to whom the credit and authority of authorship were due. In this school, anonymity was not desired by either party in the anthropologist-informant relationship, for the life history of the individual needed to be attached to a name, as a record and representation of a single person's life.

Felipe's desire for anonymity—and Parsons's basic agreement to provide it—held on to a deeper tradition in anthropology that described social forces that shaped and constituted the communities from which it emerged. Felipe positioned himself as an anonymous artist-informant, someone who did Parsons "a big favor" in her task of social ethnographic description, but he did so at a risk to his own community position. His request for anonymity cast Felipe as a secretsharer, twice over—once as an informant, sharing "secrets" of his community, and a second time as a partner in a clandestine relationship, one who shared the secret of his information-giving with an anthropologist.

Goldfrank's revelation of Felipe's identity in *The Artist of "Isleta Paintings"* sought to reframe Felipe's work and actions as an expression of personality constituted by Isleta's distinctive traits. She cast Felipe as an artist and a rebel (as well as, at other times, an outwardly conforming traditionalist), a constellation of personality traits that turned on and off in certain situations, depending on the cultural actors with whom he engaged. The trouble was, Felipe's

“personality” expressed fear of exposure, and he never consented to Goldfrank’s extended, posthumous form of personality inquiry.

Works such as *Isleta Paintings* and *The Artist of...* fomented various perspectives on ethnographic publication. For Isletans, the issue of publication lay with Parsons’s original volume, and with the secretsharer Felipe’s collaboration with an anthropologist to “spill the beans.” By contrast, Goldfrank’s publication can be understood as much more troublesome within the contemporaneous anthropological milieu because it signaled that the bond instantiated by the secretsharing relationship, between anthropologist and informant, was no longer holy, that new interpretative modes could override prior agreements that bonded anthropologists to their informants.

Felipe’s case presents an interesting challenge to engage and enliven people whom history has too-often forgotten or omitted. Sometimes there are things that are best left unshared, unaffected by recording or documentation or transmission. This is, of course, a somewhat facetious point; if we “forget” injustices then we return to whiggish history, or worse. But it is a reminder that not only is total information about people and the past is not only an elusive goal, but also a potentially troubling one. What if there are things that should not be recorded for posterity?

Conclusion

“Wonderfullest things are ever the unmentionable;
deep memories yield no epitaphs.”
—Herman Melville, *Moby-Dick*

Some things are best left unsaid; others, best left unwritten. For a developing American human science like anthropology in the late nineteenth century, these sentiments were counter to the goals of disciplinary development. Public disclosure of information added value to anthropology, even if the community in question did not wish to have such information published. American social scientists presumed a license to capture data on private or sensitive topics from marginalized communities as an unproblematic addition to scientific knowledge. But these “contributions to knowledge” were not unproblematic, in part because of the effect of documentary technologies on contextualized, stewarded knowledge and in part because of the sensitive, private nature of such knowledge in the first place. The stewards of Pueblo and Navajo knowledge, as this dissertation has shown, had disparate reactions to the attempted materialization of ceremonial knowledge, an effect of the different contours and maintenance systems in place in their respective epistemological systems. Yet, for both groups, materialization of knowledge risked its misuse outside of the care of a knowledge steward.

Anthropologists of the late nineteenth and early twentieth century recognized these concerns over knowledge stewardship but did not take them seriously. The impetus to document and an assumption that Pueblo and Navajo fears about knowledge disclosure were unfounded overrode indigenous concerns. A logic of data accumulation dominated anthropology in the middle of the nineteenth century, as fieldworkers and researchers worked to professionalize the science. An impetus toward exhaustive data accumulation found ground in the generalized and systematic tabular data accumulation directed by the Smithsonian Institution and other early ethnological scholars. As shown in Chapter 1, the paper technology of the ethnographic circular of schedules established foundational questions for anthropological scholarship, but as data piled up from unique Indian communities across America, fieldworkers came to see the diversity of

American Indian communities and the limits of standardized data collection for documenting the unique qualities of these groups. Ethnographers in the late nineteenth century began to emphasize detailed note-taking in the field, a strategy that was often used to depict cultural elements such as spiritual systems that were unique to a given community. As belief in universal developmental laws fell away, Americanist ethnography instead sought to understand a culture's inner logics, their modes of instruction, justification, and "opinions" (what John Wesley Powell deemed their "sophiological" system). The focus on the "interior" of a culture group meant that the quest to understand an indigenous community became even more obsessed with the minutiae of cultural practices, particularly ceremonialism. In the Southwest, the logic of accumulation and the desire for ethnographic novelty continued apace, driven by the popularity of the region as a place to practice ethnographic fieldwork. In time, to obtain new information, Southwesternist anthropologists frequently resorted to tactics, such as surreptitious documentation and clandestine interviews, that undermined the integrity of knowledge stewardship as understood by the communities they studied.

The effects of surreptitious documentation and clandestine interviews were conditioned by the social system of knowledge in place in diverse Indian communities and in turn had different effects on subject populations. The case study of Navajo singers in the 1880s and 1890s presented in Chapter 2, for example, indicates that ethnographer-anthropologist relationships could engage in cooperative documentary practices that were not seen to radically disrupt ceremonial integrity. Washington Matthews developed strong relationships with informants and sought to convey his respect for Navajo ceremonialism and the learned tradition of medicine singers in his ethnographies. Though he did sometimes document without singer consent, his overall comportment and actions likely helped persuade Navajo singers such as Laughing Singer and Tall Chanter to accept some level of inscription of ceremonial practices. But Laughing Singer and Tall Chanter had become powerful figures in a knowledge system that valued hierarchy, specialist training, and experience, and which endowed knowledge stewards with a measure of freedom to decide to whom they passed on their craft. In the end, Matthews could do ethnography because his chosen topic and necessary interlocutors fit within the contours of acceptability for Navajo communities.¹

¹ It could further be specified that these contours of acceptability were present for Matthews in his locale, in this time period, and distinctive to the singers with whom he engaged. Not all medicine singers of the late nineteenth century agreed with the

The ethnographic documentation of Pueblo ceremonialism, as shown in the Pueblo case studies, was a different matter entirely. In general, Pueblo communities regulated ethnographic documentation collectively, within the bounds of their compartmentalized knowledge system. Social societies and cliques stewarded and managed access to specialized ceremonial knowledge not only from ethnographers but also their community members; public knowledge of the existence of secrets (but not their contents) was understood by anthropologists and an individual pueblo's population alike. Given these social strictures on the sharing of stewarded knowledge, when sensitive information was materialized by anthropologists and circulated beyond the pueblo, the revelation of secretsharing eroded internal trust for certain knowledge stewards and exacerbated factional disputes around the role of Anglo influences (such as schooling) in the community.

The impetus to find new areas of Pueblo life to study increasingly put anthropology at odds with many of their subject community's aims. Because ethnography of the Pueblo ecumene had been a well-trod field for budding anthropologists, by the early twentieth century Southwesternists resorted to enticing potential Pueblo informants with material goods and off-site hospitality to encourage the extraction of unique information. A dynamic of "secretsharing" emerged, a bond that protected the clandestine exchange of sensitive community information for access to Anglo goods and services.

For anthropologists of the 1910s and 1920s, the secretsharer relationship served greater disciplinary and philanthropic goals. Having eschewed notions of comparative cultural hierarchies, Anglo anthropologists understood cultural documentation as a culturally uplifting, not diminishing, action. Learning about people, for Elsie Clews Parsons and other ethnographers of her generation, could push forward intelligent and accurate portrayals of often-maligned and persistently stereotyped Indian groups. On the whole, early twentieth-century Anglo ethnographers such as Parsons and White did not extract sensitive information from Indian groups for callous reasons. They did, however, extract information to grow their discipline, and often did so without a holistic reflection on how information extraction would affect the community in question.

permission given by Laughing Singer and Tall Chanter. But the case study conforms to a tendency toward chantway materialization in Navajoland after the turn of the century, especially through the influence of Hosteen Klah (Mr. Left Handed). Someday, I will write about this. Until then, see Faris, *The Nightway*. For Hosteen Klah, see Newcomb, *Hosteen Klah*.

But the accumulative, universalistic logic of novel information that set the stage for secretsharer relationships had unforeseen consequences. Because Pueblo distribution of knowledge maintenance meant that knowledge was shared mutually by select groups, secretsharing came to be seen as a violation of community trust. Such a community sentiment prevented many Puebloan people from sharing sensitive knowledge. But this “fear” could be overcome under the right circumstances, anthropologists learned: Those informants most socially and economically precarious or down on their luck could be mined by ethnographers for their intimate knowledge. Perhaps Anglo estimations of Isleta’s vibrant ceremonialism were increased by a work such as *Isleta Paintings*; but for Isletans of the time, the revelation of ceremonial intimacies to outsiders lessened its meaningful effects within the community.

The cultural disintegrating effects of unsanctioned historical ethnographic data accumulation still resonate today. Who is the rightful steward of localized, sensitive community knowledge in a globalizing world? The 2015 republication of the Acoma Pueblo tale of origin—a story not meant to be shared outside of the community—exemplifies the complexities of such a question. Though instances of Acoma’s origin story were published as early as 1946, Penguin Books republished a revised edition in 2015. This recent publication included the original text from 1946, attributed to Edward Proctor Hunt, supplemented with explanatory footnotes and a new introduction by Peter Nabokov.²

Edward Proctor Hunt, also known as Day Break, was an Acoma Indian born in 1861. He was sent to Carlisle Indian School, where he became a devout Christian (there, he also obtained his Anglo name). When Hunt returned to Acoma he opened a store on the mesa-top. We met Hunt as the storekeeper in Acoma in Chapter 4, where he proved friendly to Elsie Clews Parsons but was reticent about opening sharing information with her. While Parsons described Hunt as “secular,” he had clashed with Acoma spiritualists over his Christian faith, in time decamping with his family for Santa Ana, a pueblo near Albuquerque, New Mexico. After more trouble over religious affiliation, the Hunt family departed Santa Ana, too. Later, they joined a “wild west”

² For the Acoma perspective (represented by its Governor, Fred Vallo Sr.), see Fred S. Vallo Sr., “New ‘Origin’ publication is affront to Acoma,” *Santa Fe New Mexican*, September 23, 2015. See also, Alex Jacobs, “Don’t Buy This Book! Acoma Pueblo vs Peter Nabokov: When the Sacred is Made Profane,” *Indian Country Today*, February 11, 2016, <https://indiancountrymedianetwork.com/news/dont-buy-this-book-acoma-pueblo-vs-peter-nabokov-when-the-sacred-is-made-profane/>. An excellent summary and analysis of the issue can be found in Khristaan D. Villela, “Controversy erupts over Peter Nabokov’s publication of ‘The Origin Myth of Acoma Pueblo,’” *Pasatiempo*, January 15, 2016.

show, which landed them in Dresden, Germany, for a season, where Edward Hunt played Chief Big Snake, a Plains Indian caricature.

The Hunts returned to the United States in 1928, where they were hired as paid informants by the Bureau of American Ethnology. Along with his three sons, Hunt shared information about Acoma and Puebloan beliefs, including over seventy ritual songs and several drawings of spiritual paraphernalia. In their time as informants, Edward Hunt and his sons related the Acoma origin story to BAE ethnographers. These interviews were recorded on wax cylinders, but sat for a time in an archive. *The Origin Myth of Acoma and Other Records* was finally published nearly twenty years later, in 1946. The publication of the story frustrated many Acomans; since the time of publication, copies of the book have been purchased by Acoma in attempt to remove it from circulation.

Given his life story, what ground did Hunt have to relate the origin story of Acoma, both his place of birth and a community from which he was estranged? As shown in his interaction with Parsons in Chapter 4, Hunt understood that sharing information with outsiders in Acoma was not allowed. But anthropologists in the Bureau of American Ethnology desired a “traditional” tale, something viewed as authentic. Under anthropology’s logic of documentation of the late 1920s and early 1930s, Hunt surely seemed a viable informant, regardless of his personal conflicts with his home community.

Incidentally, Hunt’s own life story is perhaps a better ethnographic document, for it shows him as a globe-trotting Indian who wore many hats. Peter Nabokov, a professor of American Indian Studies, took up the Hunt family story in the 1990s. Through research and interviews with surviving family members, Nabokov produced a modern-day “life history,” *How the World Moves: The Odyssey of an American Indian Family*. This story of a secretsharer shows the trials and tribulations of an Indian in a rapidly changing world. Nabokov, however, complicated his contribution to historical ethnography by resurrecting the contents of Hunt’s secretsharing in the 1920s. During his research on the Hunts, Nabokov and Penguin Books decided to republish the Acoma origin “myth” as told by Edward Hunt. By republishing the book, Nabokov hoped to locate Acoma’s spiritual tradition alongside similar tales of the great religions of the world, a move conceptualized as an honoring of Acoma and its rich cultural

traditions. This edited reprint was published, in tandem with the book on the Hunt family, in 2015 as *The Origin Myth of Acoma Pueblo*.³

Acoma Pueblo's leadership contested Penguin Books' legal right to publish the Acoma origin story, arguing that this version of the tale was obtained in an unsanctioned meeting between anthropologists and an informant who had left the community.⁴ While Hunt certainly knew a version of the origin story, he was not a sanctioned knowledge steward, one who could care for it as a precious piece of Acoma history. Acomans had resisted previous efforts by anthropologists to learn their origin tale, not to mention other private aspects of their culture, as depicted throughout this dissertation. Acoma's frustration extended to Nabokov, who some perceive as having appropriated the story for his professional benefit (deeming the story a "myth" was also perceived as a slight to Acoma history). While the profit from sales of the book were also an issue, Acoma leaders concerned themselves primarily with the perception of the authoritativeness of Hunt's account, over the living historical tradition of oral knowledge-sharing and story-telling as culturally enlivening. Stewarding knowledge within the community kept the community together.

When an "origin story," healing ceremonial, or spiritual practice is extracted from an indigenous community, who is it that becomes the steward of that knowledge?⁵ From the perspective of the Acoma, the interwoven lessons of history and the spiritual and cultural meanings conveyed in a story's telling, as well as the meaningful power of internal knowledge stewardship, are lost or dissolved by large-scale publication. Extended footnotes cannot replace the oral and contextual aspects of knowledge conveyance. The 2015 publication diminished Acoma's ability to tell their own story as a living history, as the power of materialized information extends and overpowers the individual flair of the storyteller and his or her personal interpretations. The issue, indeed, is one of education: the book is a record that an Acoma person could reference and learn something from, but it is also a document disconnected from the

³ An account of how Nabokov came to the Hunt material and the decision to publish the origin story can be found in Simon Worrall, "Custer to Casinos: One Native American Family's Story," *National Geographic*, September 23, 2015.

⁴ Vallo, "New 'Origin' publication is affront to Acoma." Acoma stated it had no position on the publication of the Edward Hunt biography by Nabokov.

⁵ Native scholars continue to debate these topics within and outside of their communities. For a public discussion of knowledge stewardship and a critique of anthropological documentation. Joseph Aguilar, Diane Reyna, Brian Vallo, and Bruce Bernstein, "Uncovering/Recovering History," Symposium panel, School for Advanced Research, Santa Fe, New Mexico (video published 23 March 2015). On Pueblo knowledge practices in the recent past, see Suina, "Pueblo Secrecy"; Pandey, "Anthropologist at Zuni."

contextualized knowledge stewardship, a vibrant tradition of personalized instruction through storytelling. Acoma seeks to maintain the ineffable and experiential facets of cultural education. These ideals, as this work has shown, have long clashed with documentation and with the standards of Anglo-American knowledge production.

And yet, the preservation of indigenous traditions and stories is a constitutive part of anthropology, and the accumulated archive of ethnographic information has served as an important resource for many indigenous communities. From the perspective of the Bureau of American Ethnology, and perhaps even Hunt, telling the Acoma origin story was a form of cultural preservation. How do outsiders respect a community's autonomous stewardship of its own heritage while also maintaining records of the past as a resource for future generations? This question is not easy to answer. The fact that the publication of the Acoma origin story is controversial indicates that we are still living in a world where meaning is located in places, within the hearts and minds of peoples, and that to divorce information from its context can still cause heartbreak and anxiety over cultural disintegration. We should take this as both a positive sign of the diversity of knowledge practices but also recognize the rarity of this example of endangered knowledge. What we must come to terms with, then, is something antithetical to the documentary impulse of late nineteenth and early twentieth century anthropology: that we cannot know all instances of contextual, culturally meaningful knowledge. Not everything that is meaningful is for everyone.

Human relationships are powerful connections that can be severed or aggravated by the free flow of information. The relationship of secretsharers, while predicated on the exchange of information, were complicated linkages of people from different milieux. In the case of the Navajo singers and Washington Matthews, the preservation of cultural practices occurred with relatively few violations of propriety. In general, however, because of the power-differentiation between anthropologists and indigenous informants, the cross-cultural knowledge exchange relationships of anthropologists and their informants had disproportionate possibilities for negative effects on indigenous communities, without an equal risk to the Anglo world.

Many Pueblo (and to a different extent, Navajo) people felt strongly about the contained nature of some forms of knowledge and had reason to keep it to themselves; these boundaries to knowledge were embedded in their systems of social comportment. "Secrets," in other words,

were important constituent parts of Pueblo life. I have sought to identify some personal negotiations around the “secret,” around sensitive objects of knowledge that are differentially valued by Indian communities, by individuals within those communities, and by anthropologists. The web of connections that ethnographers spun in order to document and disclose secrets proved to have consequences for Indian communities in the Southwest. Bit by bit, the extraction of knowledge from Pueblo communities complicated their cohesive effect, and ethnographic documentary practices had no small role in this. Across the Pueblo ecumene, documentation and secretsharing ultimately betrayed something essential, something constitutive even if ineffable to outsiders. Deep memories yield no epitaphs.

Images

Image 1 – George Gibbs’s Schedule. George Gibbs, *Instructions for Research Relative to the Ethnology and Philology of America: Prepared for the Smithsonian Institution* (Smithsonian Institution, 1863).

24	PHILOLOGY.	
	ENGLISH.	SPANISH.
	<i>Name of tribe.</i>	<i>Nombre de la tribu.</i>
63	tobacco	63 tabaco
64	sky	64 cielo
65	sun	65 sol
66	moon	66 luna
67	star	67 estrella
68	day	68 dia
69	night	69 noche
70	morning	70 mañana
71	evening	71 tarde
72	spring	72 primavera
73	summer	73 verano
74	autumn	74 otoño
75	winter	75 invierno
76	wind	76 viento
77	thunder	77 trueno
78	lightning	78 relámpago
79	rain	79 lluvia
80	snow	80 nieve
81	fire	81 fuego
82	water	82 agua
83	ice	83 hielo
84	earth, land	84 tierra
85	sea	85 mar
86	river	86 rio
87	lake	87 lago
88	valley	88 valle
89	prairie	89 llano
90	hill, mountain	90 cerro, montaña
91	island	91 isla
92	stone, rock	92 piedra, roca
93	salt	93 sal
94	iron	94 hierro
95	forest	95 bosque, selva

Image 2 – Examples of Morgan’s Schedule Blanks. Lewis Henry Morgan, *Circular Letter in Regard to the Possibility of Identifying the Systems of Consanguinity of the North American Indians with That of Certain Peoples of Asia* (1859).

Degrees of Relationship in the Language of the Nation.

MADE BY (Name) (Residence) (Date) 1860.

VOWEL SOUNDS.—ü, as in art; ũ, as in at; ẽ, as in met; ĩ, as in it; õ, as in got; ů, as oo in food.

Please mark the accented syllables.

INSERT NATIVE PRONOUNS—MY, OUR, HIS

Description of Relationship.	Name, or Native Word, in English Letters.	Translation of the same into English.
1. My Father.....		
2. " Mother.....		
3. " Son.....		
4. " Daughter.....		
5. " Grand-Son.....		
6. " Grand-Daughter.....		
7. " Great-Grand-Son.....		
8. " " Daughter.....		

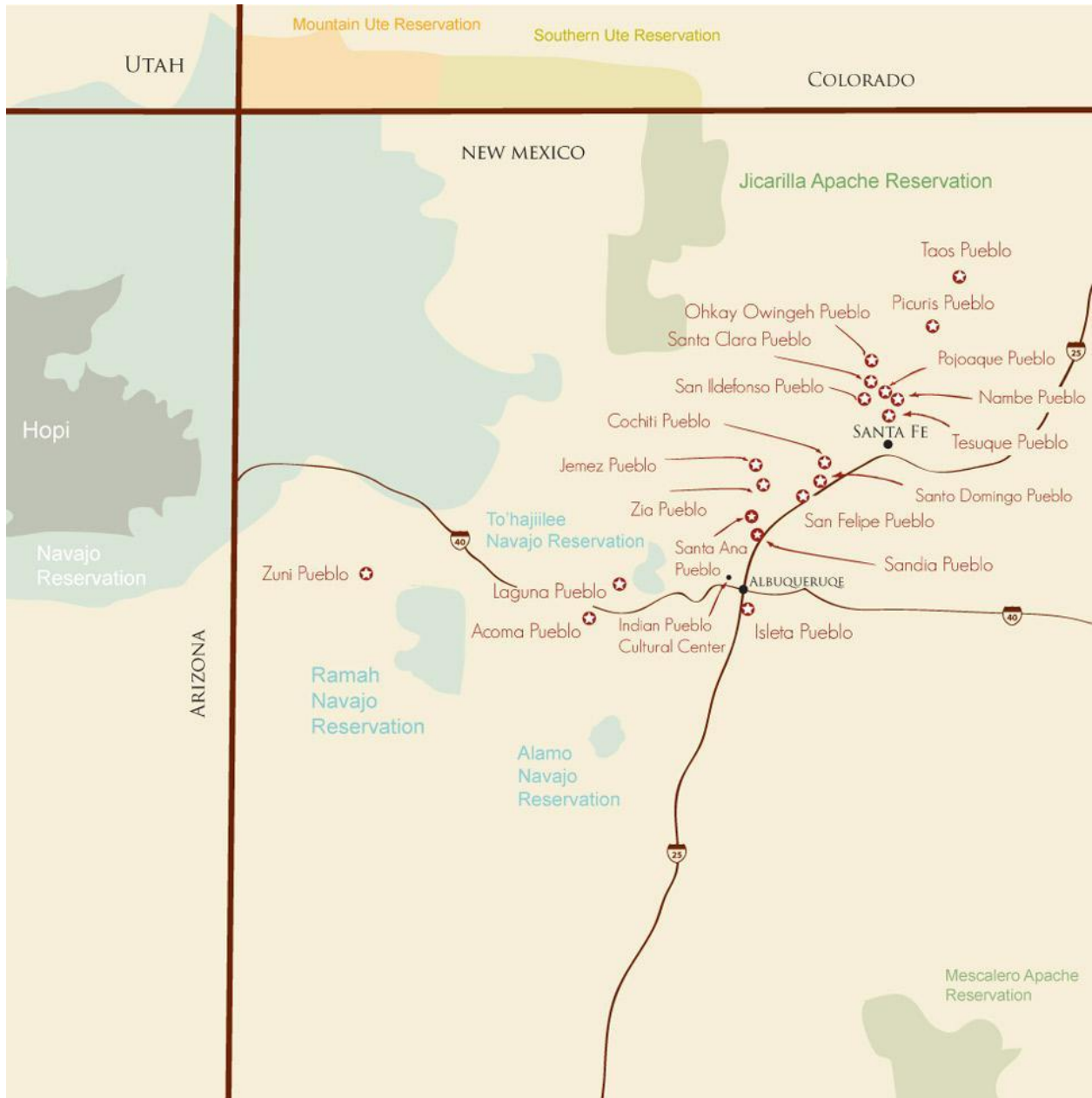
16

DEGREES OF RELATIONSHIP—Continued.

Description of Relationship.	Name, or Native Word, in English Letters.	Translation of the same into English.
84. My Father's Brother's Great-Grand-Da'ter, (said by a Female).....		
85. " " " Gr't-Gr't-Grand-Son.....		
86. " " " Gr't-Gr't-Grand-D'r.....		
87. My Father's Sister's Son, (said by a Male).....		
88. " " " " (said by a Female).....		
89. " " " Son's Wife, (said by a Male).....		
90. " " " Son's Wife, (said by a Female).....		
91. " " " Da'ter, (said by a Male).....		
92. " " " Da'ter, (s'd by a Female).....		
93. " " " Daughter's Husband, (said by a Male).....		
94. " " " Daughter's Husband, (said by a Female).....		

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Image 3 – Pueblo Ecumene Map. Image reproduced from Shumakolowa Native Arts, Indian Pueblo Cultural Center.



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Elsie Clews Parsons Papers, American Philosophical Society, Philadelphia, Pennsylvania.
John Gregory Bourke Papers (University Microfilms, Ann Arbor, Michigan)
Leslie White Papers, Bentley Historical Library, University of Michigan.
Lewis Henry Morgan Collection, University of Rochester Special Collections.
Washington Matthews Papers, Wheelwright Museum Archives, Santa Fe, New Mexico.

Abbreviations for Archives Cited in Footnotes

APS — American Philosophical Society, Philadelphia, Pennsylvania.
BHL — Bentley Historical Library, University of Michigan.
FACHL — Fray Angélico Chávez History Library, Santa Fe, New Mexico.
HLSC — Huntington Library Special Collections, San Marino, California.
NAA — National Anthropological Archives, Washington, D.C.
SIA — Smithsonian Institution Archives, Washington, D.C.
SMBRL — Southwest Museum, Braun Research Library, Los Angeles, California.
URSC — University of Rochester Special Collections, Rochester, New York.
WMA — Wheelwright Museum Archives, Santa Fe, New Mexico.

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