

FEMINIST ANTHROPOLOGISTS and the  
CONSTRUCTION of the AMERICAN SOUTHWEST

SCIENTISTS & STORYTELLERS

Catherine J. Lavender

# Scientists and Storytellers



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CATHERINE J. LAVENDER

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# Contents

Acknowledgments	vii
Introduction	i
1. Taking the Field: The Social Context of Southwestern Ethnography	20
2. Present at the Creation	49
3. The Poetic Professor	71
4. Listening Daughters	87
5. Executive Females and Matriarchs	119
6. "Is She Not a Man?"	138
7. Making It New by Making It Old	163
8. Strands of Knowledge	183
Notes	189
Selected Bibliography	217
Index	243



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# Introduction

People's folk tales are . . . their autobiography and the clearest mirror of their life.

—Ruth Benedict (1931)<sup>1</sup>

In the first decades of the twentieth century, American culture underwent violent transformation. In addition to the very real political and geographical changes wrought by the First World War, the emergence of modernism in art, culture, letters, and philosophy created a chaotic world for intellectuals. Seeking ways to make sense of the new world in which they found themselves, many looked to other cultures—especially those defined in “the West” as “primitive”—for answers. For many in the United States, especially anthropologists, Native American cultures seemed ideal places to find new traditions.

In essence, it is as if these early anthropologists looked through a window at Native American cultures, fully believing that what they saw through that window were the Native American activities on the other side of the glass. In fact what they saw through the glass were Native American activities as well as their own reflections. What they recorded in their texts, however, did not always distinguish the difference between Native Americans' actions and anthropologists' reflections.

Anthropologists attempted to make sense of issues that concerned them in their own culture by researching Southwestern Native Americans. Drawing from the issues they felt most vital in their own

culture, they determined the foci of their research. Those who craved peace looked to Indian cultures to examine war and forms of mediation that seemed to have eluded Europeans. Those who desired greater social justice sought in tribal cultures new ways for people to interact. To early anthropologists, Euro-American culture seemed fragmented and divided. The Victorian culture against which they rebelled dwelt on dichotomies that provided sets of conflicting opposites—such as good/evil, right/wrong, and even us/them—to define the world. For a significant community of women scholars who focused their research on the Native American cultures of the American Southwest, these dichotomies seemed especially clear regarding gender (male versus female), sexuality (heterosexual versus homosexual, “normal” versus “abnormal”), and nature (natural versus made, “primitive” versus “civilized”). These scholars—including Elsie Clews Parsons, Ruth Benedict, Gladys Reichard, and Ruth Underhill—created a feminist ethnography of the region that emphasized the roles of women in Southwestern societies in determining the norms of each culture. Southwestern Indians provided case studies of differing social and gender systems through which to build on a feminist critique of patriarchy. To do so they focused on ways that patriarchy had come to define limited gender and sexual roles and identities, and “unnatural” ways of being. In contrast, this community of feminist anthropologists saw Native American cultures as providing for multiplicities of genders and sexualities, and for a more “honest and natural” way of being.

As part of their critique of patriarchy, this community of feminist ethnographers set out to illustrate the ways in which women were united across cultures. While their experiences would be determined by cultural variations, these feminist anthropologists built an argument that social structures—such as patriarchy, patrilineality, matrilineality—acted in roughly the same ways on women across cultural differences. Thus, in theory, the struggles of a Tohono O’Odham woman against patriarchal control would follow roughly the same contours of—and provide lessons for—an equivalent struggle by an urban, Anglo-American woman. Women’s experiences within a more egalitarian or matrilineal society would shed light on the ways in which a women’s lack of power would shape women’s experiences in a patriarchal culture. The feminism these scholars developed through

both their ethnological writing and other writings (in fiction, personal writing, and sociological writing, for example) was both historically situated and highly intellectualized.

J. Stanley Lemons, in *The Woman Citizen: Social Feminism in the 1920s*, distinguishes between “hard-core feminists,” who “put women’s rights and women’s emancipation above all other considerations,” and “social feminists,” who also wanted emancipation but tended to subordinate this to social reform.<sup>2</sup> Since the kinds of activities that Lemon defines as “hard-core” tended to be defined as women claiming their rights in public, and what he defined as “social feminism” was a more private movement, historians perceived that feminism as a political movement had died out after the passage of the Nineteenth Amendment. Lemons’s distinction has all but defined studies of feminism in the 1920s. However, Nancy Cott, in *The Grounding of Modern Feminism*, challenges Lemons’s assumptions that the feminism he defined as “hard-core”—which came to be considered by the public to be *the* Feminist movement—had lost its support until it reemerged with the Second Wave.<sup>3</sup> Cott argues that such “hard-core” feminism continued in the struggle for the ERA, for example. I would go further, and say that the distinction between “hard-core” and “social feminism” is a largely artificial one, and—as we learn more about the lives and thought of feminist women during the period—decreasingly useful. Many feminists chose combinations of the two stances at different periods in their lives. Charlotte Perkins Gilman, to choose a single example, advocated sweeping socioeconomic changes while opposing suffrage, and also argued against birth control in Margaret Sanger’s own newspaper, *Rebel Woman*.<sup>4</sup> She left the feminist group Heterodoxy, offended by the pacifism of several of its members during World War I. Later in her life, however, she supported Sanger and the birth control movement. Other women prove equally difficult to categorize as either “hard-core” or “social” feminists. In this sense, then, the definition I use for feminism as a movement in American culture conforms to Gerda Lerner’s germinal statement that feminism as a broad term embraces many aspects of women’s emancipation. “Women’s emancipation” in turn means the elimination of “oppressive restrictions imposed by sex.”<sup>5</sup>

As First Wave feminists, feminist ethnographers emphasized women’s emancipation, meaning women’s rights to determine their

own lives by seizing control of their bodies, labors, and identities. This emancipation might be broadly and variously defined, but usually emancipation included women's control over their choices of spouse and the contours of their marriages, the rights to divorce, sexual expression, and motherhood, and social and political equality through self-reliance. Further, First Wave feminists struggled without resolution over the issue of women's equality to men versus women's distinctiveness from them; many First Wave feminists took both sides in the debate. Looking at the lives of Southwestern Native American women, feminist ethnographers found examples of women who they believed functioned as leaders in their communities, as economic forces in their own right, and as matriarchs in matrilineal societies. In matrilineal societies, feminist ethnographers found arguments for women's increased economic roles. In patriarchal societies, feminist ethnographers found analogies to their own experiences in both a male-dominated society and a male-dominated profession. From these views, they constructed an identity for Southwestern Native American women that sometimes differed sharply from the stories that their Native American informants told them about themselves.

Feminist ethnographers, for example, constructed an "executive woman" identity for some of their Native American informants. They thus emphasized ways in which these informants functioned as equivalents to feminist executive women in the ethnographers' own society. Executive Native American women, ethnographers argued, struggled with the same issues as did career women in, say, New York City—responsibility for childrearing, keeping the family together—but, at least in the case of Navajo and Pueblo women, they did so in the context of a society that was more egalitarian than Euro-American culture. Thus, feminist ethnographers presented the success of executive women in societies that did not limit them as a critique of patriarchy.

Feminist ethnographic writing about the role of the man-woman provides a concrete example of the ways that this community of feminist anthropologists constructed the man-woman role as a Native American manner of resolving gender and sexual dichotomies. Almost all North American Native American cultures included a man-woman role at some point in their history, and feminists, anthropologists especially, found man-women fascinating. Neither male nor female,

morphologically male man-women occupied a male sexual role and a female gender role. The man-woman's engagement in sexual and gender activities that Victorians would have labeled as deviant often carried no social penalties for man-women—or so informants claimed. In fact, as they illustrated, many Native American cultures valued and revered the man-women. At the same time, man-women served as respected conduits between the sacred and mundane worlds. In Zuñi theology, for example, the man-women became gods in bird form during the *Sha'alako* dances. In this ceremony, the man-women represented a link between the female land and the male sky as well as a link between the spirits and the living. Finally, the man-women represented a resolution of the Victorian split between viewing nature aesthetically and using nature economically that characterized the nature/civilization dichotomy in Euro-American culture. By engaging in female activities on the land—such as farming and gathering—while maintaining their male sexual activities—such as engaging in reproductive and nonreproductive sex with women and men respectively—man-women bridged the separations between production and a more aesthetic view of nature. The Native American informants who provided feminist ethnographers with information about the man-woman did not generally see the man-woman's ability to bridge genders, sexualities, relationships with nature, and sacred and mundane roles as anything odd; feminist anthropologists certainly did. Discussions of the man-woman in their ethnographic texts reveal the extent to which feminist ethnographers saw the role as a possible curative for Euro-American culture.

Ethnographic writing about the man-woman illustrates the lengths to which feminist ethnographers went to restructure Native American lifeways in order to make sense of their own lives. Discussing man-women gave feminist ethnographers the occasion to discuss their own anxieties about gender and sexuality in a changing society. Elsie Clews Parsons and Ruth Underhill both wrote extensively of their own anxieties about their gender identity in their own culture, which deemed them as “manly” by virtue of their feminism. Ruth Benedict's possible bisexuality, unexplored in her public writings, underscored the significance to her of examining such cultural practices as differentiating among people by their sexual identities, and used her discussion of the man-woman to make a plea for understanding of a sexuality often

referred to at the time as “deviance” or “inversion.” Gladys Reichard, who struggled to assert her own identity after an extended apprenticeship under Boas cast her in the role of a surrogate daughter, and who chose to remain single throughout her lifetime, emphasized the ways in which man-women were exemplars of independence and self-reliance. Even as each of the scholars examined here had differing reasons for addressing the role of the man-woman, they shared a common interpretation of the role as a sexual identity. All linked the male man-woman role to transvestitism and homosexuality in her own culture, usually commenting that they wished their friends at home could have such “healthy” attitudes toward sexual and gender variations. They had experienced firsthand their own culture’s fear of gender ambiguity, and through their own sexualities or the experiences of gay and bisexual friends they had seen the havoc that their own culture’s fear of sexual diversity could wreak in one’s life. Because of these concerns about their own culture’s fears, this group of feminist ethnographers constructed an idealized transvestite homosexual identity for man-women that differs markedly from their Native American informants’ perceptions of the role as a gender, and not a sexual, identity.

To illustrate feminist ethnographers’ attempts to criticize and improve their own culture by comparing it to Native American cultures, I examine texts written by women ethnographers who conducted their research in the American Southwest from the end of the nineteenth century to 1940. Early twentieth-century ethnographic texts resulted from the interplay among anthropologists’ own cultural concerns, their experiences in the field with Native Americans, and the testimony of the anthropologists’ Native American informants. The texts that emerged from this research reveal much about the anthropologists’ concerns and cultural ideas about these categories in their own lives. In order to extract information about how the texts reflect these anthropologists’ concerns, I combine textual analysis with comparison of the texts to other writings produced by the scholars, correspondence between the women, and, where available, the anthropologists’ unpublished field notes.

Anthropologists’ own writings—journals, correspondence, nonethnographic writings, and field notes—reveal that ethnographers altered their texts in order to appeal to several audiences, including their peer

ethnographers and a public readership. Often, anthropologists' texts and other personal or nonethnographic writings directly contradict one another. Comparing unpublished sources—correspondence from the field, personal journals and diaries, and field notes—to published works reveals a wealth of information about the nature of fieldwork. More interestingly, these contrasts reveal how ethnographers sometimes changed their analyses to fit their own, as well as their colleagues', expectations about the broader meaning of their discrete studies. Informants' testimony as it appears in field notes and as quoted in journals or letters to other ethnographers often contradicts what is attributed to them in published ethnographies; or, more commonly, only parts of their testimony see print. Which parts get published and which do not often indicate the thought processes of feminist ethnographers. While ethnographers often recorded informants' statements verbatim in their field notes, during the process of transforming the specific statements into cultural generalities ethnographers found what they termed "internal contradictions" in Native American narratives. Ethnographers then stated that these contradictions—which mainly contradicted ethnographers' expectations of what a "primitive" person would understand about their world—resulted from anomalies in the individual informant's experiences, rather than representing anything about the culture at large, and therefore could be ignored. In this way, for example, Ruth Underhill could dismiss as anomalous the interactions of her Tohono O'Odham informant, María Chona, with Mexican traders and the complex market economy of early twentieth-century Tucson; while María Chona told stories about shopping in stores, using commercially produced flour and cloth, speaking Spanish on the reservation, and working for wages in Tucson for white employers, Underhill referred to her as a representative of the "primitive" hunting and gathering culture of the "peaceful Papago," unchanged since Cortés set foot in Mexico. Comparing the anthropologists' published findings with their findings in progress reveals something about the creative process involved in anthropologists' construction of these ethnographic texts.

The history of anthropology provides one avenue for such critical readings. Many of the studies of anthropology that examine the contingency of meaning in ethnographic writing have addressed European anthropology, as in the work of anthropologists George W. Stocking, Jr.,



Henrika Kuklick, and James Clifford.<sup>6</sup> More importantly for this study, however, historians of anthropology have begun to periodize and historicize ethnography to show ethnographic texts as products of their times. For example, George Stocking asserts that the 1920s were a period of massive realignment in Victorian sensibilities, marked by the emergence of “modernism,” especially within the academy and academic anthropology.<sup>7</sup>

Several outstanding overviews of ethnographic research during earlier eras shed light on the background to the period from the 1890s to the 1940s. Robert E. Bieder covers the period before the professionalization of anthropology, and Curtis M. Hinsley, Jr., addresses the significance of the Bureau of American Ethnology in this earlier period.<sup>8</sup> Keith H. Basso provides the only overview of Southwestern ethnography in his 1979 essay, “History of Ethnological Research.”<sup>9</sup> The absence of more in-depth overviews like Basso’s is all the more surprising because the period from the end of the nineteenth century through World War II was a crucial time in research about and the formulation of policies toward Native Americans in the Southwest.

The history of anthropology has also included a subfield of ethnographic “restudies” of key interpretations made by earlier ethnographers; restudies typically consist of comparing new ethnographic fieldwork—focusing on indigenous reactions to the ethnographers and the ethnographic texts—to a previous study. Two of the most famous restudies are Oscar Lewis’s restudy of Robert Redfield’s research of Tepoztlán in Central Mexico and Derek Freeman’s controversial restudy of Margaret Mead’s research in Samoa.<sup>10</sup> This methodology of “restudying” ethnographic texts plays a crucial part in this study, with the comparison of published ethnographic texts to previous, pre-publication drafts and field notes, as well as to other ethnographic research of the period and more recent ethnographic research.

Patricia Albers and Beatrice Medicine address the impact of anthropological research on Native American women.<sup>11</sup> Both Albers and Medicine argue that anthropologists’ preconceptions have led them to write insultingly and inaccurately about Native American women. Medicine and Albers focus on the many errors in anthropological writings about Native American women. Neither, however, examine the roots for those errors in anthropologists’ preconceptions.

My goal in this study is not to provide an ideal ethnohistory of the Southwest. Instead, what I set out to argue here is that a distinctive dialogue about women developed in the works of Parsons, Benedict, Reichard, and Underhill, providing a significant critique of patriarchy. This critique indicated several important things. It presented the Southwest as a cultural laboratory for studying significant questions, like social structures, because it contained varied cultures—in this case, both patriarchies and more egalitarian cultures. It revealed the niches that some women scholars chose to fill, and the ways in which they used their research to fight for their own self-identities and independence. It added nuance to assumptions about anthropology as “a welcoming science,” indicating to which sections of the science women would be welcomed. In a more positive light, it also provided a site of interaction, an idiom in which feminist scholars could address questions of gender and sexual identity that concerned them in their own lives.

Feminist ethnographers challenged the assumption that patriarchy was a “natural” social structure. This was part of a broader agenda among women scientists of challenging Victorian modes of classification that dwelled on the inherent inferiority of females. Many of the restrictions on women’s options in the early twentieth century—as in the nineteenth—used science as their source of authority. Women should not study as men did, doctors such as Edward Clarke argued, because it would cause the malfunction of their reproductive systems and create hysteria.<sup>12</sup> As support for this, Clarke and others marshalled arguments that women were naturally different from men, and therefore by nature unfitted to pursuits—such as learning or rational thought—defined as male. Just as scientific racists equated racial difference to fundamental physical laws, scientific sexists argued that women’s very natures prevented them from taking an equal place in civilized society. Hence, much of the feminist critique of patriarchy in which these women took part aimed to explode assumptions about women’s natural capabilities by illustrating the ways in which women defied such classifications once cultural restrictions were removed.<sup>13</sup>

Further, to argue that something was natural was to imbue it with a positive aura. The “primitive”—defined as pure and unspoiled and therefore as natural—provided a curative for overcivilization.

To return to primitiveness was, for many modernists, a positive goal. These feminist ethnographers reveled in their chance to engage in “primitivity” as participants in Southwestern Native American lives, even if it was a temporary transformation and they would have to return to their apartments in New York City after a few short months. They rhapsodized about the landscape—few literary travelers to the Southwest would have failed to—but more importantly, they emphasized what they saw as the fundamental connections between women and the land.

This study examines the extent to which women ethnographers’ descriptions of, as well as reflections on, Native American gender, sexuality, and “primitive” identity also reflected the anthropologists’ own cultural views of these issues. It depends on a critical reading of ethnographic texts as compared to more personal writings including correspondence, journals, nonethnographic writings, and field notes. In many cases, this means looking at personal writings to see what was edited out as insignificant when the anthropologists chose information to include in their published studies. In some cases, this represents a removal of unclear or repetitive information. But, at times, the excision of information from the published versions represents a significant change in the meaning of the informant’s testimony. One example of this is the consistency with which feminist ethnographers changed informants’ references to man-women as “she” to “he.” Another is the dismissal of Spanish or Mexican practices as examples of cultural pollution, and the emphasis placed on presenting Southwestern cultures as “pristine” and free from any European or Mexican contact, even when the anthropologist apprenticed herself to help make a traditional item to sell to tourists at the Mexican market. Yet another example is the excision of references to “unnatural” partum taboos among the Pueblos, such as restrictions on pregnant women watching movies, or Pueblo dependence on white doctors when confronted with contingencies like rattlesnake bites. When this sort of difference exists between a published text and the informants’ testimony, there is something afoot; the differences reveal the preconceptions of the anthropologists.

The differences also reveal the limits of the texts as documents of informant testimony. Some texts—such as Ruth Underhill’s *Papago*

*Woman* and Gladys Reichard's *Navajo Shepherd and Weaver* and *Dezba, Woman of the Desert*—are openly autobiographical documents of an ethnographer's experiences among Native Americans. Other less openly autobiographical texts nonetheless reflect ethnographers' experiences and perspectives at the expense of informants' narratives; thus they serve as at least partially autobiographical writings about ethnographers' lives. Texts where field notes have been preserved, especially the works of Elsie Clews Parsons, indicate sharp differences between informant testimony and the final, published, version of the story. Internal contradictions within texts also point to tensions between informants and ethnographers over the story that the texts tell. Thus, ethnographers provide different emphases in the published narratives than their informants did in telling the stories themselves. Sometimes these disagreements and tensions survive in the final published texts, revealing the preconceptions of the ethnographers who wrote them.

Gretchen Bataille and Kathleen Mullen Sands have addressed this issue in detail in Native American women's narratives in *American Indian Women: Telling Their Lives*.<sup>14</sup> Bataille and Sands stress the differences between traditional European and traditional Native American narratives, especially what they identify as the Native American emphasis on collective experience and tribal solidarity as compared to the European emphasis on individual experience and originality. Bataille and Sands define Native American women's narrative as drawing upon two diverse pools of tradition: "the oral tradition of American Indian literature and the written tradition of Euro-American autobiography."<sup>15</sup> As a result, they argue, native women's narratives emerge as a unique genre. This genre is identified by the blending of the Native American tradition of telling of tribal experiences, but it also makes room for what Bataille and Sands refer to as "egocentric individualism," an emphasis on the teller's personal role in the broader tribal experience.<sup>16</sup>

Bataille and Sands imply that Native American women consciously blended these traditions in their own narratives. But it seems clear that, at least in the case of the ethnographic accounts of women's lives addressed here, the blending occurred not only within informants' consciousnesses but also in the interaction between themselves and ethnographers. In the translations of Native American women's testimony, into English and into modernist scientific idioms, Native American oral

traditions met and mixed with Euro-American traditions. Thus these texts, when subjected to textual analysis, provide a challenge to the “mixed tradition” definition of Native American women’s narratives. They also indicate the role that ethnographers’ preconceptions played in mixing those traditions.

In order to trace the ways in which these preconceptions result from ethnographers’ own experiences, I examine the life histories of each of the four anthropologists I have chosen. These biographical studies address not only these women ethnographers’ work in the field, but also the establishment and growth of an intellectual community of women anthropologists in the United States. These women ethnographers represent the emergence of anthropology as a professional discipline, and include students of Franz Boas at Columbia University (Gladys Reichard, Ruth Benedict, and Ruth Underhill [later of the Bureau of Indian Affairs]) as well as Boas’s close associates (Elsie Clews Parsons of the New School for Social Research). Looking at both professional and “paraprofessional” women anthropologists illustrates the professionalization of anthropology as a field, the emergence of academic ethnographic orthodoxy, and the implications of these changes in ethnographic texts. Establishing the intellectual and historical context for the field of anthropology during this period requires an examination of not only the state of ethnographic research in the Southwest, but also of the Native American contexts in which the ethnographic research took place. This study, then, examines the growing interest among women ethnographers in the meanings and categorization of gender identities and sexual orientations.

This study focuses on the lives and work of four women anthropologists, Elsie Clews Parsons, Ruth Benedict, Gladys Reichard, and Ruth Underhill. Each anthropologist represents a particular commitment to feminist scholarship. Their careers followed a variety of paths, indicating both the avenues available to women anthropologists and the effect of those career options on the development of their feminism. Further, their research careers span the period under examination. In addition, they represent significant parts of a community of women scholars who studied under Boas, and who shaped later generations of women anthropologists; Parsons trained Benedict, Parsons and Benedict trained Reichard, and Benedict and Reichard trained Underhill. Finally, their

work spans a significant range of Southwestern feminist ethnography. In terms of approaches, each was a pioneer of sorts—Parsons in folklore ethnography, Benedict in psychological ethnography, Reichard in ritual and participant ethnography, and Underhill in personal narrative ethnography. In terms of cultures addressed, they also cover a broad spectrum of Indian cultures, addressing Pueblo (Parsons and Benedict), Navajo (Benedict and Reichard), and Piman (Parsons and Underhill) populations in the region. All four scholars paid close attention to issues of gender and sexuality in their research; all professed to be feminists, approached their research with their feminism in mind, and wrote texts that provided critiques of patriarchy. All four worked extensively with female informants, and all four addressed issues of cross-gendered persons and both reproductive and nonreproductive sexuality in their research.

Biography is a significant part of this study because each of the women writing feminist ethnographies were, even while producing scientific texts, also writing autobiographies embedded within them. They chose for their main informants those with whom they felt the most comfortable. They looked in their informants' lives for moments of continuity between their own experiences and the informants, and broadened that search to make connections between modern Anglo-American women and the informants. In Elsie Clews Parsons's case, she chose as her informant a woman who served as an Indian analogue to herself—educated, political, and able to assist her husband by taking a public role herself. Ruth Benedict chose both male and female informants, but she focused her analysis on male informants and did not form close attachments to female informants. In part this resulted from the fact that she did not do a great deal of fieldwork, relying instead on the work of other scholars, most of whom were male, who interviewed mostly male informants. It may, however, have showed something more fundamental in Benedict's character, as the majority of her close attachments—with the notable exception of her attachment with Margaret Mead—were with men, and not other women. Gladys Reichard, who had difficulty forming relationships with others and commonly took the role of a daughter in her personal and professional life, chose to become a daughter to an executive woman. Her informants then became not only the mother-informant, but the

mother-informant's daughters, thus providing important evidence for Reichard being a daughter in Navajo culture. Ruth Underhill, older and divorced when she began her research, chose an independent and elder woman as her informant, and in writing that woman's autobiography she also wrote her own. While there have been biographies of Benedict and Parsons, and much shorter examinations of Reichard's life, and almost nothing written about the life of Ruth Underhill, the ways in which these four scholars represent the emergence of a feminist ethnography in the region have been passed over. Biographies of Benedict and Parsons do not examine the autobiographical nature of their texts, while studies of Reichard and Underhill have glossed over their feminism almost entirely.

The study then turns its focus on the ways in which these women wrote feminist critiques of patriarchy, examining their writing about gender roles like the "executive woman" and gender and sexual identities like the "man-woman." Finally, the study moves on to an examination of how feminist anthropologists came to construct primitive identity in their studies of Southwestern women and cultures, in part by obscuring the interaction between informants and the market economy. Running throughout the story is an analysis of how each of these questions both served to shape and reveal these feminist scholars' critiques of patriarchy, and further, how this critique of patriarchy came to dominate the ways they expressed their own cultural concerns in their texts.

This history is based on both published ethnographic texts and archival resources from each ethnographer. The main collections for the study are: Ruth Benedict's papers at the Vassar Library in Poughkeepsie, New York; Franz Boas's, Ruth Benedict's, Elsie Clews Parsons's, and Margaret Mead's Papers at The American Philosophical Society Library in Philadelphia, Pennsylvania; Ruth Underhill's Papers at the Denver Museum of Science and Nature and the Denver Public Library in Denver, Colorado; Gladys Reichard's Papers at the Museum of Northern Arizona, Flagstaff, Arizona; and Alfred Kroeber's and Robert Lowie's Papers at the Bancroft Library at the University of California, Berkeley, California.

There has developed a considerable literature on women anthropologists in the Southwest, including studies by Barbara A. Babcock,

Louise Lamphere, and Nancy Parezo.<sup>17</sup> Babcock has focused almost exclusively on Elsie Clews Parsons and her circle of female friends and researchers.<sup>18</sup> Lamphere has published several essays about women anthropologists in the Southwest, including a brief study of Gladys Reichard's work with the Navajo tribe.<sup>19</sup> As a rule, these studies have focused on how women anthropologists' lives reflected the experiences—most commonly the limitations—of being female in the West and in the intellectual academic community, and on the “sisterhood” that developed among female researchers in the Southwest. In contrast to these assumptions, there exist numerous instances of fierce competition and division between women anthropologists that call into question the blanket assignment of the term “sisterhood” to their relationships with one another. Further, Babcock and Lamphere claim that female anthropologists focused on “female research concerns” such as gender and family structures; however, women anthropologists also set out to deal with what both Babcock and Lamphere categorize as “male research concerns,” including, for example, social structure, economic issues, and leadership strategies. There is clearly room for a more precise examination of how anthropologists' concerns about their own gender and sexual identities, or their involvement in the academic debate about gender, affected what they “saw” in the field. Very little, for example, has been written to provide a nuanced definition of this community of scholars' very individualized and intellectualized feminism. While there are some studies of how their work focused on women and how empowering for women reading their work could be, none of these studies has investigated variations between personal writings and texts, instead choosing to accept their ethnographic writings at face value. And, finally, none of the studies has connected these scholars' feminism with a running critique of patriarchy, which often appears outside their discussion of women's experiences.

Anthropological thought deeply informed feminist ethnography. Female and male anthropologists alike worked in an intellectual context wherein concerns about sex and gender were central, in part because of the debate in both academia and cultural politics over biological and social determinism of gender and sex traits. Elsie Clews Parsons, a wife and mother interested in the development of the family as a social institution, researched mother/daughter relationships and matrilineal



kinship. Ruth Benedict, seeking harmony in her own troubled life, embraced gender equality as a determinant of Apollonian cultures. Ruth Underhill, a divorced woman fighting for her place in the academy, concentrated on gender roles and “woman’s place.” In their research about gender, Southwestern ethnographers sought answers to questions about gender in Euro-American culture. Who designated gender identities and how did that process take place? Were gender identities and roles immutable, “natural,” and biologically determined; or were they malleable, “cultural,” and socially determined? How were manhood and womanhood constructed? How did Native American peoples make sense of their own gender politics? Ethnographers asked these questions about Native American gender relations, identities, and roles because these same questions came up repeatedly in the ethnographers’ culture and played out in ethnographers’ everyday lives.

Recent work in the history of gender has turned to defining gender as socially constructed.<sup>20</sup> To put that argument simply, this would mean that the ways that a person acts as a woman or as a man depend not on behaviors caused by biological determinants, but instead on the rules of being that society has defined as either feminine or masculine. Seeing differences in definitions and structures of gender in Native American societies led some anthropologists to recognize—if not to name—the social construction of their own gender identities in their own culture. In addition, gender shaped the experiences of male and female anthropologists, and their ethnographic texts reveal their efforts to counteract the scientific sexism examined by Cynthia Eagle Russett, Rosalind Rosenberg, and Ellen Fitzpatrick.<sup>21</sup> Further, gender played an important role in the conquest whose legacy has shaped the West as a region.<sup>22</sup> The process of conquest especially characterizes the history of the Southwest as the site of, in Edward Spicer’s phrase, numerous “cycles of conquest”; and conquest there took on strongly gendered terms.<sup>23</sup> Even words like “civilization” had intensely gendered associations, usually burdened as well with allusions to the presence of white women and their moral authority.<sup>24</sup>

As an extension of their interest in the social construction of gender, anthropologists in the Southwest also concerned themselves with the study of sexuality. Ethnographic texts of the period reveal anthropologists’ conceptualizations of sexuality, both in their own

culture and in the cultures of their “subjects.” Ethnographers sought to understand how sexuality—both reproductive and social—resulted from cultural actions and mores. Ethnographers of this period faced in their own culture a period of “sexual revolution” in which relations between men and women—and same-sex relations—underwent massive and sometimes upsetting transformations. These changes in their own culture led them to seek answers outside their culture. Because they tended to consider a deep interest in their own, Euro-American, sexual culture as somewhat “improper” and “untoward,” they sought answers to their sexual questions in a less threatening fashion, by focusing on the sexual cultures of Southwestern Native Americans and other persons whom they saw as “primitive.”

The definition of gender as socially constructed has also led to the development of an historiography of the social construction of sexuality.<sup>25</sup> Historians of the social construction of sexuality argue that a society’s definitions of acceptable sexual behaviors vary greatly from culture to culture, and change over time. Further, definitions of categories of sexual behavior—including differentiation between reproductive and nonreproductive sexual activities—are socially and culturally constructed. Michel Foucault has argued that the study of a society’s “sexual discourses”—those practices outside the strictly reproductive practices that a given society accepts and even encourages—illuminates broader patterns within that culture, from gender interaction to economic relations.<sup>26</sup> Anthropologists constructed this sexual discourse in part, and their research about Southwestern Native Americans affected it.

Many recent studies of the history of sexuality have focused on definitions of sexual categories and identities. The question of the relative fluidity or rigidity of these categories and identities has become a core concern of historians of sexuality.<sup>27</sup> The study of cultural variations in these categories and identities also fascinated this group of ethnographers. Reading their studies of sexuality in Native American societies provides a starting point for examining anthropological discourse—and anthropologists’ private thoughts—about sexual and gender identity.

Patterns of conquest in the West also reveal sexual ideas. Historian of sexuality Thomas Lacquer traces the development of categories

of gender, sex, and sexuality from ancient history through the modern period; however, he says little about how European expansion—contact, conquest, and colonization—affected European and Euro-American conceptualizations of sexuality.<sup>28</sup> Clearly, the definition of the “making of the other” that Tzetvan Todorov examines in *The Conquest of America* included a sexual and gendered element, as reflected in the de-masculinization of “other” males as an ingredient of conquest.<sup>29</sup> Victorian sexual mores, for example, were deeply affected by the display of the “Hottentot Venus” in Europe, where an African woman could be sexually displayed as an expression of conquest without threatening “common decency.”

Little has been written about the role of sexuality in the process of conquest in the West, or in the cultural interactions that shaped the region. Sherry Smith hints at sexual tensions that may have existed on western army posts, where Victorian army wives wrote lyrically about sexually exciting Indian “bucks” whom their husbands fought to subdue.<sup>30</sup> Paul Hutton, in his biography of General Phil Sheridan, is much more explicit about the sexual tensions that underlay conquest; Sheridan’s reports of “Indian atrocities”—accompanied by his pleas for permission to destroy entire Indian populations—dwelt “salaciously” on details of supposed sexual defilements of white women by Native American men and women.<sup>31</sup> This background deeply affected Southwestern ethnographers, especially women ethnographers, many of whom chose Southwestern tribes for their fieldwork because they had a reputation for respecting—and not raping—white women.

Walter Williams’s and Will Roscoe’s recent studies of sexual diversity in Native American societies shed some light on the connections between sexuality and cultural conquest in the West.<sup>32</sup> Williams and Roscoe deal explicitly with the impact of conquest on Native American sexual practices. Williams traces the criminalization of the man-woman role—and its subsequent suppression and disappearance—to the extension of homophobic federal authority over Native American peoples. Roscoe’s discussion of the role of anthropologists in changing Zuñi experiences with and conceptions about sexuality indicates a broader pattern of cultural conquest. Roscoe fills his story with the agents of Euro-American conquest; soldiers from Fort Wingate near the Zuñi Pueblo, reformers and “friends of the Indian,” and ethnographers

and anthropologists all make appearances as Roscoe addresses the varying degrees to which their presence and actions changed Zuñi culture. Neither Williams nor Roscoe, however, discuss the impact that Native American sexuality had on the agents of Euro-American conquest. Both ignore the extent to which the sexual aspects of conquest transformed the conquerors as well as the “conquered.”

Ramón Gutiérrez's controversial study *When Jesus Came, the Corn Mothers Went Away* is perhaps the most complete discussion of the role that sexual issues played in the process of conquest in the Southwest.<sup>33</sup> A study of “marriage, sexuality, and power” in colonial New Mexico, *When Jesus Came* traces changes in marriage patterns and sexual behaviors among Pueblo Indians and Hispanic colonists. Gutiérrez's history reflects how the Spanish conquest of the Southwest represented a cultural dialogue, rather than the monologue usually suggested by the term “conquest”, between two dynamic and complex societies. Ethnographic texts reveal some of the connections between Southwestern cycles of conquest—especially as they played out in the sexual terms hinted at by Gutiérrez's study of this earlier period—and the activities of anthropologists in the region.

These issues have come full circle in recent times. Second Wave feminists point to more egalitarian and matrilineal indigenous societies and emphasize the significant political roles that Native American women like former Cherokee chief Wilma Mankiller play in order to critique misogynist gender relations in Euro-American society. Gay scholars and community leaders embrace the man-woman as an historical foreparent to the contemporary gay movement and emphasize what they see as the freer acceptance of same-sex sexual contact in traditional Native American cultures in order to critique contemporary Euro-American homophobia. Environmentalists applaud what they view as the natives' more sensitive land use and lighter environmental impact in order to critique the environmental degradation perpetrated by Euro-American society, and participants in the New Age movement appropriate what they see as native religious rituals in order to reestablish their linkages to the natural and “primitive” sacredness. Understanding how ethnographers “created” Native American histories to fit Euro-American needs can help make sense of these more recent patterns.

# 1

## Taking the Field: The Social Context of Southwestern Ethnography

The urgency of the disappearing primitive cultures . . . remained with us.

—Margaret Mead (1959)<sup>1</sup>

When beginning to address the ways in which feminist women anthropologists ethnographically constructed the Southwest, it is useful to start with an examination of the social context in which their research took place. As anthropologists, women ethnographers of this period were at the center of the emergence of cultural relativism in anthropology; as students of Franz Boas, they were advocates and leaders in that movement. The debates that centered within the emergence of cultural relativism would have a deep impact on the issues that women ethnographers addressed. In part, their research questions would be as much shaped by their striving to promote cultural relativism as to address feminist concerns in their work. In addition, a body of assumptions about women's abilities and the safety of women researchers in the field led women to conduct research in the Southwest more than in any other American geographic region; as a result, feminist ethnographies came to play a much more visible role in the general ethnography of the Southwest than in any other contemporaneous regional ethnography inside or outside the United States. Further, the possibilities of career available to women ethnographers shaped their research and writing and connected their own experiences as women

and as feminists to the broader social structures of patriarchy that they came to critique. Thus, understanding the reasons women chose to conduct their research in the Southwest, the core debates against and within which they wrote, and the emergence of their feminism within their anthropological thought, is essential to understanding the texts that feminist anthropologists produced during the period.

In the first half of the twentieth century, the Southwest was quite literally overrun with anthropologists doing ethnographic research. The Southwest held particular interest for ethnological researchers. It was accessible, attractive, offered a pleasant climate, and contained a great degree of cultural diversity. In addition, many saw the region as a safe place for women to conduct research, which encouraged advisors, such as Franz Boas, to send female graduate students there to begin their fieldwork. As a result, the Southwest became and has remained the site of the greatest concentration of ethnological research in the Western Hemisphere. The sheer volume of Southwestern research and the period of its flowering in the first half of the twentieth century made the resulting literature distinctive as a record of the emergence of several important intellectual trends.

In the narrow context of the history of anthropology, Southwestern ethnography traces the development of anthropology as a discipline in the United States. Especially for the students of Franz Boas and Ruth Benedict, the Southwest became the training ground for an entire generation of anthropologists, from Margaret Mead to Robert Lowie. As anthropologist Keith Basso has pointed out, "Clyde Kluckhohn, an eminent student of culture, once wrote that anthropology was a 'mirror for man.' One could observe just as correctly that the history of research in the Southwest has been—and still is—a mirror of American anthropology."<sup>2</sup> Southwestern ethnography reflected the emergence of functionalism and anthropological psychology, approaches that came to dominate American anthropology until the latter half of the twentieth century. In addition, it traced the transitional period from the cultural evolutionary models touted by European ethnologists as well as the Bureau of American Ethnology to the cultural relativism associated with Franz Boas.

In the broader context of American intellectual life, Southwestern ethnography illustrated the emergence of cultural relativism in public

discourse, as modernism became more a part of everyday life. As an outgrowth of this trend, Southwestern ethnography both reflected and encouraged the popular conception of Southwestern Native American peoples as vanishing, as archaic, but ultimately, as the keys to the survival of Western civilization. As such, ethnography of the region played an important part in constructing an identity for the Southwest; that identity is reflected in the region's present, where Pueblos have become tourist attractions, and travelers turn blind eyes to the "Made in Mexico" designations on the Navajo-style rugs they purchase for their suburban—and often eastern—homes. In short, ethnography created a discourse of what was legitimate and authentic in the Southwest that continues to be a matter of contention today.

Columbia University anthropologists began to formulate the ethnographic definition of the Southwest soon after Franz Boas founded the first American professional graduate program in anthropology at the university in 1895. The nature of the research topics that Boas and his academic colleagues found most pressing for the Southwest sheds light on their own perceptions of the region. Boas advocated "salvage ethnography," saving the oral and ethnological traditions of people who were isolated from European contact and who were, in his estimation, in danger of disappearing. Boas judged Southwestern native peoples as subjects of salvage ethnography; in doing so, he revealed that he thought of the region as, if not pristine, then at least unsettled by Europeans. As an area filled with native populations he viewed as unadulterated by European culture, the Southwest drew his interest, and more importantly held it.

One of Boas's students, Esther Goldfrank, recalled that in the 1920s and 1930s at Columbia, "The Southwest was a frequent topic of conversation." She later wrote: "Research there was seen to be intriguing, frustrating, and challenging. The strands of knowledge in the Southwest always remained tangled."<sup>3</sup> One reason for the deep interest in the region was what contemporary anthropologist John Bennett called "a pervading sense of mystery and glamour of the country itself."<sup>4</sup> In addition, as Curtis Hinsley and Nancy Parezo have pointed out, many social critics, not only ethnographers, felt that the Southwest could save Euro-American culture from its own excesses. They saw the people and landscape of the Southwest as "the means by

which Anglo-American society could be restored. Highly dissatisfied with capitalism, industrialism, puritanism, and rural morality, they went to the Southwest to learn how society could be reintegrated."<sup>5</sup>

Boas made many trips to the Southwest and continued to see it as a region filled with isolated and relatively unchanged—and unchanging—native cultures. In fact, he may well have argued that the only unifying identity of the region was “culture”—that the Southwest was a place where “culture,” a diverse, and somewhat undefinable thing, persisted. For Boas, the persistence of culture—preferably in a pristine and unadulterated form—was the most important thing. In addition, the Southwest seemed geographically designed for a generation of social scientists—anthropologists and archaeologists especially—who looked to Egypt and Africa for the fonts of historical knowledge. The deserts of New Mexico, Arizona, and California were much like those of Africa or Egypt—only closer to New York. In the view of the Boasians, the Southwestern deserts—like the deserts across the Atlantic—preserved ruins and remains that past cultures left there, and they isolated and therefore “preserved” unadulterated native cultures.

The proximity of the region to New York, and its ability to fulfill Boas’s stereotypes of unadulterated cultures made the Southwest irresistible to him and to his students. In addition, because of the variations in Native American cultures there, the region allowed Boas and his students to challenge standing ideas about culture and societies in more general terms.

The debates over theory that came to shape the writing of the region’s history took place, for the most part, far away from the Southwest. Throughout the nineteenth century and into the early twentieth, European and eastern United States research about human cultures developed several significant strains of anthropological theory that were to affect ethnographic writing in the region. Many of these forces culminated in a school of American ethnology associated with the work of Columbia University anthropologist Franz Boas and his students.

As Thomas Kuhn has illustrated, scientific discovery is always a group project in which scientific communities made up of scholars, graduate students, and collaborators focus and define specific questions to examine.<sup>6</sup> The majority of researchers then remain within



those boundaries, examining those areas. However, the boundaries shift quickly.<sup>7</sup> Once in a great while someone breaks out of these boundaries completely, makes a leap forward, and initiates a paradigm shift. Boas's championing of cultural relativism was such a paradigm shift in the field of anthropology.<sup>8</sup>

From its founding through the middle of the twentieth century, Columbia's Anthropology Department was on the cutting edge of scholarship. Department chair Franz Boas, best known for his studies concerning the broad issue of race, led a movement toward the professionalization of anthropology as a discipline. At a time when most anthropologists did little fieldwork, Boas called for extensive trips into the field. Anthropologists, Boas argued, must have actual contact with their subjects. Although John Wesley Powell, the director of the Bureau of American Ethnology, had stressed the importance of fieldwork as the basis for ethnological knowledge, most intensive fieldwork was carried out not by "academic" anthropologists, but instead by scientists conducting government- or museum-funded surveys. These early field-workers included, in addition to Powell, such important early figures as Frank Cushing, Washington Matthews, and Matilda Coxe Stevenson. Inspired in part by their example and in part by his own training in the natural sciences, Boas was the main force in bringing this concept of exhaustive fieldwork to bear on the academic discipline of anthropology. Within the halls of academe, Boas's call for fieldwork was a revolutionary statement.<sup>9</sup>

Because of this emphasis on fieldwork and on face-to-face contact with those cultures being studied, Boas's work also marked a turning away from sweeping theoretical pronouncements to painstakingly detailed studies of distinct cultures. Where traditional anthropology stressed biological determinism, Boas argued for cultural construction; and where the majority of anthropologists adhered to the theories of the scientific racists, Boas set out to stress the cultural biases that led to the formation of ideas of race and to the very ideas of the scientific racists.

Out of these twin concerns of determining cultural distinctiveness and combating scientific racism, Boas and his students shaped an anthropological tradition distinctive from studies that came before or from those produced in Europe during the same period.<sup>10</sup> The outcome

of this tradition is evident in the ethnographic texts produced by Boas and his students, especially in the Southwestern United States. Many of the texts produced focus on the life histories of individual informants—most notably the work of Gladys Reichard and Ruth Underhill—and on determining differences that existed among and between Native American groups in the region—for example, Elsie Clews Parsons's extensive folklore studies of the northern Pueblos, in which she attempted to trace interueblo variations in myth and symbol.

Boas's variations on European ethnography grew out of several traditions and developments in the field of anthropology, including the development of an American School associated with the cultural evolutionist Lewis Henry Morgan, and the emergence of a European School that debated the theories associated with Morgan's English cultural-evolutionist counterpart, Edward B. Tylor. Examining the theoretical underpinnings of the cultural evolutionists reveals to some extent the terrain of the battlefield upon which Boasians fought, as well as the arguments they challenged with their own research.

Before Boas, anthropology had been dominated by the belief that cultural differences had developed because some "primitive" cultures had failed to advance along the straight line of cultural evolution. These cultures, evolutionists like Morgan and Tylor argued, had become arrested in their development, and languished in savagery or barbarism; meanwhile, they argued, white, European culture had advanced into a state of civilization. Eugenicists used this argument to say that those in more primitive states had thus proven their lack of "fitness," in the Darwinian sense, and should be therefore prevented from reproducing. Scientific racists used these ideas to argue that racial difference was attributable to levels of development, rather than environmental and biological processes. Not all cultural evolutionists were scientific racists, but almost all scientific racists were cultural evolutionists. For this reason, Boas, who had experienced anti-Semitism and racism in Germany before he emigrated to the United States, associated cultural evolutionism with scientific racism and set out to challenge both.

Boas argued that cultural difference resulted from environmental adaptations; cultures that evolved in deserts, for example, would place great cultural importance on water and rain, and this concern would

be reflected in their mythology and rituals. In his 1911 book, *The Mind of Primitive Man*, published in the ascendancy of eugenic thought in the United States and Great Britain, Boas argued that culture is “not an expression of innate mental qualities” but instead “a result of varied external conditions acting upon general human characteristics.”<sup>11</sup> He was not the only scientist to reach this conclusion. His advocacy of what came to be called cultural relativism built upon the teaching of several anthropologists in Europe, including both Diffusionists and Functionalists. Diffusionists, like W. H. R. Rivers and Leo Frobenius, argued that all cultures had developed from some mother culture (most probably located in Egypt or in Asia) and then diffused and altered as they expanded to other parts of the world. Diffusionists challenged cultural evolution by arguing that while all civilization shared a common beginning—an Ur-Culture or a *kulturkreise*—cultural differences did not result from differing stages of progress along some shared straight line of development; instead, difference resulted from the physical dispersion of populations, which then adapted to their surroundings in various ways, creating distinctive cultures where they developed. Functionalists, like A. R. Radcliffe-Brown and Bronislaw Malinowski, stated that cultural differences had resulted from the development of differing needs and desires created by the interaction of populations and their environments; each element of any culture, Functionalists argued, had a meaning that related to what need or desire the element fulfilled. Therefore, Functionalism dropped the question of origins almost entirely, despairing of ever finding an answer.

While the Diffusionists and Functionalists argued against cultural evolution in European anthropology, Franz Boas led the anti-cultural evolution movement in American anthropology. Boas had been educated in Germany, earning his degree in geography and physics at the University at Keil, where his dissertation addressed the color of seawater. His professors in geography and physics had trained him to trust only a firm empiricism, insisting that facts come first and theory later. During a geographical field trip to Baffinland in 1883 and 1884, he decided that his real interests tended toward the study of people rather than climate and physical landscape, so he became an anthropologist, turning the Baffinland trip into his first field trip among the Central Eskimos. His experience in Baffinland led him to conclude that

geography plays a mainly limiting rather than a creative role, determining which elements will be selected out of a culture rather than deciding which will be added. The residents of Baffinland, Boas argued, did things in spite of their environment, not just because of it; they had a particular history and set of traditions behind them that were not necessarily also those of other northerly peoples who lived in a similar environment. A culture, Boas decided, is shaped by many historical forces, including contacts with other societies.<sup>12</sup>

This awareness of the complexity of cultural determinants led Boas to be skeptical of universal laws such as those set forth by the cultural evolutionists like Morgan and Tylor. It was not that Boas dismissed the possibility of broad generalizations, but instead that he felt that such generalizations might turn out to be only commonplace truisms. Rather than announcing any new doctrine, Boas attacked the works of previous writers, and his students followed this critical bent. For example, one of Boas's earliest students, John Swanton, showed that the tribes of North America did not show the progression of kinship organization that Morgan and others had postulated. Hunting bands with bilateral or patrilineal descent, Swanton illustrated, lived at a simpler level of culture on the Morgan and Tylor scales than the more "advanced" farming Hopi, Zuñi, Creek, and Natchez Indians who had matrilineal descent.

From the accumulating masses of ethnographic data on American Indians—much of which resulted from Boas's and his students' own research—Boas and his followers shaped a strongly antievolutionary theory. They denounced Morgan's stages of cultural evolution as figments of imagination, unsupported by evidence. Instead, they argued that the history of humans was a sort of "tree of culture," with fantastically complex branching, intertwining, and budding off—each branch representing a uniquely different cultural complex, to be understood in terms of its own unique history rather than compared to cultural complexes in other world regions in some grand scheme of "stages of evolution." In addition, the "American school" also challenged cultural evolutionary theory in other ways. It denied the "progressive" idea that development could be equated with progress or betterment; it refused to consider widely separated cultures as representatives of broad "culture types," such as the all-embracing category

of “savagery”; and it organized ethnographic data into “culture areas,” each thought to be unique in culture history, with a resulting distinctive array of culture elements, or traits.

Boas thought that the global generalizations about cultural evolution made by nineteenth-century writers were premature. For one thing, they were not based on extensive fieldwork. Morgan had visited the Iroquois and Chippewa reservations, and Tylor had traveled to Mexico as a young man, but neither immersed themselves in the cultures they used as their evidence. Indeed, most of the nineteenth-century anthropologists were not professionals, but rather took up anthropology as an avocation. Instead of field research, most of nineteenth-century anthropology was the result of reading books and travelers’ memoirs. Most cultural evolutionists built their theories from reports of early travelers, sea captains, missionaries, and other commentators. Boas pointed out the biased and superficial understanding that travelers, sea captains, and missionaries had of the people whom they observed, and declared a moratorium on theorizing. He advocated that anthropologists should build up a body of ethnographic data from which to make more reliable generalizations later. Meanwhile, he warned, primitive cultures all over the world were disappearing under the impact of Western imperialism. He pointed out that time was short and exhorted his students to go out and record the facts of native life before the cultures vanished. His student Margaret Mead later recalled: “[H]e had cast himself in the role of one of the responsible leaders of a giant rescue operation to preserve the vanishing fragments of primitive languages and cultures. This had to be done with almost no money, very few trained people, and no time to spare.”<sup>13</sup> Boas’s own tone conveyed the urgency of the project to his students; Mead recalled, “The urgency of the disappearing primitive cultures and of the cruel inequalities in the world on which research was grievously needed was communicated to us not by preachments but by tempo, tone, and gesture, and this urgency remained with us.”<sup>14</sup> Thus much of Boas’s and his students’ research is referred to today as “salvage ethnography.”

For his own part, Boas proved an indefatigable field researcher, despite chronic health problems including heart disease. From 1886 to 1931 Boas made thirteen field trips to the Pacific Northwest and the West Coast. In 1930, at the age of seventy-two, he made a return trip to

the Kwakiutl to try out a new method—the use of film.<sup>15</sup> Throughout his career, he trained himself to learn the languages of the area.

As part of the application of scientific method to anthropology, Boas stressed extensive fieldwork as the basis for anthropological studies. Ronald Rohner later researched Boas's techniques by interviewing Boas's anthropological subjects; in one 1964 interview in British Columbia with Mrs. Tom Johnson, the daughter of Boas's Kwakiutl subject George Hunt, Mrs. Johnson recalled Boas's attempts to blend in with the tribe during ceremonies. "He used to use our customs," Mrs. Johnson told Rohner. "He don't want to be left behind."<sup>16</sup> Questioning the extent to which Boas became involved in local culture, Rohner asked if Boas ate grease, to which Mrs. Johnson replied emphatically, "Yes. . . . He likes it, because he want to be real Indian."<sup>17</sup>

Despite the significance of his own research and statements to the anthropological profession at large, Boas's greatest impact on American anthropology came as a teacher of several generations of Columbia anthropologists. In 1895 Boas took a joint position at Columbia University and the Museum of Natural History in New York City. He founded at Columbia the first comprehensive graduate program in anthropology in the United States. Columbia under Boas quickly became the focal point for anthropological activity, as growing numbers of Boasian students began extensive fieldwork among American Indians. Among the first wave of Boas's students were those who came to define much of the American School, including Clark Wissler, Alfred L. Kroeber, Robert Lowie, Margaret Mead, Ruth Benedict, Elsie Clews Parsons, and Melville Herskovitz. The Boasians' first general works, as opposed to specific ethnographies, which attempted to generalize about Native American experience were Clark Wissler's *The American Indian: An Introduction to the Anthropology of the New World* (1917) and Elsie Clews Parsons's edited volume *American Indian Life by Several of Its Students* (1922).

In addition, Boas argued for writing ethnography that would also provide a broad cultural critique of European and American society. As George E. Marcus and Michael M. J. Fischer have pointed out, "Boas used ethnography to debate residual issues derived from the framework of nineteenth-century evolutionary thought and to challenge racist views of human behavior, then ascendant."<sup>18</sup> In 1928, Boas himself expressed his faith that "anthropology illuminates the social processes

of our own times and may show us, if we are ready to listen to its teaching, what to do and what to avoid."<sup>19</sup> In 1938, he even wrote what he called an "Anthropologist's Credo," about the responsibility of anthropologists to provide cultural critique. Boas argued that "all that man can do for humanity is to further the truth," and so he strove to "live and die" for "equal rights for all, for equal possibilities to learn and work for poor and rich alike."<sup>20</sup> Further, he encouraged his students' cultural critique in their research. As a result, as Marcus and Fischer illustrate, they "began to use anthropology's subjects as specific probes into American conditions of the 1920s and 1930s . . . [and] were primarily critics of society under the banner of relativism."<sup>21</sup>

Boas's commitment to the scientific method and his vigorous opposition to anti-Semitism and other forms of discrimination attracted to Columbia a group of enthusiastic, progressive young students of anthropology. For most of these students, Boas was fascinating; Ruth Underhill, who studied with Boas in the 1930s, later characterized her three years of study with him as "full to bursting with discovery and achievement."<sup>22</sup> Among those students were Jewish critics of racism, and a large group of women, many of them Quakers, who embraced feminism in the form of a critique of patriarchy.

Just as he stressed fieldwork as the basis for his own ethnographies, Boas emphasized the importance of his students doing fieldwork, and he trained many of them in fieldwork by taking them along with him on field trips. In the field, Boas trained his students to record everything they saw. Marian Smith characterized this as a "natural history" approach. Describing it later, Smith said, "Interest lies not mainly in systems per se, but in the 'surrounding world.' There is a fascination in following the details of a subject just for its intrinsic interest, and there is also the knowledge that, once accumulated, such systematic data will have value—sometimes in wholly unexpected directions."<sup>23</sup> Margaret Mead later remembered, "This recognition of the continuous possibilities of illumination from the material became as conscious as the idea of such methods as such remained unconscious."<sup>24</sup> Instead, Mead argued, most Boasian methodology resulted from a moment of "insight" or inspiration. "Ruth Benedict defined her approach to the contrast between Plains and Pueblo cultures not as a method of analysis but as resulting from a sudden insight."<sup>25</sup> Mead concluded,

"[E]ven such an obvious point as my recognition that it was nonsense to study adolescents without also studying preadolescents, I felt as an insight, not as methodology."<sup>26</sup> Boas himself followed this advice. For example, Boas recorded page after page of blueberry pie recipes in the Kwakiutl language, with the English translation on facing pages, although he never used them in any of his published works.<sup>27</sup>

Boas wanted an idiographic approach to culture, which he defined as trying to learn as much as possible about a particular culture in all of its minutiae, as opposed to a nomothetic approach, which would consist of searching for general laws or regularities. By historically reconstructing primitive cultures, he believed, anthropologists could begin to make some nomothetic conclusions. By 1930, however, he was skeptical of the possibility of sufficiently reconstructing cultures or of reaching nomothetic conclusions. He wrote that "an error of modern anthropology, as I see it, lies in the overemphasis on historical reconstruction, the importance of which should not be minimized, as against a penetrating study of the individual under the stress of the culture in which he lives."<sup>28</sup> Boas thus advocated a turn to the field of "culture and personality," encouraging the work of his students Margaret Mead, Ruth Benedict, Ruth Bunzel, and Ruth Underhill in those areas.

Edward Sapir, one of Boas's students, reacted to what he felt to be the depersonalization inherent in the study of culture complexes, diffusion of customs, and reconstruction of culture history by writing, "It is always the individual that really thinks and acts and dreams and revolts."<sup>29</sup> In Sapir's view, Boasian and cultural-evolutionary anthropology looked on humans as passive culture carriers without individual significance. In a series of papers and seminars, Sapir and a few other American anthropologists began in the 1920s and 1930s to work toward an inclusion of psychological study in anthropology. One of Sapir's articles was aptly titled "Why Cultural Anthropology Needs a Psychiatrist." Another of Boas's students, Margaret Mead, became interested in psychiatry and the relationships of personality and culture; for her first major fieldwork, she set out to study psychological problems of adolescents among the Samoans, and her findings later became her very popular study *Coming of Age in Samoa* (1928).

In part the new interest in psychology in non-Western cultures



was not new at all. Bronislaw Malinowski had been one of the first to concern himself with the relevance of psychoanalytic theory to non-Western cultures, in his *Sex and Repression in Savage Society*.<sup>30</sup> But interest in psychoanalytic theory was spurred by the arrival of many distinguished psychoanalysts, driven out of Germany by the rise of Adolph Hitler, to the United States. Neo-Freudians, such as Karen Horney, Erich Fromm, Franz Alexander, and Erik Erikson, became influential in the social sciences, including anthropology.

In a series of seminars during the 1930s at Columbia University, the anthropologist Ralph Linton and the psychiatrist Abram Kardiner collaborated in producing a new approach to psychologically oriented anthropology. Kardiner and Linton formulated the idea of “basic personality” as a unifying concept for understanding humans and culture.<sup>31</sup> Linton wrote, “The basic personality type for any society is that personality configuration [a pattern of psychological characteristics] which is shared by the bulk of a society’s members as a result of the early experiences which they have in common.” These commonalities of experience are brought about by the similarities in child-training practices in cultures. Linton and Kardiner argued that child-raising practices, such as toilet training or breast-feeding, tended to be fairly standardized within a particular society, so that women tended to breast-feed for about the same amount of time as their neighbors, feed their children roughly the same foods, and apply the same kinds of toilet training and other disciplines. Thus children in any given society pass through the same general gamut of childhood experiences and as a result develop many personality traits in common. The basic personality of a society, once established, is reflected in many areas of custom—particularly in religious beliefs and practices, art, mythology, and popular fantasies.<sup>32</sup>

Culture and personality studies developed rapidly in the 1940s, and many anthropologists began to use personality tests in the course of fieldwork. Interest also developed concerning mental illness and types of “abnormality” in primitive societies. All of these studies express a strong, continuing interrelationship between anthropological and psychological theories of human behavior.

Another reaction to the piecemeal cataloguing of customs and complexes developed into what have been termed “configurational

studies." Ruth Benedict's *Patterns of Culture* (1934) is the best-known expression of the trend. Taking the Pueblo Indians of the Southwest, the Kwakiutl Indians of British Columbia, and the Dobu of the South Pacific as her main examples, Benedict attempted to demonstrate that each culture is not just a random collection of customs haphazardly "borrowed" or diffused from surrounding cultures. Each culture, she maintained, is organized around some central configuration of ideas. Among the Pueblos it is an "Apollonian" moderation and restraint in all things, adherence to the golden mean; Kwakiutl culture, in contrast, is supposedly pervaded by individualism and a megalomaniacal competition for prestige and honor; while Dobuan culture is characterized by a paranoid, sorcery-ridden hostility of all against all. In each case, the economy, kinship patterns, religious practices, and other major elements of custom are all thought to be shaped and interrelated by the one dominant motif.

While a discussion of the role of psychological studies in Boasians' research indicates many of the internal divisions within the American School, Boasian ethnography in the Southwest emerged in several key strains that follow the developments of American anthropological theory. Many of Boas's students set out to fulfill his first initiative, which was to capture deeply descriptive ethnographies before such collecting became impossible. To these ends, Leslie Spier set out for fieldwork among the Havasupai and Yuman tribes, publishing his "Havasupai Ethnography" in 1928 and a book-length ethnography of Yuman tribes on the Gila River in 1933.<sup>33</sup> Elsie Clews Parsons published fragments of ethnographies of Pueblo tribes from 1915 until her study of Isleta Pueblo, published in 1931, provided her first complete ethnography of a Pueblo.<sup>34</sup> Students of Boas's student Alfred Kroeber at the University of California at Berkeley also published ethnographies of the region; C. Daryll Forde published an ethnography of Yuma Indians in 1931, and Edward W. Gifford followed with a study of the Cocopa in 1933.<sup>35</sup> Kroeber himself edited a Walapai ethnography shortly thereafter, in 1935.<sup>36</sup>

Another key concern for the Boasians was the element of cultural borrowing and diffusion. While cultural evolutionists had noted the existence of borrowing and diffusion, and Diffusionists had made cultural diffusion the center of their examinations, Boas and his students saw the significance of delineating cultural borrowings and diffusions

as a way to get down to the “original” and authentic cultural values of Southwestern groups. Confronted with geographically concentrated populations with vastly differing cultures, Boasians wondered how these different cultures had historically interacted and altered one another. Some Boasians, chiefly in the earlier period, sought to find a “mother Pueblo” from which the diverse Pueblo cultures had descended; Elsie Clews Parsons’s folklore collecting before 1920 especially focused on tracing folklore variations among the northern Pueblos in an attempt to identify the “original” versions of stories and the role that inter-Pueblo intermarriage had played in their diffusion.<sup>37</sup>

In his encyclopedic researches into California culture groups, Alfred L. Kroeber focused on shared cultural elements and cultural diffusion among trading groups; he followed his “Handbook of Indians of California,” published in 1925, with a broader continental study, “Cultural and Natural Areas of Native North America,” in 1939.<sup>38</sup> One dilemma Boas and his students faced in the Southwest was how to define the place as a region. Early in their researches, Boasians argued that the U.S.-Mexico border, which bisected the American Southwest, was at best an artificial dividing line; instead of ending at the Rio Grande, the Southwestern culture area, according to Boas’s students, extended into northern Mexico. Several Boasians set out to define the Southwest as a culture area, including Leslie Spier, Ralph Beals, and Alfred Kroeber. Spier’s fieldwork among the Havasupai made clear to him the permeability and artificiality of the border; his publication of “Problems Arising from the Cultural Position of the Havasupai,” in 1929, spurred examination of the definition of the Southwest as a region.<sup>39</sup> Ralph Beals buttressed Spier’s argument that the border cut across a legitimate trans-border culture area in his 1932 comparative historical ethnology of northern Mexico prior to 1750, tracing the interconnectedness of the region before the advent of the Mexican state and war between Mexico and the United States divided it.<sup>40</sup> Beals’s full statement of the region as a culture area appeared in his study of northern Mexico and the Southwestern United States that was published in Mexico in 1943.<sup>41</sup> The many cycles of conquest that Southwestern groups had experienced—warfare and raiding among indigenous groups, Spanish colonization, Mexican control, Anglo-American conquest on many levels—came to the attention of Boasian researchers

in part because of this attempt to define the Southwest as a region. William D. Strong's "Analysis of Southwestern Society," published in 1927, traced these patterns, as did Emil W. Haury's "The Problem of Contacts Between the Southwestern United States and Mexico," published in 1945.<sup>42</sup>

Boas's concern with the interaction of culture and environment stemmed in part from his days as a student of geography. His students in the Southwest especially set out to examine the influence of natural environment on culture formation as early as Elsie Clews Parsons's investigations of ethnobotany in her earliest field trips. Julian Steward published a study of the ecological aspects of Southwestern society in 1937, presenting traditional land- and plant-use patterns of Pueblo groups.<sup>43</sup> Esther Goldfrank examined contemporary land-management practices in her work, especially in her study of Navajo irrigation agriculture published in 1945.<sup>44</sup>

One of the most important debates that emerged from Boas's seminars throughout the 1920s was over Native American acculturation to non-Indian cultures in the region. Boas's students addressed this issue starting with Elsie Clews Parsons's 1928 publication of an examination of Spanish elements in Pueblo Kachina cults, and with her 1934 collaboration with Ralph Beals about the Spanish influences on sacred clown rituals.<sup>45</sup>

By 1930, A. R. Radcliffe-Brown's functionalist theories held sway at the University of Chicago, and Boas's student Edward Sapir and Columbia professor Ruth Benedict's interest in anthropological psychology began to have an impact on American anthropology in the region. A number of Boas's students embraced functionalism at least partially, resulting in an outpouring of functionalist studies of Southwestern cultures. Boasians and Radcliffe-Brown's functionalist disciples were alike in that they both saw cultures as systems—rather than aggregates of their contents—which changed over time and interacted in different ways. Boasians tended to differ from functionalists, however, in arguing that the same cultural aggregates need not make the same culture, as the culture's system may assign different meaning to each aggregate. In part, this variation on functionalism emerged among Boasians because of Benedict's and Sapir's insistence on the role of individual variations within each culture.

With the arrival of Radcliffe-Brown in the United States to teach at the University of Chicago in the late 1920s, studies in kinship and social organization grew. Alfred Kroeber had done a study of Zuñi kinship and social organization in 1919, and Robert Lowie did the same for the Hopi in 1929. Radcliffe-Brown's student Morris Opler's 1936 study of Apachean kinship systems set a standard that Boasians rushed to meet.<sup>46</sup> Several other University of Chicago studies of kinship followed, including Edward Spicer's study of an Arizona Yaqui village, Mischa Titiev's examination of Oraibi Hopi social organizations, and Fred Eggan's overview of social organization in the western Pueblos.<sup>47</sup>

The University of Chicago's functionalist school had an important effect on Boasian Southwestern ethnology, as Boasians struggled to reconcile and synthesize functionalism with salvage and historical ethnology—or the reconstruction of past civilizations. For their part, the functionalists also attempted to put more history into their own studies. Ruth Underhill's study of the social organization of the Papago (Tohono O'Odham) of Arizona and northern Mexico, published in 1939, is an example of a Boasian synthesis.<sup>48</sup> Morris Opler's study of Apache lifeways and Clyde Kluckhohn's collaboration with Dorothea Leighton on a study of the Navajo followed in 1941 and 1946 respectively.<sup>49</sup>

Functionalism also inspired Boasians to address religion in a new way. In place of pre-functionalist interest in the esoteric elements of religion and the spatial distribution of religious practices, Boasians focused instead on religion as a form of cultural institution. They examined how religion held the community together and focused on the ways in which religious practice encouraged community cohesion. Elsie Clews Parsons's two-volume masterwork *Pueblo Indian Religion*, published in 1939, made the argument that diverse Pueblo religious practices served to unite Pueblos internally and provided a core identity in the face of almost constant intermarriage among the Pueblos.<sup>50</sup> Ruth Murray Underhill's 1948 study of ceremonial patterns in the Southwest focused on the role religion played in determining the region and the cultures within it.<sup>51</sup>

Specific Boasian studies of religion within this new structuralist mode also continued to appear. Some, most notably Gladys Reichard's 1950 study of Navajo religion, focused on religious symbolism.<sup>52</sup> Others focused on the role of psychology in religious practice and belief, such

as Ruth Benedict's study of Zuñi mythology, which appeared in 1935, and Clyde Kluckhohn's examination of Navajo witchcraft, published in 1944.<sup>53</sup>

Benedict, especially, pushed Boasian ethnology to include anthropological psychology. Boas himself had mixed feelings about the application of psychological research practices and theories to "primitive" cultures; according to Alfred L. Kroeber, he might accept some psychological investigation, but "psychoanalysis he would have no traffic with."<sup>54</sup> Even before Benedict's work opened up the Southwest to psychological anthropology, H. K. Haeberlin had published an article in 1916 that addressed Pueblo psychology explicitly.<sup>55</sup> Benedict's own approach grew out of her fieldwork at Zuñi Pueblo, and led her to publish two brief examinations of psychological types and patterns in 1930 and 1932 before publishing her book-length study comparing the psychology of the Navajo, Zuñi, and Dobuan, *Patterns of Culture*, in 1934.<sup>56</sup>

A large number of Boasian culture and personality studies, focusing on the patterning of culture, followed Benedict's book. Clyde Kluckhohn produced a study of patterning in Navajo culture in 1941, and Dorothy Eggan published a study of adjustment in Hopi culture in the same year.<sup>57</sup> In 1946, Esther Goldfrank's fieldwork with the Zuñi and Hopi resulted in her study of socialization, while Laura Thompson published a study of Hopi thought processes.<sup>58</sup>

Answering Benedict's and Sapir's calls for ethnological focus on the individual in society, a number of Boasian psychoanalytical and psychobiological studies of life histories appeared throughout the 1940s. Leo Simmons's edition of Sun Chief's autobiography, published in 1942, encouraged several similar studies.<sup>59</sup> These included Alexander and Dorothea Leighton's study of Gregório, and David Aberle's presentation of psychological analysis of a Hopi life history, published in 1949 and 1951 respectively.<sup>60</sup> Boasian psychological ethnologies included issues not addressed in more traditional studies. These included the content of dreams and myths, and Boasians gathered chronicles of dreams as well as analyzing them. Ruth Benedict's 1935 study of Zuñi mythology consisted to a large degree of analysis of the mythological content of Zuñi informants' dreams.<sup>61</sup> Esther Goldfrank used dreams in a 1948 article to show the ways in which informant personality and situation influenced their telling and remembering of Hopi emergence

mythology.<sup>62</sup> By 1949, Dorothy Eggan provided an overview of the importance of drawing on dreams in ethnological research.<sup>63</sup>

As “social problems” came to the fore in the social sciences, Boasian ethnologists also addressed such phenomena in Pueblo cultures. Clyde Kluckhohn provided detailed analysis of patterns and manners of aggression in Apache and Navajo tribes.<sup>64</sup> Others focused specifically on childrearing practices; Wayne Dennis wrote an ethnography of a Hopi childhood in 1940, while Clyde Kluckhohn and Janine Rosenzweig collaborated on a 1949 study of Navajo children.<sup>65</sup> George Devereux focused on issues defined at the time as “deviance”: homosexuality (1937), criminality (1943), and drinking (1948).<sup>66</sup>

Finally, later Boasians extended the list of tribes from whom ethnographies had been drawn. After 1930, Boas’s students working in the Southwest continued to focus on the Pueblos but also broadened their studies to include studies of other, non-Pueblo, southwestern tribes. W. W. Hill, Clyde Kluckhohn, and Gladys Reichard wrote regarding the Navajo.<sup>67</sup> Ruth Murray Underhill wrote several studies of the Papago.<sup>68</sup> Edward Spicer covered the Yaqui, while Wendell C. Bennett and R. M. Zingg addressed the Tarahumara.<sup>69</sup> Numerous Boasians, including Alfred L. Kroeber, Leslie Spier, Daryll Forde, and Edward Gifford, wrote about Pai and Yuman groups.<sup>70</sup>

In the period from Boas’s founding of the Department of Anthropology until the beginning of the Second World War, the vast majority of studies of Western Hemisphere populations focused on the Southwest. While, of course, studies not of the Southwest had an important impact on the development of American anthropology—Margaret Mead’s work in the South Pacific springs instantly to mind as an example—Southwestern research indicates the gross contours of the field’s development during the period. Further, the Southwest became a training ground for American anthropologists, like Mead, who went on to do their life’s research in other areas.

The sheer weight of research on the region would indicate the role that anthropology and ethnographic literature would have in shaping the definition in the American mind of the region’s cultural heritage, politics, history, and landscape. In part because of the ethnographic texts produced by Boasian anthropologists during this period, the Southwest became associated—especially outside the region—with a

Native American as well as, to a lesser degree, a Spanish or Mexican past. The landscape of the Southwest came to be seen as a sacred space inhabited by spiritual people, a belief held by New Age pilgrims to Sedona even today. Most importantly for this study, the models of Native American culture that Boasian ethnographers created in texts published during the period shaped, and were in turn shaped by, American thought about the meaning of gender and sexuality. The texts that resulted from the flurry of Southwestern research—especially as conducted by feminist researchers in the region—reveal an intriguing glimpse of the construction of not only the Southwest as a region, but of the emergence of a significant strain of feminist ethnography.

The period from the 1890s through the 1920s saw a huge influx of women into schools like Columbia University, the University of Chicago, the University of Michigan, and the University of Wisconsin.<sup>71</sup> The unprecedented number of women attending universities in the United States during this period has prompted Rosalind Rosenberg to term the era the “feminization of academe.” The increase in female students mirrored the advent of a group of women faculty. Few women had been able to pursue graduate study by the 1890s, and as a result, many of the early women faculty members were social activists rather than “pure” academics.

At the University of Chicago, for example, many of the women faculty members were, or became, deeply involved in the social reform activities at Jane Addams’s Hull House. In part because of the interests of these women faculty, and in part because of their own class and social backgrounds, the majority of women who entered universities as students during this period enrolled to study in the social sciences.

With the support of socially committed female faculty members and of male faculty members like John Dewey at the University of Chicago and Franz Boas at Columbia University, female undergraduate and graduate students undertook social research with vigor. Female students flooded departments of sociology, history, and anthropology to such an extent that it was not uncommon for women to form the majority of students in some social science courses. The number of Columbia female graduate students of anthropology was so startling that Boas was moved to write to a friend in 1920, “I have had a curious



experience in graduate work during the last few years. All my best students are women."<sup>72</sup>

Many of the women who entered Columbia viewed themselves as feminists before they started to study anthropology. Early students, like Elsie Clews Parsons, had taken an active role in suffragism, reform fueled by female moral authority, and other aspects of First Wave feminism. They were well versed in ideas about women's equality, the importance of motherhood as the source of female moral superiority, and the need for women's emancipation. First Wave feminists tended to define such emancipation as the freedom to pursue one's ambitions without limitations caused by gender. They emphasized the necessity of women to become self-reliant and self-supporting, and to seize control over their fertility, labor, and selves. Their feminism was political and politicized. But First Wave feminism also included variations. As a counterbalance to Alice Paul protesting for votes for women by chaining herself to the fence in front of the White House, there was Carrie Chapman Catt, working behind the scenes to negotiate with congressmen to bring the Nineteenth Amendment to a vote. Ruth Benedict, for example, represented a specific brand of First Wave feminism. While she strove to build a happy marriage with an unsupportive husband, she wrote biographies of nineteenth-century feminist heroines and drew from their passionate lives to address situations that confronted her in her own. While some women's historians have argued that feminism "died" after the passage of the Nineteenth Amendment, in fact, as Nancy Cott has pointed out, feminism continued, although in a less public form than it had taken during the suffrage campaigns.<sup>73</sup> Instead of remaining as a viable political force, with the exception of the ongoing feminist political struggle for the Equal Rights Amendment, feminism in the 1920s, 1930s, and 1940s became an intellectualized movement. As such, it emphasized the articulation of women's struggles over the fielding of armies to fight them. Feminism, according to Cott, became a more personal and more private movement, as women drew on feminist thought to make sense of their lives but did not necessarily proclaim those connections publicly. The work of Ruth Benedict, Gladys Reichard, and Ruth Underhill represents a case study of how this functioned. While each called herself a feminist, their works were not overtly "feminist" in the ways in which

Elsie Clews Parsons's sociological studies were. In part this resulted from the realization that to present feminist anthropologies overtly was to risk one's reputation as a scientific thinker; even Parsons toned down her overt feminism when she made the switch from sociology to ethnography, partly in reaction to Alfred Kroeber's dismissal of her earlier writings as "unscientific" and "unethnological" "feminist propaganda." Still, the feminism comes through in their ethnographies, especially in the inherent critiques of patriarchy that appear in their studies of Southwestern tribes. For these feminists, their feminism took the overt form of a running critique of patriarchy, shaped by their attempts to define patriarchal structures as well as providing more egalitarian examples as counter and positive structures.

As well, the careers of these women served to shape and sharpen their feminism. Their own struggles served as useful analogies to the feminist view of the struggles of all women within patriarchal cultures. The lives of Boas's female students shed light on the possibilities for career afforded these women Ph.D.s in the first half of the twentieth century. In some important senses, career defined the sort of research and writing that a scholar could undertake. And since the majority of Boas's female doctoral students entered upon graduate study with the intention and desire to become, like Franz Boas, a university professor, the career options available to them indicate important patterns against which they would write. Shirley Leckie's study of the life of Angie Debo, a contemporary of the anthropologists addressed here, sheds some light on the importance of this issue of career. Angie Debo was an impoverished historian from hard-scrabble roots whose desires to gain employment as a faculty member at the University of Oklahoma were quashed because she was an outspoken and non-demure woman Ph.D. Debo gained fame and some notoriety for her clear-eyed studies of the political and economic difficulties of Native American communities in resistance against Euro-American intrusion and chicanery. Her most famous work, *And Still the Waters Run*, was a study of murder and mistreatment of Indians in Oklahoma by prominent Oklahomans, published only after an extended controversy because Debo named names.<sup>74</sup> According to Leckie, Debo took up the cause of Native Americans in part because she was sublimating her own frustration over marginalization, exclusion, and bias in her

academic career. Rather than address such marginalization directly and call it what it was, Debo fought for a broader cause of social justice in which Native American causes became foils for her own. "Given her background and her own situation," Leckie writes, "the dispossession of the Five Tribes was, undoubtedly, a topic of compelling interest. As one imbued with the spirit of Oklahoma, Debo could express her anger over the fate of the Five Tribes in a way that she could never have expressed anger over her own situation."<sup>75</sup> To some extent this may be true of women anthropologists. As suggested by the careers available to them, they certainly experienced frustration and marginalization if their chief goal was indeed a full professorship at a research university. This marginalization served to clarify what was at stake.

Margaret Mead, a Columbia University anthropologist, argued that "Anthropology, a new science, welcomed the stranger." Explaining why women chose anthropology as a field of study, and why the field admitted them to some extent, she stated,

As a science which accepted the psychic unity of mankind, anthropology was kinder to women, to those who came from distant disciplines, to members of minority groups in general (with American Indians assuming a special position as both the victims of injustice and the carriers of a precious and vanishing knowledge), to the "over-mature," the idiosyncratic, and the capriciously gifted or experienced, to refugees from political or religious oppression.<sup>76</sup>

Ruth Bunzel argued that addressing women's concerns came naturally to her. She recalled, "I felt there was a great lack of knowledge about peoples' lives—particularly about women—so being a woman, that was the obvious place to start."<sup>77</sup>

Franz Boas encouraged women students to become anthropologists. Robert Lowie, one of Boas's first graduate students, argued that from the beginning Boas had seen the necessity of including women's ethnographies in order to truly salvage Native American cultures. Lowie wrote, "The same urge to see aboriginal mentality in all its phases has made Boas encourage work by trained women." One reason for this impetus was women ethnographers' special access to female

informants. "Since primitive peoples often draw a sharp line between the sexes socially, a male observer is automatically shut off from the native wife's or mother's activities," wrote Lowie. "A woman anthropologist, on the other hand, may naturally share in feminine occupations that would expose a man to ridicule."<sup>78</sup>

Still, it would be a mistake to argue that anthropology as a profession welcomed women scholars on completely equal terms. While the discipline provided feminist scholars with ammunition in their struggle for equal treatment, women anthropologists also sought further ammunition in the subjects of their research. For this reason, feminist anthropological writings about Southwestern Native American women became valuable ways for feminist scholars to make broader accusations against sexism and patriarchy in the ethnographers' own culture. They also underscored the reasons for limitations placed upon their own career aspirations by attitudes about gender in the American community.

Not all women anthropologists can be called feminists. This is not because some identified themselves as not being feminists, but because some did not designate themselves positively as feminists. I have chosen, then, a core group of women anthropologists who defined themselves as feminists and who had a broadly significant impact on the field of Southwestern ethnography in an attempt to identify a vein of feminism as it appeared in the first half of the twentieth century in ethnographies of the region. The four on whom this study focuses—Elsie Clews Parsons, Ruth Benedict, Gladys Reichard, and Ruth Underhill—have been chosen because they represent a community of women scholars who maintained among them—despite personal differences—an ongoing community beyond their shared graduate training.<sup>79</sup> They were all participants in Boas's agenda of promoting cultural relativism and in carrying on salvage ethnography. Further, they all identified themselves positively as feminists. They represent different generations in the development of the field; Parsons worked in the region beginning in the 1910s, Benedict in the 1920s, Reichard in the 1930s, and Underhill in the late 1930s and 1940s. Their research focuses on most of the important cultural groups in the region: the Pueblos (Parsons and Benedict), the Apaches (Benedict), the Navajo (Reichard) and the Piman (Parsons and Underhill). That they created

a general theory about women in Southwestern Native American cultures based on studies of such vastly differing cultures indicates the existence of a more general agenda and preconception on the part of these scholars. Finally, these four scholars were chosen because they represent the variety of career paths that women anthropologists took throughout this period.

While the majority of male anthropologists who completed doctorates in anthropology went on to academic careers, the careers of women anthropologists working in the American Southwest from the turn of the century to the Second World War followed four main paths. The first path was that of the informal scholar, a mostly self-trained researcher who worked on the fringes of the academic world, making significant scholarly contributions but marginalized as a nonprofessional anthropologist and sometimes condescendingly referred to as a "hobbyist." The second path was that of the traditional academic, a full-time professorship at a research institution and similar opportunities to those afforded male Ph.D.s with comparable backgrounds. The third path was that of the grant recipient, a scholar whose research depended almost wholly on the availability of independent research funds outside an institution. The fourth career path was that of the civil servant, a scholar who took a research position with a governmental agency and produced texts on the government's behalf. In this study, four women anthropologists represent each of those paths of experience.

Elsie Clews Parsons's career perhaps best represents the informal career path. While Parsons was trained in sociological fieldwork, she picked up her training in anthropology less as part of a professional program of study than through experience and participation in professional organizations. Matilda Coxe Stevenson could also be classified as an informal scholar, although neither Parsons nor Stevenson produced findings that deserve to be lumped together with the more transitory travelers' accounts of the Southwest produced by others who might more properly be referred to as "hobbyists."<sup>80</sup> Rather, Parsons and Stevenson were talented women scholars who arrived on the scene before professional programs in anthropology existed, or before training in such programs was available to them. Parsons, especially, fits this informal career pattern; her work in the field was often viewed

askance by family members and by her predominantly male, professionally trained colleagues, and appreciated mainly after the sheer weight and energy of her research came to light years later. Reading the correspondence between Parsons and Ruth Benedict, who fit the model of the traditional professionally trained academic career, it is sad to note that more often than not Benedict and other professional anthropologists approached Parsons for her financial assistance rather than her scholarly expertise. Only much later in Parsons career, after the publication of *Pueblo Indian Religions* and scores of other smaller studies, was she accorded full respect as an anthropologist. Still, even then she was almost universally set apart from professionally trained anthropologists like Benedict, Lowie, and Kroeber, who did very similar research to Parsons's, as a "folklorist," nomenclature that carried decreased status as a "less scientific" field of study. And throughout her career, even after she had become the first woman president of the American Anthropological Association, male anthropologists criticized her both in public and in private for her social activism and feminism, pointing to it as one reason why her research was "less trustworthy" than that of male scholars.

Ruth Benedict represents the second career path for women anthropologists in this period, of an academic career that followed the same general contours as the professional academic careers of male colleagues, yet was characterized by some peculiarities associated with gender discrimination. Benedict received a Ph.D. in anthropology from Columbia and went on to teach at Barnard and Columbia while editing a journal and producing "academic" studies. Benedict's career followed roughly the same lines as those of her male counterparts Robert Lowie and Alfred Kroeber, although her later career veered in a different direction, more in common with other female academics of her period than with males. While Kroeber—and later Lowie—left Columbia upon graduation and set out to found other graduate programs in anthropology, in this case at the University of California at Berkeley, Benedict remained at Columbia, in part because career options for a female academic were circumscribed. At Columbia, Boas fought for Benedict's inclusion as a full member of the faculty, despite objections from more traditional male faculty members. Upon his retirement, when most—including Boas—argued that Benedict should

have succeeded him as the senior professor in charge of the anthropology program, she was passed over in favor of Ralph Linton, a junior colleague with less experience.<sup>81</sup>

Benedict's career path also had something to do with the timing of her securing her degree. Those who graduated later, at the onset of the Great Depression, had increasingly fewer chances at the dwindling number of academic appointments made then. Benedict was aware of this pattern herself, and noted in an essay on women and anthropology prepared for the Institute of Professional Relations for the Women's Centennial Congress in 1940 that throughout the Depression, there were few teaching jobs and almost none of them went to women.<sup>82</sup>

Gladys Reichard represents the third career path of the grant recipient, an itinerant academic who made do with a variety of positions on the fringe of the more traditional academic career as an instructor, museum curator, and, most prevalently, soft-money funded positions and grants. Gladys Reichard's career followed this path; she was a faculty member at Barnard, an instructor at Columbia, and funded most of her research through grants and collaborations with untrained but wealthy "hobbyists" who had plenty of money to invest in field research. Ruth Bunzel, another student of Franz Boas, never secured a faculty position, and instead survived her entire career on "soft money." She told Nancy Parezo in 1985,

I came in at a time when things were opening up for women. There was no difficulty in fieldwork . . . [but] women had a tough time in getting appointments in anthropology. There were no jobs and they didn't go to women when they did turn up. . . . There was a time when I wanted to have a stable position but it wasn't in the cards. I had no illusions. I knew as a woman it would be difficult.<sup>83</sup>

Margaret Rossiter has noted this as a common pattern in the careers of women in the sciences throughout the period:

Anthropology presents an extreme case of this dependency on foundations and fellowships during the 1920s and 1930s. Since it was a small field and had few teaching positions

available, most of its younger women did important work and built whole careers on little more than a series of temporary fellowships from the NRC and SSRC. In fact, there seems to have been a tendency, in this field at least, to give the fellowships to women to “tide them over” while the few jobs available went to men.<sup>84</sup>

Franz Boas himself knew that most academic departments were unwilling to hire women, especially during the Great Depression. Throughout the 1930s, Boas appealed to wealthy benefactors to fund his students, but, as Rossiter has pointed out, “most of the actual jobs in the field in the 1930s went to men, while the women made do on short-term grants and fellowships.”<sup>85</sup>

Ruth Underhill represents the final career path for women anthropologists, that of the scholar who pursued a career as a public intellectual; in her case, she joined the federal government. The civil servant career path is in a sense a variant on the itinerant academic career, although characterized by less dependence on soft monies and a lesser degree of freedom to choose one’s research issues. Frances Densmore’s career as an ethnomusicologist for the Bureau of American Ethnology followed this path until her sad unemployment during the Great Depression functionally ended her field-research career and left her scrambling for economic survival. Ruth Underhill’s career characterized this path, as well; after her training at Columbia, Underhill went on to work for the Bureau of Indian Affairs, producing textbooks regarding various Native American cultures and culture areas to be used in Indian Bureau schools throughout the United States. Only after her retirement from government service was she able to secure an academic appointment, at the University of Denver, where she was in essence an itinerant academic for the last few years of her life.

Many of the women anthropologists who worked in the American Southwest during this period had careers that followed multiple paths, alternating from one career path to another over the course of their lives. They also shared common experiences as women scholars that differentiated their experiences from those of their male colleagues: they were more dependent as a group than male colleagues on soft monies rather than the traditional academic career path; they were less



likely than male colleagues to secure an academic position; when they worked, they were paid less than their male colleagues as a rule for comparable positions. The knowledge that women scholars generally had more winding and sketchy career paths than male anthropologists served to cement feminist anthropologists' convictions about the limitations placed upon women in patriarchal societies. These convictions, in turn, led them to focus on a critique of patriarchy in their ethnographies, sometimes at the expense of the ways in which their Native American informants saw the world.

To begin to address in detail how this critique of patriarchy played out in feminist ethnography in the region, I start with the biographies of these four feminist anthropologists. While I provide biographical overviews of each, the point of the biographies is to trace the emergence and practice of their feminist views, examining how those views came to shape the ethnographies that they wrote. In their lives as wives, mothers, daughters, and scholars, their experiences helped shape their views of women's place in society in general, and pushed them to recognize cultural constructions of women's place when looking at other cultures. These life experiences were reflected in their studies of the lives of Southwestern Native American women. Through those experiences, they crafted and honed a critique of patriarchy that led them to create a feminist ethnography of Southwestern Indians.

## Present at the Creation

Most of the earliest women anthropologists had little or no professional training; in this respect, Elsie Clews Parsons is a bit of an anomaly for her “informal” career cohort, in part because she had undergone professional training as a sociologist.<sup>1</sup> Nonetheless, from 1916 to 1941, Parsons produced ninety-five publications concerning the Southwest, ninety of which dealt with the Pueblo peoples of Arizona and New Mexico. Nothing in Elsie Clews Parsons’s family background would have led people to expect she would become a pathbreaker, although those who knew her often characterized her as something of a force of nature. Born November 27, 1875, in New York City, Parsons was the eldest child of three and the only daughter of Henry Clews, the son of a Staffordshire potter who had emigrated to the United States and founded a New York bank, and Lucy Madison Worthington, a descendent of President James Madison.<sup>2</sup>

Her life followed the main contours of a person of her social position, but Parsons demonstrated a great degree of independence. Although her family pressed her to become a debutante, she chose instead to pursue an education, attending Barnard where she received an A.B. in 1896, and then on to Columbia, where she received an M.A. in 1897, and a Ph.D. in 1899. She wrote her dissertation on “The Educational Legislation and Administration of the Colonies.” She studied at Columbia with sociologist Franklin H. Giddings, a Spencerian committed to “self-realization.”<sup>3</sup> Later, Franz Boas would

recall Parsons's "intense devotion to individual freedom," spawned in no small part during her study with Giddings.<sup>4</sup> Boas characterized Parsons's early work as "a strenuous revolt against convention . . . a purely intellectual criticism of fundamental forms of our modern ways of life."<sup>5</sup> While Boas's recollections of Parsons tended to focus on her thought, she also cut a rather memorable figure on a personal level, as the woman of high social status who haunted the halls of academia; Alfred Kroeber later recalled that "her statuesque figure floated through the seminar alcoves of the Low Library on Morningside Heights as a memorably astonishing sight."<sup>6</sup>

In 1900, a year after she completed her doctorate, Elsie Clews married Herbert Parsons, with whom she maintained an open marriage until his sudden death in 1925. With the exception of a stormy period over an affair between Herbert and academic reformer Lucy Wilson—incidentally one of the first women archaeologists to work in the northern Rio Grande region—the union was one in which Parsons found support for her academic pursuits and social activism. One marked ideological rift occurred, however, during World War I, when Herbert volunteered for service in the intelligence branch of the American Expeditionary Force; Elsie remained behind, holding to her staunch pacifist stance. Parsons spoke publicly against the war and American involvement, losing and making friends because of her position. Writing infrequently and with emotional distance, Parsons chose to pass her greetings to Herbert through her young sons.<sup>7</sup> Alfred Kroeber recalled her demeanor during the war as evidence of Parsons's commitment to personal freedom, later writing that "As late as the middle of our participation in the First World War she refused to shake hands with any member of our armed forces: she had always disapproved the gesture as a dictation, she was doubly annoyed by the hierarchical status implied by the uniform."<sup>8</sup> Her reticence deepened as her frustration with Herbert's support for the war grew.

Elsie Clews Parsons bore six children while married to Herbert Parsons, four of whom—John Edward, Lissa, Herbert, and Henry McIlvaine—survived.<sup>9</sup> She also took on the demanding role of political wife after Herbert won election to Congress representing New York. However, she continued to challenge pointless convention and

to put her own unique style forward; on one memorable occasion, she refused to speak as a congressman's wife unless she was allowed to wear her favorite color, orange.<sup>10</sup> And she insisted on remaining with the men after dinner rather than retiring from the table.

Throughout her marriage to Herbert and especially after his death, Parsons turned her seemingly limitless reserves of both energy and cash to pacifism, socialism, feminism, and anthropology. After a brief appointment as an instructor in the Sociology Department at Barnard College from 1902 to 1905, she taught graduate courses on the family and sex roles at Columbia University. She spent a good deal of her time with young radicals and intellectuals, many of whom she met at Mabel Dodge's Greenwich Village salon. Parsons wrote occasionally for Max Eastman's *Masses*, and became friends with Walter Lippmann and the *New Republic* group in 1914.<sup>11</sup> She also became involved with Heterodoxy, a feminist network in Greenwich village.<sup>12</sup> As part of Heterodoxy, she took part in discussions of feminism and women's experiences with other members of the group, including Charlotte Perkins Gilman, the author of *Women and Economics* and *The Yellow Wallpaper*; Dr. Josephine Baker, the head of the United States' first children's hygiene bureau; Grace Nail Johnson, the African-American leader and NAACP member; Elizabeth Gurley Flynn, the IWW organizer; Mabel Dodge, the author and patron; and Crystal Eastman, the copublisher of *The Masses*.<sup>13</sup>

Throughout this period of her intellectual life, Parsons wrote mostly on sociological topics, taking a special interest in the politics of sex and gender. Parson's first major publication was her 1906 study, *The Family*, which grew out of her lectures at Columbia University. In it, Parsons put forth a feminist argument that the admonitions upon women to serve as mothers and wives proved women's fitness to social and political equality with men. Because Parsons discussed trial marriage in the study, preachers decried her from pulpits, newspapers denounced her on their front pages, and the social registry dropped her name from its rolls.<sup>14</sup> In 1913, to avoid the publicity associated with *The Family*, Parsons adopted the male pen name of John Main in order to publish *Religious Chastity*, an argument against the oppression of women. Later, however, Parsons published exclusively under her own name, writing several sociological studies of women in society: *The Old-Fashioned*

*Woman in 1913*, *Fear and Conventionality in 1914*, *Social Freedom in 1915*, and *Social Rule in 1916*.

In these works, Parsons argued not only for the liberation of women, but for the free expression of the individual personality in society.<sup>15</sup> In all of these works, Parsons presented her various ideas about women and feminism, both closely tied to her ideas about individualism. Parsons did not support the suffrage movement as an end in itself, because she feared that moralistically voting women would inhibit individual expression. Parsons felt that women gained strength through maintaining social convention, and feared that voting women would use the ballot box to force social convention. Parsons predicted, "When women are given the vote, they will tend in their politics to exercise their skill in manipulating the private habits (drinking, dressing, lovemaking, etc.) of their fellow citizens."<sup>16</sup> According to Parsons, women's conventionalism resulted from the limitation of women's legitimate political voice to the protection of home and domesticity. Exposure to the world outside their homes, through work and social experience, would serve to counteract this, however. Parsons felt that granting women the vote without the accompanying increase in their social roles outside the home would result in an uninformed and overly conventional electorate. So, although she supported suffrage, she did not march with the suffragists.

Parsons's sociological writings also indicate her transition from agreement with Giddings, who championed a Spencerian idea of cultural evolution, to more Boasian views. A comparison of *The Family* (1906) and *The Old-Fashioned Woman* (1913), illustrates this transition. In *The Family*, Parsons argued that the subordination of women in American society was a holdover from "primitive" cultures, because the modern world still was threatened by women's reproductive abilities. She supported her argument with ethnographic secondary information, mostly from the cultural-evolutionist school. By the time she wrote *The Old-Fashioned Woman*, however, she had begun to question the idea that "primitivism" was a predecessor for "civilization"—the basis of cultural evolutionism.

One way in which this change emerged in her social activism was the transformation of her views concerning birth control. In 1906 she wrote, in *The Family*, of the negative situations that she termed "the

voluntarily childless marriage” and marriages that “restricted the number of children to one or two.” Of these situations, she wrote,

Unfortunately it seems to affect the classes who, for the sake of the cultural progress of the race would do well to have . . . more numerous offspring. The classes, on the other hand, who from economic and cultural points of view can least afford child-bearing are those who are most prone to it.<sup>17</sup>

As the influence of Giddings and Spencerian thought waned in her writings, she emerged as an important supporter of the mostly middle-class birth control movement; Margaret Sanger, one of birth control’s leading proponents in the era, wrote in her autobiography that Parsons had provided her invaluable and courageous support just before Sanger’s 1916 trial.<sup>18</sup>

*The Old-Fashioned Woman* also represents Parsons’s definitive break with cultural evolutionism with regard to the treatment of women. In this study, Parsons argued that all societies constrained women with restrictive taboos. In essence, Parsons concluded that the subordination of women, rather than being merely a remnant of primitivism, was a commonality across cultures. Furthermore, she argued that modern cultures were undergoing important changes with regard to gender roles and celebrated the signs of gender unrest she was able to identify.

Her friend Randolph Bourne responded to her final sociological study, *Social Rule*, appreciatively in a review in *The Dial*, stating, “Mrs. Parsons has made herself one of the few radical writers who see that central conflict between personality, which makes for life, and the interests of status, which inhibit and cramp and crush the personal life.”<sup>19</sup> Having become well known among the intellectual left in New York City, Parsons joined with several of these people to help found the New School for Social Research. The New School became a refuge for her, and she taught several courses there in its first years. However, she did not use The New School as a way to secure a permanent and traditional academic career. Instead, the New School gave her the opportunity to explore themes that would not normally be addressed in more traditional classroom settings, such as

the ethnography of sexuality and gender. By 1920, she had ceased to teach there on a regular basis.

In part, Parsons's loss of interest in teaching at the New School resulted from her newfound fascination with anthropology. At the age of forty-two, Parsons discovered anthropological research. In part, Parsons was inspired by her experiences accompanying Herbert on a 1905 congressional inquiry to the Philippines.<sup>20</sup> She had also traveled, with Herbert and alone, in the Southwest, and had become fascinated with the Pueblos. She fell in love with the Southwestern landscape, feeling that "Whether Indian or White one was fortunate indeed to live for a time in a world of such beauty."<sup>21</sup> Leslie Spier has argued as well that Pliny Earle Goddard, of the American Museum of Natural History, encouraged Parsons to undertake research on the Pueblos.<sup>22</sup> Parsons had legitimate scientific as well as personal reasons for turning to anthropological research, according to Spier: "[S]he wished to supplant earlier 'literary' recordings [of folklore] with accurate renditions, to trace their provenance and change under dissemination, to analyze them as matrices of custom and attitude of mind."<sup>23</sup>

Ethnology fascinated Parsons, who felt that doing ethnology was especially important for women, for "ethnology opens your eyes to what is under your nose."<sup>24</sup> Women doing ethnology, Parsons argued, could not help but notice patterns that affected the ethnologist in her own society. Leslie Spier, one of Parsons's collaborators, emphasized this personal impetus for ethnology in Parson's work. After she died, he wrote, "[I]t is clear in her later writing (and in conversation particularly) that her private interest remained in analysis of other civilizations for the light it would shine on our own problems of personal adjustment."<sup>25</sup> One of Parsons's biographers, Rosemary Levy Zumwalt, has noted that Parsons's turn to ethnography marked a "shift" in her concerns from "contemporary American problems to her new field, anthropology." However, Parsons did not cease to comment on contemporary life. Instead, she focused on the lives of Southwestern Indian peoples, and provided their stories as commentaries on the failings of her own society. Parsons's prior publications on pacifism, socialism, and feminism became, instead, articles and books on how Pueblo people dealt with conflict, competition and resource sharing, and motherhood.

Beginning in 1915, Parsons undertook extensive and extended field trips in the Southwest. However, despite her teaching, research, and impressive record of publication, she never was able to obtain a permanent position on either the sociology or anthropology faculties of any school. Instead, she shaped the field by serving as a leader in professional organizations and editing journals; she was president of the American Folklore Society from 1918 to 1920, president of the American Ethnological Society from 1923 to 1925, and was set to become the American Anthropological Society's first woman president when she died as a result of complications from an appendectomy in December of 1941, eight days before the conference at which she would officially take office. She was also the assistant editor of the *Journal of American Folklore* from 1918 to 1941.

Louise Lamphere has written that Parsons "remained a patron of anthropology rather than one who could directly shape its future through the intellectual training of its students."<sup>26</sup> Certainly, in 1918, she formed the Southwest Society, which coordinated and donated funds for women anthropologists' expeditions to the Southwest. Through the Southwest Society, Parsons funded the fieldwork of many of Franz Boas's Columbia anthropology graduate students as well as her own expeditions to the Southwest, some in partnership with Boas himself.<sup>27</sup> However, Lamphere's assertion that Parsons did not shape the field through the training of students is mistaken, as it omits the many students she did take under her wing, including Ruth Benedict, who studied under Parsons at the New School. In fact, it is important to point out that Benedict, though routinely identified as "Franz Boas's student," took few courses with Boas; instead, according to Gene Weltfish, she took her coursework at the New School under Parsons and Alexander Goldenweiser, transferring the credits to Columbia, where she wrote her dissertation under Boas's direction.<sup>28</sup>

In addition to teaching at the New School, Parsons forged important relationships with female graduate students in anthropology. She mentored them through extensive correspondence about both their fieldwork experience and by commenting on drafts of their writings. Often these relationships began as a financial mentoring, when Parsons or the Southwest Society agreed to fund a particular student. Yet many of these sponsorships blossomed into intellectual and even



emotionally significant partnerships that should not be dismissed. Parsons's contribution to the professionalization of these students included sharing the results and methods of her own extensive and impressive fieldwork in the Southwest.

Fieldwork was vital to Parsons's work. As a field-worker, she was indefatigable. On the one hand, extensive fieldwork earned her standing as a legitimate scientist, and may have been her ultimate answer to Kroeber's dismissal of her earlier writings as "unethnological." On the other hand, fieldwork was risky, possibly dangerous—exactly the way to challenge conventionality. In a letter to Gladys Reichard shortly after Parsons's death, Alfred Kroeber wrote that Parsons was "by nature a high grade field worker." Kroeber argued that anthropology attracted Parsons mainly because it required fieldwork. "Sociology failed to give her this opening," he wrote Reichard, "and when she happened to run across it through anthropology, it was just what she wanted."<sup>29</sup> Parsons responded to this lure with increased frequency from 1915 to 1941.

In August and again in November of 1915, Parsons visited Zuñi Pueblo, where she stayed at the Zuñi governor's house and met Margaret Lewis. Her lifelong friendship with Lewis would become one of the most important influences on her Pueblo research. Throughout 1916, 1917, and 1918, Parsons did extensive fieldwork at Zuñi, attending *Sha'alako* at the invitation of Margaret Lewis. She made side trips for comparative purposes to Laguna, where she collected folklore. These visits resulted in the publication of her "Notes on Ceremonialism at Laguna" in 1920.<sup>30</sup> Twice in 1918 she spent a total of three months at Laguna, and made a joint trip to Zuñi Pueblo with Alfred Kroeber. During this latter trip she first conceived of writing *Pueblo Indian Religion*. In 1919, she returned to Laguna Pueblo to work on a series of essays for *Man* on Pueblo mothers and children.<sup>31</sup> In late May of 1919, she went to Laguna again, for a one-month joint trip with Franz Boas. He worked on Keresan language, while she concentrated on genealogies.<sup>32</sup>

From 1916 to 1932, Parsons focused on the Hopi, and on Acoma, Laguna, Isleta, and Taos Pueblos. She spent the summer and fall of 1920 in Hopi villages, especially on the First Mesa, where a Hopi family adopted her.<sup>33</sup> During this visit, she arranged for a Hopi-Tewa man, Crow Wing, to keep a journal of the years 1920 to 1921. She

also interviewed a Hopi-Tewa woman, Yellowpine, for information on women's roles. From these discussions Parsons crafted the essays "Hopi Mothers and Children," which appeared in *Man*, and "Getting Married on First Mesa, Arizona," which appeared in *Scientific Monthly*, both in 1921.<sup>34</sup> During this extended stay at the First Mesa, she purchased from Stewart Culin the manuscripts of Alexander MacGregor Stephen, who had lived at Keams Canyon and on First Mesa from 1880 until his death in 1894, and who had kept minutely detailed notes of Hopi and Hopi-Tewa social life and ceremonies. Parsons edited, annotated, and published them in 1929 as "Hopi Tales by Alexander MacGregor," in the *Journal of American Folklore*, and in 1936, in two volumes, as *The Hopi Journals of Alexander M. Stephen*.<sup>35</sup>

In 1921 and 1922, Parsons stayed at Jemez. This extended field trip became the basis for *The Pueblo of Jemez*, her first full ethnographic study published about a single Pueblo village.<sup>36</sup> During her Jemez fieldwork, Parsons made frequent side trips to Zuñi, Isleta, Sandia, and Taos.

In 1923 Parsons focused her attention on the Tewa Pueblos, living in Alcalde, a New Mexican town two to three miles from San Juan Pueblo. Parsons sought out informants from San Juan, Santa Clara, and San Ildefonso. They met secretly, due to the social sanctions leveled against those who agreed to talk and "tell tribal secrets" to the outsider. She also made short visits to the Pueblos to follow up on what informants told her; as she put it, "to check up in a general way on maps of houses and kivas and to give me impressions I could compare with life in other pueblos in which I had lived."<sup>37</sup> She published the results of this research, and those of a later visit in 1926 and 1927, as *The Social Organization of the Tewa of New Mexico* in 1929.

In 1924, Parsons again visited the Hopi, and in 1926 and 1927—the years after her husband's death—she lived for several months on a ranch between Santa Clara and San Ildefonso. There she interviewed informants from San Juan, Nambé, and Tesuque. She also returned to Taos in January of 1926 and began research among the Pima of Arizona in December of 1926.

In 1927, Parsons did work with the Kiowa and Caddo in Oklahoma before returning briefly to San Juan Pueblo in December of 1927. After a short break in her Southwestern research, she set out to Zapotec in

Mixtla, Mexico, in 1929, continuing research there until 1933 with short visits elsewhere, including Taos in December 1931 and Zuñi in April 1932. Ralph Beals joined her in her work with the Cahita groups of Sonora in 1932. Parsons then took a rather extended break from fieldwork, publishing her findings throughout the remainder of the 1930s. In 1940 and 1941, she did research in the Ecuadorian highlands. Throughout all of her Southwestern and Central American fieldwork, she also continued various collecting trips for folklore materials.

Parsons's folklore fieldwork took her throughout the West Indies, the American South, and Nova Scotia collecting folk story traditions of the descendants of African slaves. In her work among African-origin populations, she was assisted by African-American graduate students in anthropology, such as University of Pennsylvania Master's student Arthur Huff Fauset, whose ongoing research in African-American folklore Parsons subsidized.<sup>38</sup> However, because she saw the racial chasm between Indians and whites as smaller than that between African-Americans and whites, and because she had learned to speak Spanish, she found it less difficult to conduct research without formal translators or assistants in the Southwest and Mexico. Instead she formed close relationships with a few informants. In New Mexico, she depended upon Mabel Dodge Luhan, her friend from Greenwich Village who had married a Taos Pueblo man, and Margaret Lewis, the wife of the Zuñi governor, for both direct information and access to others from their Pueblos. Her most in-depth studies were based on Taos and Zuñi research, and an examination of her field notebooks indicates that the Taos and Zuñi interviews often lasted for days apiece, while most of the other Pueblo interviews are shorter, with a more varied cast of informants, and generally more limited in terms of the scope of research.

Parsons's relationship with Margaret Lewis especially had a lasting and deep impact on Parsons's work. From her first field trip to Zuñi in 1915 until her death in 1941, Parsons maintained a correspondence and friendship with Lewis. Triloki Pandey interviewed Margaret Lewis in 1972, and Lewis said that Parsons "was a real friend of my husband and me. We always wrote to each other. . . . Although she was very talkative, we enjoyed having her with us and she was also glad for that."<sup>39</sup> Parsons's friendship extended to buying Lewis a set

of false teeth in the spring of 1932, and Lewis came to call Parsons "Lady Mine" and "Dear Lady."<sup>40</sup> Lewis was a member of the Cherokee tribe who had married a Zuñi man who later became governor of the Pueblo.<sup>41</sup> Parsons later wrote a series of unpublished articles for the magazine *Century* about her experiences in the Southwest, in which she described Lewis as the "first female governor of the Zuñi Pueblo," because Lewis practiced such influence over governmental affairs as the governor's wife and the tribe's chief interpreter. Parsons recalled that during one meeting between the Indian Agent and the governor, at which Margaret Lewis was serving as the interpreter, the Lewis's baby began crying; Parsons wrote that "she asked the Governor to take it out to quiet it. The Governor had ingratiating ways with the baby, besides he could be spared more readily than the interpreter who quite obviously was more than interpreter."<sup>42</sup>

Parsons attributed Lewis's political power to her Cherokee background, writing, "You may have guessed that Margaret Lewis was not a Zuñi woman." Lewis had come to Zuñi at about the turn of the century from Oklahoma. Parsons described her as "one of those Cherokees who are descended from three races at least. Her language, dress, and more superficial ways of life appertained to one culture; her ease and grace and suavity were of another; her hair and coloring, perhaps her grit, were Indian." Lewis had originally come to Zuñi to teach school, and found it, Parsons wrote, "a somewhat lonely life, I suspect she was snubbed by the few White people of the place. I have heard her say nothing in the world would make her marry a White man."<sup>43</sup>

Lewis also provided Parsons with introductions to those who would be willing to talk to her. Lewis's role as the governor's wife lent an air of tribal sanction to serving as an informant. This made securing information at Zuñi much easier than at the other Pueblos, which were much more secretive. Parsons argued that she had found only two major methods of approaching informants. She described these as "paying court for weeks, perhaps months, to the townspeople in general . . . or chance interviews, preferably away from the pueblo, with persons who are more or less taken by surprise and have not time to begin to entertain the fear of consequences." Parsons learned quite quickly how internal political divisions could work against securing informant cooperation; she recalled, "In San Felipe the two best informants of the

town were 'progressives' and there, they said, already under suspicion. One of them was willing to work at language, and the other to put me up in his commodious house until it was learned that I had been 'talking' to a girl neighbor whose father was a conservative, besides a mean man, and would betray them. Thereupon my landlord so intimidated the other man, his 'cousin,' that . . . I had to leave town and plan for interviews at Algodones, the Mexican settlement three or four miles away."<sup>44</sup>

Parsons also met frustration over payments for information. While she fully expected to pay informants, she often found herself embroiled in complex negotiations involving rumors of high payments to other informants, informants' beliefs that Parsons's wealth had resulted from her publication of informant stories, and actuarial calculations of the life-expectancy of "cursed" informants. A Laguna man named Wesuje, her chief informant for the Laguna colony of Pohnuati, presented Parsons with just such a dilemma. Over eighty years old and blind, Wesuje had spent his life shepherding and telling stories for the children. He also told the stories to Parsons through a schoolgirl interpreter, until, she wrote, "He stopped narrating to me because a neighbor in from Laguna told him I was paying twelve dollars a story, whereas I paid him only a dollar or two for several stories. After I had stated that my rate was the same in Pohnuati as in Laguna, and that his stories, besides were 'only Mexican stories,' the women in his family urged him to continue, but he was obdurate."<sup>45</sup>

On another occasion, Parsons was interviewing Lucinda Martinez of Taos Pueblo over the course of several days in November 1926, when a discussion about the appropriate fee began. "I read the names of the estufas," Parsons wrote in her field notebook. Martinez replied, "I would not tell you *that* for *one* dollar!" Parsons wrote that Martinez "wanted more money for her two stories! [to which Parsons replied,] Don't be greedy!" Martinez answered, "Not greedy, but if I get sick & die, I want more money to enjoy, want a silk dress." Part of the trouble was that Martinez's husband, Pedro, had encouraged her to ask for more; as Parsons wrote, "Pedro tells her I am making a lot of money from her stories."<sup>46</sup>

More of Parsons's problems in securing informants resulted from the code of secrecy that most of the Pueblo tribes held. Even while

she tried to circumvent it, Parsons respected the secretiveness that was also her most difficult challenge. She dedicated her *Taos Pueblo* to Mabel Dodge Luhan's husband, Tony Luhan, "To My best friend in Taos, the most scrupulous Pueblo Indian of my acquaintance, who told me nothing about the pueblo and will never tell any white person anything his people would not have him tell, which is nothing."<sup>47</sup> While there has been a great deal of speculation regarding whether or not Tony Luhan did serve as an informant for Parsons, it seems from her field notebooks that he did not. His wife, however, did provide Parsons with a wealth of information that she in turn may or may not have learned from her husband; as an outsider, Mabel might not have felt bound by the code of secrecy as Tony did. Ralph Beals recalled, "Elsie had collected a good bit of material about Taos, none of it from Tony." Instead, he argued, "Elsie had secured her information about Taos, the most secretive of all [of the eastern Pueblos], by getting Indians into hotels in Santa Fe or preferably Albuquerque, and bribing them into giving her information under pledge of secrecy. She did not, so far as I know, use the method of an earlier female ethnographer, Matilda C. Stevenson, of getting them drunk on whiskey."<sup>48</sup>

Parsons was aware, as were prospective informants, that those revealed as informants often faced recriminations and loss of status in their own communities. On several occasions, Parsons became embroiled in Pueblo attempts to ferret out the people who had given her information that resulted in published studies which had been read by tribal members and found offensive or inaccurate. After the publication of *Pueblo of Jemez* in 1925, for example, two Jemez Indians wrote to her that people at Jemez who had not given her information were being accused of having done so and, as they put it, "persecuted," ostensibly by persons who actually had served as informants. They asked Parsons to provide the names of those who had been informants, in the name of fairness. She replied that the book had been written for a small audience of white students of Indian history and as a record in case present-day Jemez traditions should pass away into oblivion. Most significantly, she refused to provide the names of her Jemez informants.<sup>49</sup> According to one of her colleagues, "Jemez threaten[ed] to murder her if she ever reappeared on their lands," and this was one of the reasons she set out to do field research after Jemez in Mexico.<sup>50</sup>

Similar situations surrounded the publication of *Taos Pueblo* in 1936. This time, however, Parsons responded with some concern, as her friend Tony Luhan, the Taos husband of Mabel Dodge Luhan, was accused of having told Parsons the Pueblo's secrets. Parsons had, in fact, hesitated to publish the book, because she feared that factions within the Pueblo would use it against one another; Parsons finally published it, feeling secure that no faction could use it and that no one at the Pueblo would read it anyway. Most importantly, Parsons thought, the manuscript contained no sacred information. After the controversy erupted in 1939, Parsons tried to reassure her friend Mabel Dodge Luhan, saying in a letter that from its tone seems certainly to have been written to be read aloud to the Pueblo and the tribal council, "The book is nowhere on public sale; very few people want to read it. It is not the kind of book Americans are interested in or want to read." Parsons amended her typewritten letter to Luhan, correcting the word "Americans" with the addition of the phrase, "White People," to indicate those outside of the tribe. She continued, "I don't want people generally to read it; only a few people who are interested in Pueblo history, and want it to be remembered when all the old people are gone."<sup>51</sup>

The controversy erupted after two federal government employees had told people at Taos about the book's existence. Sophie D. Aberle, general superintendent of the United Pueblos Agency from 1934 to 1943, brought the book to the attention of people in the Pueblo, and word of mouth built enough interest in the book that, in late December, Tony Mirabal and a group from Taos approached an outsider to read it aloud to the Pueblo. Mirabal later wrote to Parsons that he had asked "a government employee named James Crawford from Santa Fe to come and read it out loud because there had been much talk and many misunderstandings about all that was in it."<sup>52</sup> Even before Mirabal wrote Parsons about the reading she already had heard about it, because she wrote Mabel Dodge Luhan the next day enraged that "if the book was read aloud by a Government employee it was certainly very stupid and outrageous."<sup>53</sup>

Once the Pueblo had heard the book, the recriminations began. Most of the controversy, which raged for over a year, seemed to center on factional disputes between the "Peyote boys" and the Taos council

led by Governor Guadalupe Lucero, over several issues but especially the punishment meted out by Tony Mirabal toward two "peyote boys," representatives of one of the Taos factions. The peyote boys alleged that Mirabal, then governor, had had them flogged twenty-five strokes, an assertion that Parsons included in the text of *Taos Pueblo* and which Mirabal disputed; as more attention focused on the book, tensions erupted within the Pueblo. Tensions also erupted between the Taos council and local white authorities who had wrongfully arrested Mirabal and had been yanked up short by Commissioner of Indian Affairs John Collier, who appeared in person at Mirabal's trial to ensure that Mirabal was acquitted of the flogging charges.

Tony Mirabal dictated a letter to Mabel Luhan, which Luhan sent on to Parsons, making clear some of his differences with Parsons's accounts of Taos life. He especially objected to being cited by name in the text, especially since he argued that he had not served as an informant. He wrote, "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." In addition, Mirabal objected to Parsons's interpretations of Taos history: "Such as when you say the reason my two sons both of them have the white spot in their eyes it is because I was making fun of a girl with that kind of eyes. I have never made fun of such a girl and *you* know that I have not told such a thing to you." One purpose of writing to Parsons was to make sure that, as Mirabal told her, "At least now you know how your book is mixed up." But Mirabal also wrote to defend his honor; to Mirabal, as to the Jemez informants a decade earlier, being named an informant was a mortal insult that must be answered. He argued that Parsons should reveal the names of her true informants, in order to protect those who had not served as informants, for fairness sake. He asked, "If people have told you lies must they be protected? Must I suffer because they have told these lies and you have written them in a book? Must I lose my chance to help my people because you write these lies in a book, and they do not believe me anymore?" Mirabal saw this as a tangible threat to his continued influence and respect in the Pueblo. He concluded his letter by stating, "I do not expect any sober Indian to come and accuse me to my face of the things you have written in your book but I must tell you that if a drunken Indian comes and says these



things to my face and I lose my temper and kill him I will not be to blame but you and your book will be to blame."<sup>54</sup>

For breaking their silence with an outsider, informants feared punishment from both otherworldly and mundane adversaries. While informants often stated curses and threats to their health prevented them from talking, they also just as often stated that they feared everyday retribution and rebukes from other members of their Pueblos. In Taos, on her second visit with him, a male informant told Parsons that "his wife did not want him to talk . . . and besides, 'some smart boys,' as he called them, had advised him against talking to me, and, he might have added, frightened him."<sup>55</sup> Responses of this sort led Parsons to seek other ways to secure informants, using psychological manipulation to convince informants that they were not telling her anything she did not already know, as well as luring them away to secure neutral places where they could inform without their activities being known. "The only way to learn something from a Pueblo Indian, as from the secretive elsewhere," Parsons concluded, "is to know something else."<sup>56</sup>

As a result, many of Parsons's published tales read like detective fiction, with informants scurrying around hotels and train stations in disguise in search of Parsons. "The following notes," one collection reads, "were made during . . . an interview of several hours at Lamy with a Santo Domingo man who succeeded in eluding his pottery selling colleagues between trains and joining me in a room, off the station's patio."<sup>57</sup> Another reads, "The following notes were made . . . at interviews with an Isleta woman at Albuquerque, in a hotel room, safe from observation."<sup>58</sup> Often, she had to make elaborate arrangements, as she did during her 1923 trip to research the Tewa Pueblos. Parsons recalled, "I settled in Alcalde, the Mexican town two or three miles north of San Juan, and, here, thanks to my helpful and understanding hosts of San Gabriel ranch, I secured informants from San Juan, Santa Clara, and San Ildefonso."<sup>59</sup> Such arrangements produced uneven results; the information given depended in each case on a single informant. Parsons had great difficulty in securing corroborative informants from any of the Pueblos. Parsons recalled, "The San Juan informants were by far the best, being intelligent and scrupulous—the man the most accomplished teller of folktales I have met in any

pueblo. Not merely was his memory excellent, but he was an artist, a great artist, with feeling for values, humorous and dramatic, yet using with fidelity as well as with resourcefulness the patterns of his narrative art and of his daily life." This informant's stories were published in the *Tewa Tales*. In contrast, Parsons recalled, "Information from San Ildefonso was least satisfactory. The women were particularly timid and not well informed; the man was a threefold liar, lying from secretiveness, from his sense of burlesque, and from sheer laziness. Curiously enough, this man, whose social position is of the best, but whose veracity is of the worst according to both white and Indian standards, has probably been hitherto one of our best sources of authority on the Tewa."<sup>60</sup>

Further, Parsons expressed concern on several occasions that even when informants told secrets or told the truth they came from cultures which were no longer culturally unadulterated. In 1918, she speculated that "fifty percent of Zuñi culture may be borrowed from White culture."<sup>61</sup> She determined this even if her Zuñi informants were unaware of it or denied it. She wrote in 1916 of an argument with an informant over whether his belief was "Mexican" or Zuñi.<sup>62</sup> She later argued that many folktales from Acoma, Laguna, and Zuñi were "probably of Spanish provenance," though she provided neither argument for nor evidence of it.<sup>63</sup> She also argued that one of the chief difficulties of fieldwork was mutual suspicion between herself and informants, which she referred to as "like Alice on the croquet field."<sup>64</sup>

Parsons found impediments to going into the field came not only from tribes unwilling to have her investigating them, but also from her family, especially as she grew older. Ralph Beals, who accompanied Parsons on her fieldwork in Mexico from 1933 to 1935, recalled that Parsons often had to make elaborate plans to evade family members who objected to her field trips as unsafe. Beals later recalled that "her family always raised objections—she was getting too old to be trusted alone!—so she would unobtrusively slip off from Connecticut to New York or vice versa, taking only an overnight bag and having stashed away other items she wished to take previously—and simply take off, leaving no word where she was going." These sudden disappearances might well have caused her children some concern, which Parsons dismissed; instead, Beals remembered, "Some weeks

later she would write and tell her family where she was and how long she intended to stay there, forbidding them to bother her except in extreme emergencies."<sup>65</sup>

Further, Parsons would not tell anyone, including her children, when she might return, in case they might pressure her to return before she was ready. According to her daughter Lissa, Parsons's two aides and her full-time secretary, Miss Nagee, "were under instructions never to let any of her friends know and not to let us know, when she was returning from one of these trips." Rather than finding this irritating, Lissa recalled, "This absolutely fascinated me." Lissa soon learned that there were, as she remembered, "three people who did know when she was coming back, and who had advance word"—"Mrs. Crockett, who came and did her face, and the nice woman who came and did her hair, and a third one who came and did her nails." These women always knew beforehand when Parsons would return, and, Lissa remembered, "they greeted her on the doorstep and whisked her in. And when she announced that she was back, she was all fixed up. And when mother was fixed up she was very beautiful."<sup>66</sup>

Parsons began to do more fieldwork once her daughter Lissa was in her teens, as Lissa recalled later, "because then she felt that I could run the house for her and take care of my younger brothers." Parsons's economic position provided her with opportunities to do fieldwork, and to find the privacy and time when home to write up her findings without interruptions from family responsibilities. Governesses and servants watched over the children and performed domestic work. Still, Parsons did find family life a distraction when working. Her daughter later recalled that, when at the summer house in Lenox, Massachusetts, with the children, "there was only this one sitting room and there were four lively children, and that was rather noisy." Parsons coped with this by hiring a family friend, the architect Grant LaFarge, to design a cabin for her in the woods, to which she would retreat to work. Lissa recalled, "She had no electric light up there, and no telephone, and she could get away and be quiet up there and get away from us children. None of us enjoyed climbing the hill, for one thing. . . . But she did get some privacy that way."<sup>67</sup>

Privacy was much harder to come by in the field than at home, and Parsons's experiences in the course of her fieldwork were a far

cry from her living conditions while living in New York. While working together in Mexico, Ralph Beals and Parsons lived in what Beals referred to as rather “primitive conditions,” plagued by several bouts of food-related illnesses as well as giant cockroaches and other pests. However, Parsons took this in stride; Beals reflected, “As I was to learn later, [Parsons] always wanted the best, but in the field, she translated this into the best the local environment afforded.”<sup>68</sup> This was not to say that Parsons adapted completely to her surroundings; unlike some other researchers, who endeavored to disappear into their cultural surroundings by “going native,” Parsons maintained her own individualistic style even when in the field. Beals recalled, “In the field Elsie cared very little for the local style or opinions. She wore rather plain serviceable dresses and always wore a sort of shawl over her head, fastened back from her face by two clips or pins, giving on the whole a rather Egyptian effect. Inasmuch as she could not adopt the styles and behavior of the locals, she tended to ignore them, on the grounds that whatever she did she would be conspicuous.”<sup>69</sup> To be herself in the field, Parsons believed, was the most honest approach; as Beals put it, “Elsie did not by the remotest flight of imagination ‘go native’ in the field.”<sup>70</sup>

As soon as she returned to her home in New York, moreover, Parsons immediately went to work to make herself “presentable” to her own circle. Her daughter Lissa recalled that Parsons would return from her field trips, “looking, really, as my grandmother said, perfectly dreadful, ‘scandalous,’ my grandmother said—because she wore khaki clothes and she had on an old felt hat and she was touching up her hair and of course it hadn’t been touched up while she’d been away, so she wore a bandanna tied around it, and then these saddle bags full of manuscripts.” Even Parsons’s luggage drew comment for its shabbiness: “‘Disreputable’ was the word my grandmother would use, referring to the luggage. She would say, ‘Your mother is here, and that disreputable looking luggage in the front marble hall belongs to her.’”<sup>71</sup>

Parsons was swift to criticize women who had as their sole function in life their grooming. Writing about her female relatives in a letter to her husband, Herbert, she said that their whole function in life was to “bathe, curl, anoint, powder, manicure etc., and think about dress all day long.”<sup>72</sup> While good grooming should not distract

a woman from her more important pursuits, which, in Parsons's life, meant her fieldwork and research, it was nonetheless important.

Still, despite the fact that Parsons immediately resituated herself as a society figure upon her return to New York, she had relatively simple tastes in life, which may have explained her adoration of the Southwest and life there. Gladys Reichard, whose work in the field Parsons encouraged and funded, recalled that Parsons "used to say that her idea of complete comfort was to have *at the same time* a cigarette, a cup of coffee, and an open fire. And characteristically she added quietly, 'You know it is very hard to get all three together. It is easier among Indians than among ourselves.'" <sup>73</sup> In all, those who remembered Parsons in personal rather than professional terms recall a woman with a subtle sense of humor, a strong will, an intense determination in pursuit of her science, and an open mind concerning most things. She had a real ability to laugh at herself, as Beals remembered. Beals recalled, "However irritated, she was always able to see the viewpoints of others and, after a short time, to laugh at herself." While Beals and Parsons did fieldwork together, there was some friction caused by Parsons teasing Beals about his conventional manners, such as moving to the curb side of the sidewalk when walking with her. Beals recalled she enjoyed "making a good women's lib issue of it." Beals was "a little irked" because he did so as a result of his upbringing, but he bided his time to respond. Beals waited until a shared meal, and then, knowing that "Elsie smoked cigarettes rather heavily, always in a long holder," found his opportunity to respond. "After the meal she put a cigarette in her holder and held it patiently for me to light it for her. After a time she said impatiently 'aren't you going to light my cigarette.' I had turned the tables and ridiculed her inconsistency in expecting such service. She was really abashed I think. She apologized and admitted she did like some courtesies and should not ridicule those she did not appreciate."<sup>74</sup>

Parsons's aversion to convention for convention's sake did not dissuade her from seeking traditional avenues to respect in her field of research; she served as an officer in numerous scholarly organizations, and it is a great testament to her ability to put herself forward as a scholar even without an academic affiliation that she was elected as the American Anthropological Association's first woman president in 1941.

Parsons's interaction with Beals and other anthropologists—especially women—indicated her simultaneous possession of great social status and somewhat lower professional status. This conflicting identity created a complexity of social situations that shed light on the sometimes confusing exchanges which took place between Parsons and others in her field. Many of her interactions with male colleagues, especially early in her anthropological career, were overshadowed with gender attitudes, such as male colleagues' dismissal of Parsons's research as "feminist propaganda," as Alfred Kroeber labeled it in 1916, or as merely personal and therefore unscientific.<sup>75</sup> Her interaction with female colleagues, however, was more complex. Many of those who went on to become professors during her lifetime—Ruth Benedict, for example, and, to a lesser extent, Gladys Reichard—Parsons had introduced to the field of ethnology. Further, Parsons had funded most of the women's research. However, because she remained an independent scholar, she maintained a position of lower academic status. This independent status became even more significant in a period during which academic and professional credentials were becoming increasingly important in the field of anthropology. In this sense, then, Parsons's status was always in flux: at one moment, she was the benefactor to whom female scholars owed patronage and appreciation, she was a founder of the field, she was a respected scholar and officer in several scholarly organizations; at the next moment, she was the "hobbyist" and independent scholar, she had no doctorate in anthropology, she seemed to put her activism ahead of her science. For this reason, Ruth Benedict could write alternately of Parsons as her friend and patroness and as her pedantic critic. Parsons's relationship with Benedict was perhaps the most illustrative of Parsons's fluctuating status.

Parsons introduced Ruth Benedict to anthropology in a 1919 course at the New School for Social Research on "Sex in Ethnology." Further, according to Barbara Babcock, "It was she who persuaded Boas to take on Benedict as a graduate student."<sup>76</sup> Thus, Parsons felt free to reveal that she was critical of Ruth Benedict's turn to psychology with the publication of *Zuñi Mythology*. Parsons argued in her responses to *Zuñi Mythology* that psychological approaches were useless unless they included analysis of how many people in any culture displayed the same psychological traits. Writing that "psychological interpretation without accompanying analysis of distribution is ever precarious,"

Parsons criticized the more general emergence of a psychological outlook in Benedict's work.<sup>77</sup> Benedict, for her part, found Parsons formidable. She wrote to Margaret Mead in 1928 about Parsons's response to the delivery of Benedict's paper on "Psychological Types in the Cultures of the Southwest" at the Twenty-third International Congress of Americanists in New York. In this paper, Benedict introduced her discussion of Apollonian and Dionysian types in the Southwest, and, she told Mead, "Elsie was speechless and rose to make all sorts of pointless addenda when she recovered her breath."<sup>78</sup> In December, Benedict completed the paper for publication in the conference proceedings, and "had to leave it this week with Elsie. She went over it carefully and made suggestions." Parsons had commented to Benedict that it had become "a very good paper," and Benedict wrote to Mead, "Pretty good to fetch conviction from Elsie on so alien a point."<sup>79</sup>

That Benedict felt it necessary to "fetch conviction" from Parsons illustrates the indebtedness that Benedict felt toward the more senior scholar who had funded her research at Zuñi. It also reveals the degree to which Parsons's comments carried weight at the Americanist conference. But Benedict's assessment of Parsons's criticisms as "pointless" indicates as well how Parsons's status could make scholars like Benedict take her less seriously.

Despite her fluctuating status, Parsons's impact on Southwestern ethnography was wide reaching. As both patron and mentor, she shaped the research of the numerous women anthropologists who followed her. Her numerous publications became the standard for Southwestern ethnography. Further, her openness about her feminism broke ground for feminist anthropologists who came along after her. Both by watching how she was criticized for her feminism and how she negotiated and navigated provided valuable foresight to other feminist anthropologists. Her emphasis on looking at women and women's experiences cross-culturally, as reflected in her cross-cultural studies of motherhood, provided an important grounding for the critique of patriarchy that made up feminism in Southwestern ethnography. And her straightforward assertion that she studied ethnography because it showed her the realities of her own culture became the approach as well of her student Ruth Benedict and others working in the region, including Gladys Reichard and Ruth Underhill.

## The Poetic Professor

Like Elsie Clews Parsons, Ruth Fulton Benedict was—and remains—a controversial figure.<sup>1</sup> While the broad impact of her research is rarely dismissed, criticism of Benedict has become something of a growth industry among current academics. Writing jointly, Barbara Babcock and Nancy J. Parezo have indicated the prevalence of studies of Benedict that represent “a series of sympathetic or unsympathetic portraits that depict . . . a deeply disturbed personality searching for peace in the anthropological endeavor.”<sup>2</sup> Another common depiction of Benedict, Babcock notes, is as a romantic, idealistic poetess who published lyrical poetry under the name “Anne Singleton.”<sup>3</sup> According to Nancy Parezo, “Benedict’s work and life are seen as the result of an unhappy childhood,” and in part as a result of her growing deafness in adulthood.<sup>4</sup> Parezo continues, “[D]iscussions of Benedict and her contributions have focused on personal characteristics—her shyness, melancholy, deafness, generosity, and aloofness. Her personality is seen as the basis for her ideas.”<sup>5</sup> In this sense, most studies of her work have focused on ways in which her work reflects her dysfunctional personality, rather than investigating her work as part of a broader pattern of women anthropologists’ experiences or the context of the development of feminist anthropological research in the Southwest.

Benedict’s feminism grew out of her experiences as a wife as well as her experiences as an anthropologist. Once Benedict received her



doctorate in anthropology, her career followed many of the contours of her male colleagues' academic careers, but the ways in which her career diverged from the more common experiences of classmates like Robert Lowie and Alfred Kroeber are significant in illustrating some of the more generalized patterns of gender discrimination within the academy. While she could not maintain that she had been prevented from having a career, she could argue that her career had been thwarted by perceptions of her ability as a woman. After her graduation from Columbia's doctoral program, Benedict became a lecturer at Columbia in 1923. She continued teaching at Columbia on one-year appointments until 1931, when Franz Boas pressured the university to grant her an untenured assistant professorship. When Boas retired in 1936, Benedict became acting chair, and seemed poised to be confirmed as the permanent chair after a short delay. In the spring of 1936, the dean of the Graduate School, Howard Lee McBain, stated that he planned to name Benedict chair of the Department of Anthropology, remarking that "some university was going to have to make a woman chairman of a graduate department and that Columbia ought to be the first to do so."<sup>6</sup> However, when McBain died of a heart attack on May 7, 1936, the decision was delayed, and McBain's successor passed over Benedict for Ralph Linton, who became chair instead.<sup>7</sup> Benedict remained at Columbia, although she never served as the department chair. In 1937, Benedict was promoted to the rank of associate professor. Eleven years later, in 1948, after teaching at Columbia for twenty-five years, after publishing four influential books, and after her election as president of the American Anthropological Association, she became a full professor.<sup>8</sup>

In addition to her service as a teacher at Columbia, much of Benedict's career consisted of service with scholarly journals in her field. From 1925 to 1940 she edited *Journal of American Folklore*, with Elsie Clews Parsons serving as assistant editor throughout much of that period. Benedict also worked for the government of the United States, and, from 1943 to 1945, was a special advisor to the Office of War Information. In this capacity she completed research about Japanese culture that became *The Chrysanthemum and the Sword*, published in 1946. At the time of her death in 1948 she was beginning service as the director of a study of contemporary Asian and European cultures cosponsored by the Office for Naval Research and Columbia University.

Benedict was born June 5, 1887, in New York City, and from the start she seemed to be destined to become an academic. Unlike Elsie Clews Parsons's family, which discouraged her intellectual pursuits, Benedict's mother pushed her to succeed academically. Benedict's mother, Bertrice Shattuck Fulton, was a teacher who had graduated from Vassar; Benedict's father, Frederick Fulton, a doctor, died young when Benedict was not quite two years old. His unexpected death forced Benedict's widowed mother to move with Ruth and her younger sister, Margery, from teaching job to teaching job—in New York, Missouri, and Minnesota—until she took a position as a librarian at the Buffalo Public Library. But aside from the extensive mobility caused by the economic exigencies of widowhood, the death of Benedict's father also affected Ruth profoundly. Well-meaning relatives forced her to look at him in the coffin while nearby her mother wept uncontrollably in what biographer Judith Modell terms "a display of grief repeated ritually year after year."<sup>9</sup> Seeing the contrast between the memory of her father's repose in death and the ongoing vision of her mother's manic grief would, according to Modell, later lead Benedict to feature in her cultural analysis "a habit of contrasting distinct images."<sup>10</sup>

The difficulty caused by her father's death and her mother's inconsolable grief and resultant depression made Benedict's childhood grim. The family's frequent moves and Benedict's inability to penetrate her mother's grief left her isolated. Benedict's deafness, discovered by the Norwich Public School when she was five years old, exacerbated these difficulties.<sup>11</sup> Her deafness contributed with her shyness to Benedict's pronounced stammer, an impediment she would struggle with through her adulthood as well. In part because orality proved so awkward and difficult, and in part because she found that her schoolteacher mother would excuse her from household chores while she was reading or writing, Benedict turned to expressing herself in writing, in poetry, stories, and a prodigious journal she kept throughout much of her life.<sup>12</sup>

Finally settling in Buffalo, Benedict's mother enrolled Ruth and her sister Margery in the elite St. Margaret's Academy there, where teachers further encouraged Benedict's writing. She graduated from Vassar in 1909 and then spent a year in Europe with three female friends. Once she left Vassar, however, her academic pursuits met frustration. She found employment as a social worker in Buffalo in 1910, but the

job proved unsatisfying. She moved to Los Angeles in 1911 to join her sister and brother-in-law, living with them while she taught in a variety of girls' schools. Though she liked Los Angeles and Pasadena, she returned to New York in June of 1914 upon marriage to Stanley Rossiter Benedict, a biochemist who taught at Cornell University.

From 1914 to 1919 she floundered in New York, unsuccessfully seeking a career. While this was a disappointing period for Benedict, who could not find anything to hold her entire attention, it was perhaps more disappointing on a personal level than professionally. Her relationship with her husband was not what she had hoped; she wrote less than a year after her marriage, "What are our weddings . . . but presumptuous distractions from the proud mating of urgent love?"<sup>13</sup> Later in her life, she came to refer to "the perpetual lock and key of marriage."<sup>14</sup> Benedict believed that her husband had "rejected me—all of myself I valued—[which] cut the roots of my life at their source."<sup>15</sup>

Throughout this period of her life, Benedict described in her journals her fight against depressive tantrums, which she termed "blue devils." These would plague her for most of her adult life.<sup>16</sup> In order to hold the "devils" at bay, she wrote in her journal, "I have to set my mind to *invent occupation*, to make up something at every step of the way."<sup>17</sup>

Despite what she termed "brawls" between her husband and herself, the actual breaking point for the marriage between Stanley and Ruth came when she discovered that she could not have children.<sup>18</sup> Benedict had placed great emphasis on having children. In part, she yearned for a child whose development she could foster, as she had fostered the development of children through both her social work and her teaching. She also desired a child because it would prove that she was a passionate woman rather than the frigid nun she feared herself to be.<sup>19</sup> Benedict argued later that the ability to have a child would have illustrated the strength of her "animal nature."<sup>20</sup> Her inability to have children coupled with her insecurities about herself as a woman led her to record in her journal that Stanley no longer found her sexually desirable.<sup>21</sup> Benedict's overwhelming desire to bear a child, preferably a daughter, hastened Stanley's emotional and physical withdrawal from her. Victor Barnouw, her student, later wrote that Benedict recalled being "crushed" by her inability to do so, and that as a result of this blow, she later turned to the study of anthropology.<sup>22</sup>

However, the path toward anthropology was not as direct as that. Challenged by Stanley to undertake something to which she would devote herself, she set out, she wrote in 1914, "to steep myself in the lives of restless and highly enslaved women of past generations and write a series of biographical papers from the standpoint of the 'new woman.'" She intended to prove with the studies her conviction that "the restlessness and groping are inherent in the nature of women."<sup>23</sup> She chose as her topics three agitating, ambitious feminists: eighteenth-century English author Mary Wollstonecraft, nineteenth-century American transcendentalist author and social reformer Margaret Fuller, and nineteenth-century South African antiracist writer Olive Schreiner. She worked on the studies sporadically from 1914 to 1918, periodically surrendering writing in her journals in favor of working on the essays; "isn't that the best," she wrote, "without being so wordy about my conflicting selves?"<sup>24</sup> However, she found it difficult to remain focused on the studies, writing in her journal in May of 1917, "How far awry my plans have gone this year! I was to make good in writing—I've not touched it."<sup>25</sup> Her changing sensibilities throughout the period led her to obsessively revise the essay on Wollstonecraft until she had six versions on which she was working by 1918. Of the three subjects, she only completed the biography of Wollstonecraft, which she never published.<sup>26</sup>

Still, though the biographical work never resulted in a publication, it did prove significant in terms of helping Benedict to focus her feminism.<sup>27</sup> She argued in various drafts of her biography of Wollstonecraft that feminism was, rather than a system of logic, a "passionate attitude."<sup>28</sup> In fact, Benedict argued, "feminism does not live by its logic," but instead "the bright stinging realms of their dearest desires."<sup>29</sup> Through the studies of these three women, she came to argue that ambitious, achieving women had consistently encountered opposition from men and a male-controlled world. Later, under the tutelage of Elsie Clews Parsons, Benedict would equate male control with patriarchal societies. However, at this point in her writing she focused on individual men in her subjects' lives, not on the social structures that shaped their actions. She focused, for example, on Mary Wollstonecraft's relationship with her husband, William Godwin. Benedict struggled to reconcile her vision of Wollstonecraft

as a passionate woman who had, as Benedict wrote, "saved her soul alive," with Wollstonecraft's decision to marry Godwin. Benedict saw Godwin as an "estimable and frigid" man whose intellectuality she equated to "the congealing of icy and solitary logical processes."<sup>30</sup> Finally, she presented Godwin as a soulmate to Wollstonecraft, a man who fulfilled the role of companion and confidant that normally would have been filled by another woman. In this sense, she wrote in her final draft of the preface for the collection of feminist biographical essays, husbands like Godwin were the "married standard bearers" who had ensured the survival of feminism in the twentieth century.<sup>31</sup> Further, Benedict saw in Wollstonecraft's life lessons for "modern" women; she argued that only twentieth-century women, whom Benedict referred to as "we," have the ability to recognize the patterns in the "swift, whirling facts" of Wollstonecraft's life. She consciously stated that "the feminist movement needs heroines."<sup>32</sup>

Frustrated that she had been unable to see the biographical project through to conclusion, and downhearted when Houghton Mifflin rejected her proposal for the volume, Benedict enrolled in the fall of 1919 at the New School for Social Research. She sought there an outlet for her energies but also the opportunity to make some real contribution to the causes of pacifism and feminism that had consumed her throughout the First World War. She later told the American Association of University Women in 1946 that she had returned to academia to discover "what makes the U.S. a nation of Americans, France a nation of Frenchmen."<sup>33</sup> Seeking to address the broad question of identity, and avoiding such "old approaches" as those offered by John Dewey, Thorstein Veblen, and Charles Beard, Benedict focused on courses in ethnology. Benedict enrolled in a course on "Sex in Ethnology" taught by Elsie Clews Parsons. When Parsons took an extended leave, Benedict enrolled in courses taught by Alexander Goldenweiser, who had received his Ph.D. in anthropology from Columbia University in 1910.<sup>34</sup> Benedict fell under his spell, recalling the generosity with which he shared his time outside of the classroom. Goldenweiser was, Benedict recalled, a fascinating teacher full of startling insights; he was also a bit eccentric and erratic. One colleague remembered him as a romantic of a peculiarly "Germanic" sort, "who is prepared to sing an ode to Life with the intention of putting a bullet through

his head when he is finished."<sup>35</sup> Benedict found Goldenweiser's lectures so inspiring that she made special note of those rare occasions when he let her down, as she wrote during one of his classes, "2 P.M. Goldenweiser . . . Bored!! How could he!"<sup>36</sup>

Parsons, although she only taught Benedict in one course, had a profound effect on Benedict as well. Benedict read her feminist books, and mentioned them throughout her journal entries for the period. For her part, Benedict caught Parsons's attention to such an extent that Parsons took her to Columbia University to meet Franz Boas. In 1921, Benedict enrolled as a graduate student at Columbia University. Boas allowed her to transfer her course work at the New School with Parsons and Alexander Goldenweiser. This she supplemented with courses she took at Columbia, in languages, statistics, and anthropology. She sat in on most of the courses Boas taught, despite the fact that she sometimes found his style of teaching frustrating; in one of the courses she took with Boas, a seminar on kinship, she wrote across the top of her notes, "*boring*."<sup>37</sup>

Benedict's marriage had a profound effect on her feminism. Benedict remained married throughout her studies at the New School and Columbia, commuting daily by train from Long Island to Manhattan. In 1922, the strain of the commute became too great, and she took an apartment near the campus where she stayed during the week, commuting to Long Island on weekends. Still, she felt that her marriage precluded her from undertaking fieldwork for her dissertation, which created considerable tensions between herself and Stanley Benedict. She had spent a short period on fieldwork with the Serrano Indians near Pasadena during a visit with her sister in the summer of 1922; but Benedict felt a period of extended fieldwork would further strain her marriage. Boas accepted her proposal to do an extensive review of already published literature, resulting in her dissertation, a study of "The Concept of the Guardian Spirit in North America," completed under Boas's direction in 1923.<sup>38</sup> Shortly after she received her degree, Benedict's husband demanded that they live separately and asked her for a divorce. They had grown apart, and he was also in love with another woman. Fearful of being alone, she stalled the formal proceedings until 1930.<sup>39</sup>

Separated from her husband from 1923, Benedict threw herself into

her poetry—the majority of her published poems appeared during the period from 1923 to 1931—and into fieldwork in the western United States. Benedict's anthropological research among the Southwestern tribes took her to Zuñi Pueblo in 1924, 1925, and 1927, Cochiti Pueblo in 1925, among the Pima in 1927, and among the Mescalero Apache in 1931. These years of fieldwork resulted in her first book, *Tales of the Cochiti Indians*, published in 1931, as well as her two-volume *Zuñi Mythology*, published in 1935. However, of all the research, her 1927 fieldwork with the Pima had the most significant impact upon her, for during this trip she found herself, according to Sidney W. Mintz, struck by the “marked differences between Pueblo and Plains cultures and began to think of each culture as ‘personality writ large.’”<sup>40</sup> This thesis later became the core of her study, *Patterns of Culture*, published in 1934.

The idea of cultures as personality types consumed Benedict from 1927 through 1934; in fact, her feminist analysis became secondary to her psychological analysis in this period. In 1932, she wrote to Margaret Mead, “[W]hat I’m fundamentally interested in is the character of the culture and the relation of that institutionalized culture to the individual of that culture.” When Mead argued that the ongoing debate about the possibility of developing a stable classification scheme for personality types indicated some of the difficulties of such an approach, Benedict replied that she shared Mead’s analysis of “how helpless we are without stable classifications the psychologists ought to have provided us with.” Benedict argued, “It would make it neater if they had, but I don’t know that it would be any guarantee of good anthropological work in cultures.” To Benedict, it was a problem of storytelling rather than of scientific analysis: “I feel about it just as I do about a novelist’s getting down to his character with the correct motivations, etc.; it might help him to have had Freud investigate it for him first, but usually all it’s done is to take his eyes off the real person he’s describing, and it’s actually vitiated more character-drawing than it’s helped.”<sup>41</sup> Nonetheless, Benedict pressed on and published *Patterns of Culture* two years later.

*Patterns of Culture* became a best-seller and an “instant classic” upon publication. A comparison of the cultures of Pueblo Indians, Dobu Islanders, and the Kwakiutl, *Patterns of Culture* distilled each

culture into a personality type. She based her analysis of the Pueblos on her own research, as well that of other anthropologists, including Elsie Clews Parsons. Her analysis of the Dobu Islanders she based on Reo Fortune's 1932 study, *The Sorcerers of Dobu*; her analysis of the Kwakiutl came from Boas's data.<sup>42</sup> In Benedict's analysis, the Pueblos represented a placid and harmonious, almost socialistic, personality type. The Dobu Islanders, paranoid and even mean spirited, represented a fierce personality type. The Kwakiutl represented a megalomaniacal personality type. In addition to these three distillations of culture as personality type, Benedict provided vivid comparisons with other cultures. Drawing conclusions by comparison, Benedict argued that the Pueblos were "Apollonian," seeking balance in all things, while the Plains tribes, by virtue of their emphasis on warrior societies, and the Kwakiutl, by virtue of their cannibal societies and bride prices, were "Dionysian."<sup>43</sup>

While Benedict did not state the differences in gender systems between Apollonian and Dionysian cultures specifically, it is significant to point out that the Apollonian cultures she discussed were matrilineal egalitarian cultures, while the Dionysian cultures she addressed were patrilineal patriarchies. The more egalitarian gender relations in a matrilineal society, Benedict asserted, were part of what created the much sought after harmony of an Apollonian culture. In Zuñi, for example, which Benedict presented as an Apollonian culture, inheritance and identity were matrilineal. This meant that men and women had their own duties but one gender's duties did not rank above another's. Women, Benedict illustrated, "care for and feed the sacred objects that belong to them." Men, however, "learn the word-perfect ritual of their sacred bundle and perpetuate it."<sup>44</sup> Therefore, men and women needed each other to perpetuate the shared rituals in which each played a separate but equally vital part. However, Benedict also set out to illustrate the rights of women in a matrilineal society, sometimes at the expense of or in opposition to men. Referring to women within a family, she emphasized the ways in which they shared values and a woman's culture, writing that "they present a solid front."<sup>45</sup> In her discussion of Zuñi marriage practices, Benedict discussed the rights of a Zuñi woman to divorce her husband, as well as the procedures she must follow to do so. Benedict wrote that the Zuñi woman "gathers



together her husband's possessions and places them on the doorsill. . . . When he comes home in the evening he sees the little bundle, picks it up and cries, and returns with it to his mother's house."<sup>46</sup> In contrast, she did not provide any discussion of ways that a man might divorce his wife, although that too was possible. In contrast, Benedict argued that among Dionysian cultures, patriarchal and patrilineal patterns stripped women of their power and created internal conflicts by making women objects over which men and clans battled. Bidding among the Kwakiutl over bride prices, for example, created competition among men wishing to "own" the woman in question.<sup>47</sup> It also created intergenerational conflict, as bidding men set out to overwhelm the bride's father with their greatness. Finally, this competition could take on a larger dimension, when the groom's party might take up arms and raid the bride's village in order to capture her. Benedict explained, "The fight might get out of hand and people be killed in the conflict."<sup>48</sup> Thus, while presenting Apollonian cultures as a measured ideal and Dionysian cultures as societies out of control, Benedict also provided an underlying critique of patriarchy as a system.

This distillation of entire cultures into single personality types earned Benedict praise from the public and criticism from scholarly colleagues who labeled her research as overly impressionistic and reductionist. Perhaps the chasm between public praise and scholarly scorn inspired Benedict to write more for a public audience in her later work. *Patterns of Culture* certainly marked her departure from publications in more traditional scholarly journals and toward a popular audience of layreaders, a publication pattern that other women anthropologists, such as Benedict's student Ruth Underhill, would emulate.

Throughout her research during this period, Benedict enjoyed the support of the Columbia women's network, and Elsie Clews Parsons's Southwest Society funded her research in 1924, 1925, and 1927. Benedict's journals during this period suffered because she focused her energies on transcribing stories that informants told her, but her lively and intense correspondence with another of her students, Margaret Mead, compensated for the journal's lack. In letters to Mead, Benedict discussed her experiences in the field, commiserating with Mead's concurrent experiences alone in Samoa and expressing a great deal of affection and caring for Mead.

Benedict also rejoiced in the exhilaration of being in the Southwest, describing the landscape to Mead. In Santa Fe, at the beginning of her Jemez trip in 1925, she wrote to Mead using metaphors to describe her surroundings, noting, "I went up the arroya [*sic*] today straight into the mountains. There were great radiant clouds piled on top of them and shadows in their laps—I lay down on the bank."<sup>49</sup> Newly arrived in Peña Blanca, she wrote Mead,

I like this place. The Jemez range facing us across the flats of the Rio Grande is a lesser edition of a one-walled Grand Canyon. The shadows prick new contours every hour just as they did there. It is a quarter horizon's stretch of opalescent colors. And we see it across the green irrigated fields of alfalfa and corn, with a still greener line at the joining where the cottonwoods grow along the river itself. By mid-morning the clouds have formed over the range, still as mountains, and more varied, and with the constant beauty of their shadows heightening the beauty of their range.

The cumulative effect of her surroundings left Benedict feeling "as if I'd stepped off the earth onto a timeless platform outside today."<sup>50</sup>

Despite the beauty of the landscapes around her, Benedict continued to struggle with depression while in the Southwest. Much of the depression she attributed to her isolation in the field, and she offered advice to Mead in Samoa as to avoiding it. "However it seems, don't forget that you must save yourself for the field work which is bound to need all the physical fitness you can lay hold of," she told her. "Develop all the expedients you can against weeping—companionship is only one of them." The catalog of things that staved off Benedict's depressions, she wrote Mead, "ranged from brushing your teeth and gargling your throat with every onset, to playing you're your own daughter for a year."<sup>51</sup> Another expedient Benedict found in the field was long, exhausting days of transcription that left no time for black thoughts. The tedium of transcribing for up to nine hours a day and then editing as the light failed helped her keep the "blue devils" at bay; she wrote in her journal, "I can get a kind of thoroughgoing convincing relief from the devils out of a terrible chore that I don't get out

of any holiday."<sup>52</sup> The best relief, though, was her ongoing fascination with her male informants.

For the most part, Benedict chose male informants, arguing that usually men set the standards of behavior that governed a culture's personality. Throughout her research in the Southwest, she focused on male informants, and her letters to Mead are filled with references to male Indians. Often she told Mead of their physical attractiveness, referring to one in Cochiti as "stunning, with melting eyes." This particular informant, according to Benedict, had "the perfect confidence which I can't help believing has come from a successful amour with a white woman. He hopes I'll be another Mabel Dodge; he's all ready to take Tony's part and I will say he's a better catch than Tony." She finally dissuaded him by inviting a Cochiti girl to sleep beside her, after which he "took it to heart that if I let him kiss my hand six times with much heat, on departing, I meant nothing that interested him."<sup>53</sup> At Zuñi, she worked with an informant named Nick Tumaka, who served in a similar capacity to many anthropologists researching Zuñi in the 1920s and 1930s. Ruth Bunzel wrote Boas that he "is an old rascal who wants to see which way the cat jumps."<sup>54</sup> Benedict, for her part, admired Nick because he told her his stories "with fire in his eye."<sup>55</sup>

Benedict did not record what she paid Nick Tumaka for his time, although she did rejoice that "my old shaman is poor," which reduced the stigma of his taking money from her, "the poor working girl."<sup>56</sup> Like Elsie Clews Parsons before her, Benedict faced the prospect of having to pay her informants, but found she could get by with paying them much less than Parsons did. In a 1925 letter written during field research at Cochiti Pueblo to Margaret Mead, Benedict pointed out, "I pay so little here I can afford to take the tales as they come—only a dollar a three-hour session."<sup>57</sup>

Like Parsons, Benedict too had to overcome the reticence of informants who feared retribution for talking to an outsider about esoterica. Her Zuñi informant, Nick Tumaka, was later branded a witch by members of his tribe, in part because of his interaction with Benedict. After her two-volume *Zuñi Mythology* appeared in print, he was punished by being hung by his "arms behind his back over a church beam."<sup>58</sup> Benedict refused responsibility for this punishment. In fact she had argued in a letter to Margaret Mead ten years earlier that Nick

Tumaka's "too solitary and too contemptuous" manner had already caused him troubles in the tribe. It had prevented him from becoming, she wrote, "a really great man." Assessing Nick Tumaka for Mead, she wrote, "I think any society would have used its own terms to grant him as a witch. He's too solitary and too contemptuous."<sup>59</sup>

Benedict found the informants in Cochiti infinitely more difficult to draw out than Nick Tumaka. Cochiti reticence, and Benedict's conviction that their culture had become "polluted" by outside elements, led her to write to Mead, "I'm thankful it was Zuñi stories and not these I got my thousand pages of, for those are at least rich and earthy with their manners and religion, and these are rather the recreation of a people without either." Benedict lamented, "The disintegration of culture has gone further in the Rio Grande than I'd thought. It makes me more appreciative of the privilege of getting at Zuñi before it's gone likewise."<sup>60</sup>

Benedict found that one way of overcoming this reticence to share secrets was, as Parsons had also done, to get informants away from the community. She had mentioned in a letter to one part-Zuñi correspondent her intentions of getting a Zuñi informant by taking "him with me to a safely American place."<sup>61</sup> Still, she marveled that she never found "the spiked fence Elsie [Clews Parsons] talks about," or, as she wrote a few days later, "the spiked dangerous fence that Elsie, and Dr. Boas in [Cochiti's] case, make so much of."<sup>62</sup> She had a way of listening, perhaps influenced by her deafness, that made her face animated, according to Mead. Her informants responded to this. Benedict's active listening style encouraged her interaction with male informants especially. Her animated mannerisms led male informants to believe that she was responding to them emotionally. Aside from her preference for male informants, she also attracted male attentions in the field. In Cochiti, she wrote to Mead, "As soon as I go out for the water the men begin to come in."<sup>63</sup> She also recorded how her male informants flirted with her; some even kissed her hands.

Benedict had less interest in female informants; she tended to view their contributions as inferior to her male informants' stories. She did not identify any female informants in her journals, with the exception of the Zuñi informant Nick Tumaka's wife, Flora, who was identified always in combination with him. After telling Mead in some detail

about several individual male informants with whom she was working, she concluded, "And of course there are women, but they are more of a piece."<sup>64</sup> She once confessed to Boas, "I am tired of working with old women."<sup>65</sup> Benedict's sincere commitment to feminism encouraged her to focus in her work on the roles and experiences of women; however, she did not find it particularly necessary to consult Indian women to do so. This contradiction indicates in part some of the complexities of Benedict as a person as well as an anthropologist.

Her emphasis on working with male informants and her disdain for female informants also indicate the challenges of Benedict's particular take on feminism. While she argued that women of differing cultures shared the confines of female identity, she nonetheless did not value all women as possessing the same abilities. Benedict might have argued that women all had the same possibilities, but that cultural norms had created differences in their actual abilities. Thus, women informants she spoke to did not possess those qualities she valued most; they were not the storytellers, because that was a male tradition rather than a female one. To dismiss women as informants because they, in the cultures she worked with, did not have the information that interested Benedict was not a contradiction with her feminist belief that women were united across cultures. In fact, it indicates that her feminism was intellectualized to the extent that the experiences of actual women—as opposed to theoretical women—were of less interest to Benedict than the structures which shaped their lives. Had Benedict encountered a woman informant who made sense to her as a leader, an executive, or even a "New Woman"—as did Parsons with Margaret Lewis, Gladys Reichard with Dezba, and Ruth Underhill with María Chona—then she might have paid more attention to that woman's experiences. But she either did not run across that sort of a woman in the field, or if she did, did not recognize her as such. It is important to remember that Benedict's experience in the field was much more limited than that of Parsons, Reichard, or Underhill. Much of her work she based on other people's fieldwork. She was a synthesizer, and as such, paid less attention to individual informants in general, male or female.

A similar contradiction appears with regard to Benedict's assessment of racial and ethnic characteristics. While in 1940 she wrote the

passionately antiracist *Race: Science and Politics*, an assertion of the unity of human cultures and people, Benedict's belief that cultures were personality types led her, at times, to make connections between a person's cultural heritage and their personality that smack of racism or ethnocentrism. For example, of one elderly male Cochiti informant, nicknamed "The Fair" because of his coloring, Benedict noted, "he has the habit of enthusiasm and of good fellowship. I'll warrant it comes from where his fair skin comes from."<sup>66</sup> In Cochiti, also, Benedict wrote, "there's a jolly old man who tells me stories and has the Spanish-American temperament transplanted bodily."<sup>67</sup>

Such attitudes were not entirely missed by contemporary Native American critics. In 1925, Jaime de Ángulo, a student of linguistics whose father was Zuñi, wrote to Benedict in horror after she had written him asking for his help in securing informants at Zuñi. In her letter to Ángulo, Benedict had discussed strategies of overcoming Zuñi reticence at sharing details about Zuñi mythology and religion. De Ángulo reacted passionately, writing to Benedict, "you have no idea how much that has hurt me." He asked Benedict, "do you realize that it is just that sort of thing that kills the Indians?" Describing the destruction of culture that resulted from the revelation of esoteric knowledge in a society that values secrecy, de Ángulo stated, "That's what you anthropologists with your infernal curiosity and your thirst for scientific data bring about." Although Benedict did not respond to his criticisms immediately, she did attempt to placate de Ángulo five years later, as she recalled, "over whiskey and soda till after midnight."<sup>68</sup>

Her critics notwithstanding, the overall effect of Benedict's work was to challenge the assumptions of cultural evolutionists and scientific racists. Her studies of cultural types indicated Boas's theory that human cultures branched like trees, developing in a multitude of ways rather than in a straight line. Likening cultures to individual personalities, as Benedict did in both *Patterns of Culture* and *The Sword and the Chrysanthemum*, provided a metaphor for cultural relativism as a celebration of individualism.

Although Benedict had a profound effect on other scholars and scholarly discourse, perhaps her most lasting legacy came through her teaching at Columbia University. Students later remembered her with a sort of mystical air, for she was a bit mystical in the classroom.

Victor Barnouw recalled, "Like most of Benedict's students, I looked up to her with a mixture of veneration and bewilderment. With her silvery aura of prestige, dignity, and charm, she seemed to be like a symbolic representative of the humanistic values of the Renaissance." In the lecture hall or alone in her office, Barnouw recalled, "Ruth Benedict sometimes had a way of talking about 'primitive' peoples as if she could see an x-ray of their souls projected upon an invisible screen before her." The overall effect could be unsettling. As Barnouw remembered, "Ruth Benedict often seemed to have a kind of private language and way of thinking which made communication uncertain."<sup>69</sup> Still, her students recalled her with appreciation, focusing on her role as a pioneer of psychological ethnography as well as synthetic studies of the meaning of culture.

More importantly for the formation of a feminist anthropology of the American Southwest, among her students were numerous women who went on to shape the ethnography of the Southwest. In addition to Gladys Reichard and Ruth Underhill, Benedict's students included Esther Goldfrank, Ruth Bunzel, Dorothea Leighton, Gene Weltfish, Dorothy Keur, Natalie Woodbury, and Kate Peck Kent. As the first woman leader of a Santa Fe Laboratory of Ethnography field school, Benedict taught women ethnographers from Columbia's program as well as those from the University of California at Berkeley and the University of Chicago. She also fought for the equal treatment and training of women both at Columbia and in the Santa Fe Laboratory. Her most important contributions to the feminist ethnographic construction of the American Southwest came from her broad surveys, such as *Patterns of Culture*, which served to draw focus to social structures like matrilineality and patriarchy, and her training of several generations of feminist anthropologists who followed in her wake.

## Listening Daughters

Gladys Reichard, whose research focused mainly on the Navajo, became well known for her studies of Navajo culture and religious practice, for her apprenticeship as a Navajo weaver, and for her encyclopedic overviews of Navajo sandpainting designs.<sup>1</sup> She was born in Bangor, Pennsylvania, in 1893, the eldest daughter of a Pennsylvania Dutch Quaker family. Her father, Noah W. Reichard, was a family physician. Her mother, Minerva Ann (Jordan) Reichard, died when Gladys was young. The Reichard household was intellectually oriented, and their father and stepmother encouraged Gladys and her younger sister to pursue higher education. After she graduated from Bangor High School in 1909, Reichard taught in a country school in Northampton County, Pennsylvania, and from 1911 to 1915 at an elementary school in Bangor. In 1915, Reichard entered Swarthmore College, and graduated in 1919 with honors as a classics major. With the Lucretia Mott Fellowship to fund her graduate study, she moved to New York City in 1919 to pursue a doctorate in anthropology at Columbia University. She had studied anthropology while at Swarthmore, and took immediately to Franz Boas when she met him at Columbia.

Almost immediately, Boas took Reichard under his wing both personally and professionally. Reichard became a sort of daughter to Boas, living for some time in his house during her studies at Columbia. As Boas's daughter Franziska recalled, "Gladys used to live with us. She



had an apartment upstairs. She had three rooms up on the second floor, in New Jersey."<sup>2</sup> Professionally, Reichard gravitated toward Boas as a mentor, although she also studied with Ruth Benedict. However, her relationship with Benedict was strained, though cordial. Benedict, for her part, usually sent "love to Gladys" in her letters to Boas, but accompanied the endearments with somewhat snide references to Reichard's less-than-scholarly "pep." Benedict did not take Reichard as an entirely serious student of anthropology. In this assessment she shared her friend Edward Sapir's opinion, who wrote to Boas that Reichard "wasn't serious enough."<sup>3</sup>

Benedict and Sapir thought Reichard was not serious enough in part because of her connections with Boas. The fact that she acted as a sort of daughter to Boas seemed to have aroused some jealousy. Further, Reichard was a very sunny person, especially in the 1920s, and went out of her way to plan department picnics and outings. David Aberle, who knew Reichard in the 1940s and 1950s, told Louise Lamphere in 1986: "There was a kind of naivete to Gladys's approach, and a simplicity of interpersonal style that was, I think, sort of put down both by women and men in the Columbia department."<sup>4</sup> Reichard also, despite her sunny disposition, had a fiery temperament when she felt she had been wronged. She crossed swords on several occasions with Benedict, most memorably over Reichard's appointment to the faculty of Barnard. This position was in essence a more stable, though lower-status job than Benedict's position at Columbia, and Benedict expressed dissatisfaction that the job had not gone to her when Boas offered the position to Gladys. Boas argued that as Benedict was still married to Stanley, Reichard, who was single, needed the job more. Benedict apparently held what she perceived as usurpation against Reichard. Benedict was bitterly disappointed, and wrote in her journal on the day she learned of Reichard getting the job, "Worst sick headache I've had in years. . . . I suppose it's hanging on to the idea that I can teach at Barnard."<sup>5</sup> When Reichard left to take a Guggenheim Fellowship in Germany from 1926 to 1927, Benedict took over her position at Barnard, but was obliged to give it up again when Reichard returned. Benedict returned to her unpaid lecturer position at Columbia, and would have been a rare person indeed if she did not harbor some resentments toward Reichard for the differences in their situations. When she finally did secure a

permanent position at Columbia in 1931, Sapir wrote her that it marked “a modest and criminally belated acknowledgement of your services.”<sup>6</sup> Sapir shared Benedict’s frustration over the delay in her securing a stable position. Thus, she expressed her dissatisfaction in a downplaying of Reichard’s intellectual abilities.

Boas, however, thought Reichard was extremely bright. While she had intended to study cultural change, Boas encouraged her to focus instead on linguistics. Reichard completed her Master of Arts degree in 1920. From 1920 to 1921 she assisted Boas in his classes at Barnard College and taught at the Robert Louis Stevenson School in New York City. In 1922 and 1923, she carried out a year of fieldwork on the grammar of the Wiyot Indians in California; this research became her dissertation, which she completed in 1925. In 1923, Reichard served as an instructor of anthropology at Barnard, and stayed there the rest of her life, becoming an assistant professor in 1928, and professor of anthropology in 1951.

However, as one of two anthropologists rostered at Barnard, she was generally unable to work with graduate students, and research was not a part of her job. Her stature as a faculty member of an undergraduate college was less than that of her male counterparts in graduate school, like Kroeber, Lowie, and Sapir, who went on to teach at graduate universities in large departments of anthropology. These colleagues at major universities—including, after 1931, Ruth Benedict—produced graduate students who went on to champion their work. In addition, Reichard was one of two anthropologists, and taught in the Department of Social Sciences, rather than a formal Anthropology Department. This meant that even within Barnard she led a somewhat marginalized existence. As a faculty member of a women’s college, albeit one attached to a very well-respected university, Reichard’s status was further diminished. Most importantly for her research, however, her job at Barnard was to teach. She was not specifically expected to do research, and Barnard made no provision of funds for their faculty to undertake research. In order to do research, then, she was wholly dependent on outside grants.

Nonetheless, Reichard secured grant-based funding to pursue extensive fieldwork with not only the Navajo but other groups as well. From 1923 to 1955, she undertook numerous field trips among the Navajo,

focusing on social organization, linguistics, religion, and aesthetics. Her earliest research was funded by Elsie Clews Parsons and the Southwest Society, but—especially after the 1920s—part of this fieldwork was funded by the Bureau of Indian Affairs, for whom Reichard wrote a Navajo orthography and founded the Navajo Language School in 1934. In 1926 and 1927 she received a Guggenheim Fellowship to study Melanesian design through museum collections. In 1938, she undertook fieldwork focusing on Coeur d’Alene folklore. From 1940 to 1955, she spent her summers based at the Museum of Northern Arizona, and conducted fieldwork among the Navajo from there. In 1954, the year before her death, she undertook fieldwork to investigate Salish linguistics. Reichard’s grant record was considerable, especially compared to Ruth Benedict’s.

Throughout the 1920s, Benedict had sought grants and fellowships not only to fund her work but because they carried great professional prestige. The National Research Council fellowships, which had been developed in 1923 and funded by the Rockefeller Foundation, became an important avenue to professional legitimacy. Margaret Mead had received an NRC fellowship to fund her research in Samoa. In 1924, Ruth Benedict applied for funding, but was told that she was over the age limit. Even though Elsie Clews Parsons interceded on Benedict’s behalf and argued that Benedict, although one year over the thirty-five-year age limit, deserved consideration, the NRC refused to budge. Explaining its decision to Parsons, who then forwarded their letter to Benedict, the NRC fellowships were intended to allow for the transition from graduate study to full-time status as a faculty member. “It has been our experiences,” the NRC wrote Parsons, that a person not yet in a faculty position by the age of thirty-five “is not very promising material for development.”<sup>7</sup> Benedict faced another refusal in 1926 from the Social Science Research Council, and in the same year from the American Council of Learned Societies. Even in the same years as she was turned down for these fellowships, Ruth Bunzel received an SSRC grant, and Boas received an ACLS grant. In 1926, as well, Reichard received her Guggenheim grant. After 1926, Benedict did not apply for grants.

Reichard’s success with grants resulted in part from her close association with Boas. Further, she built alliances with senior figures in the

field, like Boas and also Elsie Clews Parsons, who had some influence and acclaim and who could write letters on her behalf that impressed grants selections committees. Her alliance with Pliny Earle Goddard, curator of ethnology at the American Museum of Natural History, further provided Reichard with a high-status research partner, which increased her chances of receiving funding. Further, she sought funding for a variety of projects for which she was uniquely suited. When the Bureau of Indian Affairs was funding innovative approaches to education in the early 1930s, Reichard secured BIA funds to develop her experimental Hogan School. In the school, founded in 1934, Reichard and her interpreter, Adolph Bittany, aimed to teach Navajo students how to write the Navajo language using the system that Reichard had devised. The school had eighteen students in 1934, and situated on Maria Antonia's home settlement area, Reichard could continue her work with Miguelito and Maria Antonia. She encouraged the participants to use Navajo and her writing system to write down their own history, thus collecting testimony from the students while teaching them Navajo.<sup>8</sup> Thus, by providing a vital service to a funding agency, she was able to subsidize her own research as well. In 1934 as well, she also taught an in-service course in applied ethnology for the Bureau of Indian Affairs.<sup>9</sup>

Reichard's first experience in the field came in a 1923 field trip to the Navajo reservation funded by Elsie Clews Parsons.<sup>10</sup> In this first trip, Reichard took along her sister Lilian, a photographer whose photographs illustrate several of Reichard's books, including *Dezba*. Reichard and Lilian also took along an old Ford motorcar purchased in Farmington, New Mexico, with funds from Parsons. She wrote Parsons about the car, "We have named her Elsie . . . but like naming a baby, her name is appropriate in almost no respect. She is more temperamental than a movie star & keeps Peggy busy cranking her (altho she has a self-starter), but when she does start she goes like the wind."<sup>11</sup> This car became a valuable asset, not only for traveling around, but for repaying her informants with favors of transporting them to and fro.<sup>12</sup> In 1924, she returned to the Navajo reservation with Pliny Earle Goddard. Her itineraries, especially when traveling with Goddard, were extensive during these summer months. For example, in her second summer trip made in 1924, she wrote Parsons that they had

worked in Shiprock, Lukachai, Ganado, Chinle, and Gallup, and had time to make a side trip to Zuñi.<sup>13</sup> She did not divide her time evenly among the communities, however, choosing to focus on places where she found good translators handy and where she felt she could get reliable information. She wrote to Parsons, who had also funded this second trip, "We spent about three weeks in Shiprock, one at Lukachai, both places being very fertile in material and easy to work because of good interpreters." At Ganado, also, she wrote Parsons, "We had an ideal interpreter." However, Reichard told Parsons that the Chinle work seemed useless, as she took down genealogical testimony there from an informant who insisted he had nine wives and because the translator had been "indifferent." The problem of finding translators was one which plagued Reichard until she learned to speak Navajo herself. This in itself was quite an impressive feat, as the Navajo language is one of the most complex among Native American languages. These two summers of field research resulted in her book, *Social Life of the Navajo Indians*.

*Social Life of the Navajo Indians* provided an ethnographical overview of the Navajo. The majority of the study, however, focused on analysis of Navajo clans and clan groups and the differing rituals and ceremonies practiced by each. Inspired by Elsie Clews Parsons's genealogical studies of the Pueblos, Reichard recorded matrilineal and matrilocal patterns in Navajo society. Parsons liked the book a great deal, perhaps in part because it reflected her own work. Boas also thought well of the work. However, Father Bernard Haile, the man who had given Pliny Goddard the "Blue-Eyes" manuscript, reviewed it harshly in the *American Anthropologist*. Haile criticized some of the distinctions between groups and structures that Reichard had made. This reflected what became a common pattern with Reichard's work later on; she was regularly attacked by Haile and Clyde Kluckhohn on the grounds of what they perceived as factual or research errors. One example was that Kluckhohn chose to spell Navajo with an "h," and Reichard insisted the "j" was more proper. However, the "Navaho" spelling was more commonly accepted at the time, and Reichard looked eccentric for having made a case for the alternate spelling; after her death she was vindicated when the Navajo tribe officially designated its name as "Navajo." In November

of 1943, Clyde Kluckhohn wrote a letter to Reichard indicating that he found her refusal to adopt the *h* spelling as evidence of her “tendency toward idiosyncratic separatism.”<sup>14</sup>

After the publication of *Social Life of the Navajo Indians*, she turned her focus to other cultures. She received a Guggenheim Fellowship to go to Hamburg, Germany, and this trip resulted in a study of *Melanesian Design*. She then turned her attention to Coeur d’Alene language. However, Pliny Earl Goddard’s unexpected death resulted in her returning her focus to Navajo culture; upon Goddard’s death, Reichard—Goddard’s literary executor—literally inherited the manuscript for “Blue Eyes”—so-named after its Navajo informant—upon which Goddard had been working, and which Father Berard Haile had provided to Goddard.

Reichard’s relationship with Goddard had been the subject of a good deal of gossip before his death, and the reaction of Goddard’s widow to Reichard’s literary executorship brought much of the gossip into public. Louise Lamphere has drawn on interviews with friends of Reichard to illuminate this gossip.<sup>15</sup> Reichard had never married, and no references appear in her papers or autobiographical writings about any romantic relationships with either men or women. Reichard, Lamphere, claims, traveled “as a couple” when in the field with Goddard, a married man with children.<sup>16</sup> Lamphere argues that her more-than-professional connection to Goddard was underscored by the fact that he died at her house in Newtown, Connecticut. “Their relationship came to the attention of Dean Gildersleeve of Barnard College. Reichard may have been threatened with dismissal and had difficulties with promotions and benefits later because of this incident.”<sup>17</sup> As Reichard left behind no documentation or commentary upon this relationship, it is difficult to prove conclusively that the relationship was more than a close professional working relationship. What is clear about Reichard’s life, however, is that she was somewhat of a loner. She made her closest professional connections with people who were her seniors and who played the role of parents to her; she, like Benedict, insisted upon calling Franz Boas “Papa Franz.” Even in her fieldwork, she chose to be adopted as a daughter into a Navajo family. As a young woman, this sometimes cut her off from colleagues closer to her age. She wrote sadly in 1932 to Parsons, “I just want to

tell you how much I appreciate your keeping on believing in my job and the way I am doing it. I say this because you and Papa Franz are about the only ones who do."<sup>18</sup> Later in her life, when these elders died, she found herself rather isolated indeed. One of her students, Frederica de Laguna, described her as "a lonely spirit" whose "attachments were warm and true, but . . . not easily made."<sup>19</sup>

After Goddard's death, the acquisition of the "Blue Eyes" manuscript focused Reichard on Navajo history full time. The "Blue Eyes" manuscript became the foundation for her later studies of Navajo religion, culminating in her publication in 1950 of *Navajo Religion: A Study in Symbolism*. In Goddard's manuscript she found the beginnings of an overview of Navajo religion, and she set out to learn about the thought of the Navajo soon after reading it. In 1930, she began an extended field residence on the Navajo reservation. Ganado Indian trader Roman Hubbell assisted Reichard by finding her a Navajo family to live with just south of the trading post at White Sands. The head of the family, Miguelito (also known as Red Point), took her in, and her *Spider Woman* (1934) recounted her experiences among the family. She continued to live off and on with Miguelito's family, apprenticing herself to Miguelito's wife, Maria Antonia, as a weaver, and in 1937 she published a technical account of weaving, *Navajo Shepherd and Weaver*. In 1939, Reichard completed a fictionalized biography of Maria Antonia, published as *Dezba, Woman of the Desert*.<sup>20</sup>

This period of Reichard's writing marks the most important development of her critique of patriarchy. In *Dezba*, Reichard presented two figures of authority in a matrilineal and matrilocal society. These figures were Dezba, based on her informant Maria Antonia, and Lassos-a-Warrior, based on Hostiin Klah, a man-woman. Throughout the study, Reichard presented a picture of a strong woman in constant struggle against and negotiation with white cultural incursions. As Deborah Gordon has pointed out, "Dezba . . . is the figure who negotiates with whites and who constantly reminds us of the danger of involvement with whites without actively resisting their presence."<sup>21</sup> In a sense, this is an accurate assessment of Dezba. Dezba was not openly confrontational and did not show most whites her resistance. However, she did quite actively resist, although she tended to do it covertly. In fact, Reichard defined Dezba as a woman based on her

ability to stand up to the pressures of white society to assimilate. In Navajo cultural terms, Reichard pointed out, resisting change from without was one way a woman showed her leadership to be legitimate. Dezba resisted, moreover, as a mother. Within Dezba's story of resistance lay Reichard's critique of patriarchy.

Dezba resisted assimilation to white culture concerning two major issues. The first was the education of her children. Throughout the text, Dezba referred derogatorily to assimilated children who could no longer perform the duties of being Navajo, whose knots when weaving came out immediately; Dezba stated that this was the result of them having returned from white schools.<sup>22</sup> Reichard focused on the struggle of wills between Dezba and her son, John, over John's desire to follow in his elder brother's footsteps and attend white schools. Although Dezba resisted sending her son John, Reichard argued, she was soft-hearted with her sons, unable to refuse them something that they seriously wanted.<sup>23</sup> Finally, John went away to a government school. He subsequently ran away from the school twice, looking each time, he said to his mother, for something to eat. The already meager diet of the school was worsened by the fact that older, stronger boys took his food away from him. He stated that the smell of mutton roasting at a nearby Navajo camp had enticed him to run away a second time and he was taken in by a kindly family who contacted Dezba and sent him home to her. He was thin and scraggly then, and, as Reichard wrote, "now his full eyes seemed abnormally large." His eyes had changed, and "there was an unnatural brightness in them, a puzzled seeking look which had to do not only with lack of food."<sup>24</sup>

Seeing her son's bedraggled state and hearing his stories about hunger at the school, "Dezba could not understand," Reichard wrote. According to Reichard, "She had always thought the white people were rich. They had big houses. They had automobiles. From them the Navajo got such silver as they had. How could those school people be so poor?"<sup>25</sup> Thus Dezba provided the critique that Reichard herself held to be true, that the effects of white contact on Navajo people were often harsh, negative, and—from the Navajo viewpoint—unfathomable. In Dezba's logic, only the truly destitute would suffer a child to starve; why rich people would do it was utterly beyond comprehension. His exposure to white culture and society, however, caused John



to challenge Dezba's benevolent authority in increasingly disturbing ways. For example, he refused to sleep on the beds she provided him, choosing instead the bedroll he brought from school.<sup>26</sup> By doing this, he refused an obligation to her that he would have repaid in kind in Dezba's old age. He drank from a little cup he carried with them, and which he refused to share with the rest of the group, directly challenging his mother's property rights and his mother's authority.<sup>27</sup> Eventually, he disobeyed his mother's arguments, made in his best interests, and ran away to go to another government school.<sup>28</sup>

With this anecdote, Reichard illustrated the negative effect that male experience in a patriarchal society can have within a matrilineal and matrilocal society. It was not just that John challenged Dezba's authority, but that in doing so he weakened the bonds within the family. He removed his labor from the family's resources and stood alone. Reichard's critique of patriarchy was that it affected not only women, but children as well. Through patriarchal structures, children learned to compete with each other even until one might starve. Government-run schools, *Dezba* illustrated, destroyed Navajo identity in part by breaking down the fundamental gender relationships within the family. It created daughters who cannot weave, and sons who will not contribute to the well-being of their families.

Receiving a sabbatical year in 1938 and 1939, Reichard returned to White Sands, taking her Ford, "Elsie," to outlying areas on the reservation to watch and record the fire dance. Reichard had been studying sandpainting throughout her stay with Maria Antonia and Miguelito, for Miguelito was an expert sandpainter. Emboldened by her training with Miguelito, during this field trip she corrected some errors—chiefly in the use of sacred colors—in a medicine man's sandpainting. Reichard took great pride in her ability to correct the medicine man's variations from what Reichard believed to be the more traditional sandpainting, writing about it both in letters to several correspondents, including her mentors and benefactors Boas and Parsons, and in an article submitted to *The Atlantic Magazine*.<sup>29</sup>

From the mid-1930s on, Reichard focused on Navajo religious beliefs and practices, and the ultimate culmination of this was her *Navajo Medicine Man: Sandpaintings and Legends of Miguelito* (1939).<sup>30</sup> In part this interest grew out of her dual apprenticeships to Miguelito

and Dezba; watching Miguelito's sandpaintings and Dezba's weavings, Reichard began to question more and more the spiritual content of both activities. But also, Reichard turned to religious studies in order to produce work that would be more respected than her "impressionistic" studies of weaving and women's lives had been. William Lyon argues, "After 1937, her style became more belabored, probably from a conviction that generalizations should be sparingly made, and that she must maintain a respectability among other scholars."<sup>31</sup> Perhaps more importantly, however, Reichard displayed a marked aversion to making cross-cultural comparisons of the sort for which her mentor Ruth Benedict and Benedict's star pupil, Margaret Mead, had received such a critical pummeling even amidst praise. According to William Lyon, "Reichard probably believed that the Navajos were so different, so 'other than,' that comparisons with the Anglo culture, which might provide a familiar pattern of reference, were extremely risky."<sup>32</sup>

For Reichard, acceptance among the group of Navajo scholars who had sprung up around her—overwhelmingly male, and the majority of whom had come to Navajo studies after herself—was not quick in coming. One reason for this was Reichard's reputation of being, as one who knew her put it, "a little too scrappy for her own good."<sup>33</sup> Edward Sapir, Harry Hoiyer, Berard Haile, Clyde Kluckhohn, and Leland Wyman formed what was essentially an old-boys club—leaving aside the fact that most of them were "new boys" in Navajo studies—and they were unceasingly critical of Reichard's work. William Lyon argues that "she was not fully accepted because they did not approve of her scholarship [and] because she was a woman."<sup>34</sup> In addition to attacking Reichard in reviews of her work, several set out actively to discredit her research; for example, Clyde Kluckhohn wrote to Reichard in October of 1943 to discredit her chief linguistic informant, Adolph Bittany, as "a screwball."<sup>35</sup>

Reichard and Kluckhohn had a well-publicized animosity toward one another, and, as William Lyon has pointed out, her publications in the 1950s—especially *Navaho Religion*—seemed aimed directly at discrediting his general survey work and psychoanalytical approach.<sup>36</sup> Leland Wyman attempted fair-mindedly to mediate their dispute, but finally, wincing from the wounds he had received while standing between them, gave up. He wrote to Reichard, "You two prima

donnas sure keep me on the jump!"<sup>37</sup> Reichard also disliked Ruth Benedict's close friend Edward Sapir, and Benedict wrote to Margaret Mead about their friend Sapir's great success at charming the 1928 American Anthropological Association: "Nobody but Gladys could resist him."<sup>38</sup>

"Still another reason for Reichard's controversies," William Lyon argues, "is that she remained a loyal disciple of Franz Boas, and some of his other students; rather than attack him directly, they attacked her instead."<sup>39</sup> This is probably true, as illustrated by the numbers of times that her apprenticeship under Boas was mentioned in hostile reviews of her work. Her relationship with Boas was somewhat closer—and more personal—than that which most of Boas's students enjoyed. As a result of this closeness, and because Reichard never—unlike Robert Lowie and Alfred Kroeber—challenged Boas's approaches directly, Reichard was more associated with Boas than perhaps any other of his female students. Even Ruth Benedict had directly confronted Boas in print, in her use of psychological studies and typologies in her work. Louise Lamphere has argued that taking the symbolic role of Boas's daughter had benefits as well as long-reaching negative effects.<sup>40</sup> As his "daughter," Reichard had to defend his theories long after even he had changed his thinking, and her contributions were often dismissed as merely those of a lesser Boas. Though Reichard was indeed scrappy with Boas in letters and privately, she was protective of him among other scholars, especially as his health failed, and this protectiveness was often mistaken for slavish adherence to his ideas. Alfred Kroeber wrote Edward Sapir about Reichard in 1924 that her "chief fault" was her "super-impregnation with Boas" and his approaches. Her "devotion" rendered her "hard and efficient and charmless," and "saturated with the old man." It was not Reichard's style, however, that Kroeber found troubling. Instead, he argued, "She's neither quarrelsome nor dogmatic, but argument with her is useless because she has Boas lock her mind and keep the key."<sup>41</sup> In fact, Reichard's work wandered far from what Boas had taught her, and Boas himself spoke with admiration of the ways in which she had developed approaches of her own.

More than Boas had done even among the Kwakiutl, Reichard inserted herself into the culture as an active participant. When Reichard set out to do fieldwork among the Navajo, she began by apprenticing

herself to a Navajo weaver. In this technique, she built upon methodological discoveries made by Ruth Bunzel.<sup>42</sup> Bunzel had developed a methodology of apprenticeship with Native artisan informants during her fieldwork at Zuñi in 1924. Bunzel had succeeded Esther Goldfrank as Boas's secretary and editorial assistant in 1922. In this capacity, she became "increasingly interested with anthropology."<sup>43</sup> By the summer of 1924, she recalled, she wanted to "try a bit on my own" to see if she "wanted to be and could be an anthropologist."<sup>44</sup> She approached Benedict about the possibility of accompanying her into the field as her stenographer, and Benedict approved. When Bunzel approached Boas with the idea, she recalled, "Boas heard me out, snorted in his inimitable fashion and said, 'Why do you want to waste your time typing?'" Boas encouraged Bunzel instead to investigate pottery designs as artistic expressions. Boas sending an untrained person into the field in this way, Bunzel recalled, "raised something of a tempest in our little teapot." Elsie Clews Parsons, who had funded Benedict's research trip, threatened to withdraw funding if Bunzel went along as a researcher. Boas stuck to his guns, however, arguing, "Intelligence and will were what counted."<sup>45</sup> Parsons finally assented.

Without any formal training, Bunzel set out to formulate her research questions and to develop a methodology for the project. She felt, she said, that "I was really alone in a big sea and I had to swim." Believing that "Zuñi artists were not going to be any more articulate about what they were trying to do than the poets and painters I had met in Greenwich Village," she determined that direct questioning would yield little.<sup>46</sup> Instead, Bunzel decided to make papier-mâché pots and ask informants to paint sample designs on them. In addition, she would ask the informants to teach her how to paint the designs herself, in essence, apprenticing herself to the artist/informants. She would then record the meaning they attached to each design as well as the artists' manner of teaching her about them. The research from this project became the basis for her influential 1929 publication, *The Pueblo Potter*.

Margaret Mead later indicated what a methodological breakthrough this was. Bunzel's innovations included, according to Mead, "the use of papier maché models, the introduction of pots from other areas which the artist was asked to copy, a self-apprenticeship with analysis of Zuñi

teaching methods." Mead concluded, "Her method was so merged with problem that the methodological . . . was blurred."<sup>47</sup> For her part, Bunzel later said, "I was too ignorant at the time to know that I was pioneering; that I was on the frontier of a whole new kind of anthropology." Further, she wrote, "I didn't know that I was employing 'participant observation' and 'projective techniques' because I had never heard of these things."<sup>48</sup> Gladys Reichard, however, did recognize Bunzel as a pioneer, and borrowed her methodology of participant observation when she undertook her work among the Navajo.

Bunzel's anthropological career also provides an interesting counterpart to Reichard's in that, like Reichard, Bunzel depended wholly on grants in order to conduct her research. She held several adjunct teaching positions, including lectureships at Barnard from 1929 to 1930, 1933 to 1941, 1953 to 1960, and an adjunct-professor position at Barnard from 1953 to 1960. In addition, Bunzel worked as a social scientist for the Office of War Information from 1942 to 1945, and as a Columbia University senior research associate from 1969 to 1987. However, she paid for her research through grants, including Social Science Research Council funding from 1927 to 1929, Rockefeller Foundation grants from 1927 to 1929, and a Guggenheim Foundation grant from 1930 to 1932. Elsie Clews Parsons revised her initial assessment of Bunzel, and funded many of Bunzel's subsequent research trips, as well.

Ruth Bunzel based her long career entirely on soft-money positions. She told Nancy Parezo in 1985, "I came in at a time when things were opening up for women. There was no difficulty in fieldwork . . . [but] women had a tough time in getting appointments in anthropology." The Great Depression did not help, either. "There were no jobs and they didn't go to women when they did turn up. . . . There was a time when I wanted to have a stable position but it wasn't in the cards. I had no illusions. I knew as a woman it would be difficult."<sup>49</sup>

Boas was conscious that few teaching positions would be available for women. As a result, he strove to secure funding for them from private donors as well. In the 1930s, Boas appealed to wealthy people to fund his students, but "most of the actual jobs in the field went to men, while the women made do on short-term grants and fellowships."<sup>50</sup> Margaret Mead recalled, "His letters were filled with discussions of

how some small sum—two or three hundred dollars—was somehow to be raised to keep someone writing up material or to keep someone a couple months longer in the field.”<sup>51</sup> Thus, he secured grants that funded many of his female graduate students’ research, including that of Gladys Reichard and Ruth Bunzel.

For much of Bunzel’s career as a researcher, she worked with Catalina Zunie, an elderly informant who taught many Zuñi children how to make pottery. Reichard adopted a similar pattern of research. Reichard attached herself to Maria Antonia, to whom she served as an apprentice and under whose tutelage she learned how to weave. Then her teacher taught her the significance of the patterns she had learned to make.

Reichard’s decision to focus on weaving was part of her conscious desire to focus on the roles of women in the Navajo tribe. In part her interest in women resulted from the deep impact that Elsie Clews Parsons had on her, both professionally and personally. While Reichard was very private about her feminism, she did reveal it in letters to Parsons. Some of the discussion that took place between Reichard and Parsons had to do with the junior scholar expressing frustrations about gender inequities in the field. In the summer of 1929, Reichard took part in a Laboratory of Anthropology archaeological field school. She noted with frustration that the work, which was divided up among teams that had been segregated by sex, was not always equally divided. Routinely, the team of female students were asked to “clean up” between the male teams, sweeping away dust from discoveries that the males had made, rather than being taught how to make discoveries of their own. She wrote Parsons that the “main contention” of the male participants and directors

is that girls are all right, entertaining, etc. but no good in science because you can’t do anything with them. Kroeber ends all remarks with ‘Boas will place her.’ It never seems to occur to any of them that if he can, others might be able to, were they sufficiently interested.<sup>52</sup>

A similar situation had evolved in the ethnology field school that Ruth Benedict ran in 1931, and she too became embroiled in a fight to end

such discriminatory training. In 1931, Reichard also wrote to Parsons when Ruth Underhill was appointed to an assistant professorship at Columbia, "which is a grand scoop for feminism! If there is another woman in Columbia proper I don't know who it is."<sup>53</sup> Her interest in the progress of women in her field carried over into an interest in women in her research.

Reichard wrote that she "decided that learning to weave would be a way of developing the trust of the women. . . . By weaving, I could observe the daily round as the participant, rather than as a mere onlooker."<sup>54</sup> As a weaver, she contributed as a female member of the family to the family's well-being. While her rugs were not sold—one example of her weaving is in the collections of the Arizona State Museum—she participated in the preparation and dyeing of yarn. One thing that Reichard provided the family was access to her car, which she brought along with her to Arizona; she often drove members of the family around. In exchange for these contributions, Maria Antonia and her female relatives took her on as an apprentice weaver. As Reichard wrote in *Navajo Shepherd and Weaver*, "My grandmothers and sisters' showed me with unfailing patience and persistent good humor, each step in the long process of transforming wool from a sheep's back to the rug."<sup>55</sup>

Ruth Bunzel later commented on Reichard's demeanor in the field, declaring that "the quality of her relationship with the Navajo was such that for her fieldwork was never a lonely or tedious chore . . . but rather a happy homecoming to people she valued as human beings and whose joys and problems she was able to understand and share."<sup>56</sup> Reichard did, indeed, think of herself as deeply enmeshed in Navajo culture. With some pride she wrote of this connection, "I even had a 'bead,' a token that I was known to the Navajo deities who preside over lightning, snakes, and arrows [that] signifies that I have undergone a ritual which lasted five nights and days."<sup>57</sup>

In addition to her status in the field, Reichard was a popular teacher among her students. Several who studied anthropology with her as undergraduates went on to do graduate work in the field. They remarked that her practice of using her field research in the classroom had caught their attention and made them want to study anthropology further. Although Reichard did not have graduate students at Barnard,

she did hire several Columbia students as assistants, and thus mentored them as well. These Columbia students included Ruth Underhill, Kate Peck Kent, Marian Smith, and Eleanor Leacock. For these students, especially, Reichard's enthusiasm for the field of anthropology proved infectious. Kate Peck Kent recalled of Reichard, "She was fun to work for. She assumed you had a brain."<sup>58</sup> Eleanor Leacock recalled that in part she made anthropology appealing because it carried such power for Reichard herself, as "the major integrating force in her life."<sup>59</sup> Like Benedict before her, Reichard left behind a legacy of mentoring woman anthropologists, even though her teaching responsibilities did not include the opportunity to instruct graduate students.

As had Reichard, Ruth Underhill completed her Ph.D. in anthropology at Columbia University and, unable to find the academic position she desired, found employment with the Department of Indian Education in the Bureau of Indian Affairs. As a result, most of Underhill's research was funded by the federal government. Her job was to write textbooks that would be used to teach Indian children about their own historical and tribal traditions. This educational effort was to carry out John Collier's "Indian New Deal," ending termination and closing the shameful period of forcibly cutting Native American children off from their tribal traditions. As a result, Underhill's ideas about Navajo, Tohono O'Odham, and many other tribes' histories and lifeways trained several generations of Native American children in what it meant to be Indian. Still, Underhill was best known for producing the first published Southwestern Indian woman's autobiography, Maria Chona's *Papago Woman*, published in 1936. In this work of feminist ethnography, Underhill set out to elevate a woman's struggle against patriarchal culture and to draw connections between the aspirations of Anglo career women and a Tohono O'Odham woman.

Ruth Underhill was born in Ossining-on-Hudson on August 22, 1884. She was the daughter of Anna Murray Underhill and Abram Sutton Underhill, a New York City lawyer whose family first arrived in North America in 1636; she was also a descendant of Captain John Underhill, the "Indian Fighter," and she later enjoyed contrasting this ironically with her image as "Friend of the Papagos." Underhill was the eldest of four children, three female and one male, and grew up in a comfortably upper-middle-class Quaker home. She remembered that



her father was a farmer at heart and her mother a city dweller who yearned for a brownstone-front townhouse. Still, she later recalled, they made many compromises for each other and learned to live together with the aid, as she put it, of much prayer.<sup>60</sup>

Underhill had the run of her father's library, where her interest in Greek classics and the "new thought" of nineteenth-century authors like Darwin was encouraged by her father and her uncle, who tutored her in Greek at an early age. This experience later aided her in learning several languages, including Spanish and Papago. Underhill remembered her childhood as rather solitary, despite the presence of her siblings: "I had a very lonesome childhood . . . and I was always quite a foreigner to the girls' gossip in our little circle."<sup>61</sup> Underhill possessed a serious manner that made her feel isolated from other women at certain times of her life. Near the end of her studies at Columbia, she recalled, she once lived in an inexpensive apartment that shared a hall with several other young women. During an evening of especially raucous behavior on their part, her admonition, "But is it really more fun to scream?" gained her the nickname "Teacher." The offender "looked at me," Underhill later recalled, "as though I had asked why people have hands and feet. 'Were you ever a girl, teacher?' 'No!' I snapped."<sup>62</sup>

Underhill's parents enrolled her at the Ossining School for Girls followed by the Preparatory School for Bryn Mawr. Instead of attending Bryn Mawr, however, Underhill broke from tradition and attended Vassar; she graduated Phi Beta Kappa with an A.B. in English literature in 1905. She remembered the president of Vassar as "a nice portly gentleman [who] felt that his function was to prepare us for marriage."<sup>63</sup> Perhaps it was Underhill's natural rebellious streak that kept her from marrying until the 1920s.

After graduation, Underhill served in Boston as a case worker for the Massachusetts Society for the Prevention of Cruelty to Children, and this began a lifelong concern for the welfare and education of children. Underhill found social work more exciting than what she felt were her only other options—marriage or teaching.<sup>64</sup> It also provided her with an outlet for her desire to help people. After a year in Boston, she went to Europe and traveled until 1908. While she was in Europe she enrolled in courses at the London School of Economics and the University of Munich.

After she returned to the United States, Underhill worked as a social worker for the Charity Organization Society in New York City, where she lived in Greenwich Village.<sup>65</sup> In the 1920s the Village was populated by a lively bunch of radicals and feminists, and Underhill remembered that she “mingled with . . . ‘charity cases,’ Marxian enthusiasts, and psychoanalysts.”<sup>66</sup> Throughout this period she wrote occasional columns for the New York *Sun* under the title of “Seraphine.”<sup>67</sup> During World War I, from 1915 to 1918, she worked for the American Red Cross, and from 1918 until 1920 she administered an orphanage in Italy. In 1920 she returned to the United States, settled again in New York City, and published her first novel, *White Moth*.

*White Moth* concerned a young woman who had risen to a management position in business and the problems that she confronted there. Among the trials faced by Underhill’s heroine was the experience of meeting a former suitor less successful than she in the business world. *White Moth* contained a hint at Underhill’s internalized rejection of conventional women’s roles and reflected Underhill’s own ideas about women and their “place” in society. In *White Moth*, Underhill’s feminism became apparent. She presented the argument that patriarchy limited women’s natural abilities to succeed in any arena—in this case the male-dominated world of business—and emphasized the negative effects women suffered when they betrayed their natural ambitions to please men.

Ruth Underhill married in the 1920s, but she successfully shrouded most of the details from history. Her husband’s name was Charles Crawford, and the marriage produced no children. Asked fifty-some years later why her marriage had ended in divorce, Underhill stated that she “just got the wrong man.”<sup>68</sup> Underhill never remarried, and one of her friends argues that this was because “she was just too smart for the boys. They couldn’t keep up with her.”<sup>69</sup> However, Underhill did have one extended relationship while in her eighties and nineties with a professor of ancient history, Colum Gilfallan.<sup>70</sup>

After her divorce from Crawford, Underhill enrolled in the graduate program at Columbia University. She later recalled, in fact, that she had gone directly from the steps of the courthouse in New York City where she filed for divorce to the registrar’s office on the Columbia campus.<sup>71</sup> Certainly, she saw her divorce as a moment of great personal

emancipation, and shortly after it became final she celebrated with a "Crawford Unwedding Trip."<sup>72</sup>

When Underhill first enrolled at Columbia, she had no clear idea of what she should study. She later said that she really wanted to learn about "life," and she set out to discover the discipline that would best address that subject. She tramped up and down the halls of Columbia in search of a department that offered courses that she felt would explain people and their motivations. In the end, she chose anthropology because she wanted "to know more about PEOPLE" and because anthropology professor Ruth Benedict took her under her wing.<sup>73</sup> This marked the beginning of a valuable friendship between Underhill and Benedict that would last until Benedict's death in 1948.

Boas had a reputation for a domineering, paternal way of teaching that some students found irritating, but Underhill blossomed under his attentions.<sup>74</sup> Still, she did fear that Boas did not take her seriously at first. She felt that her age and her background counted against her in this area, and the fact that she "had better clothes" made her seem like she was studying anthropology for "fun." Underhill later said that she felt that Boas was "suspicious of me because he thought that I was a society lady."<sup>75</sup>

Despite any possible "suspicions" on his part, after Underhill's first year of coursework Boas funded her first trip among the Tohono O'Odham while sending her younger, mostly male colleagues to nearer and less expensive research sites.<sup>76</sup> The Tohono O'Odham were also a wonderful research topic; there had been very little fieldwork done with them, they were not traditionally hostile to outsiders, and they had lived in the same area for a great deal of time. Asked why she thought Boas had given her such a plum assignment, Underhill replied, "He had this tremendous feeling of justice and right. He felt that of the two groups that were not well treated, Jews were the first and women were the second. So, he did for the Jews and did well, and then he turned his attention to the women."<sup>77</sup> After Underhill returned from Arizona, Boas "made quite a little of it" and, with the encouragement of Benedict, funded three more trips to the Tohono O'Odham, from February to October, 1933, from November 1933 to January 1934, and from October to November, 1935.<sup>78</sup>

Underhill felt that the best of her training came from the ideological

struggles between her two advisors, Benedict and Boas. She often mentioned their contributions to her education as equal, and once remembered that “Boas and Benedict opened a door through which a light shined on me.”<sup>79</sup> Another student of Benedict’s and Boas’s, Margaret Mead, remembered that the two professors balanced and complemented each other well. Mead described Benedict as “tentative and shy and [she] always wore the same dress. She spoke so hesitatingly that many students were put off by her manner, but . . . her comments humanized Boas’s formal lectures.”<sup>80</sup> For Underhill, the tension between Boas and Benedict pulled her in two directions at this point in her research, with Boas dictating a social-political structural approach and Benedict encouraging a psychological approach.

Boas’s interests lay with political structures—who ruled and how they were chosen—and structures of social organization—which families were most prominent and how they became so. Benedict looked more at the life of one individual as it reflected the beliefs of a broader society. She was most interested in how people thought and how that reflected cultural values. For example, where Benedict wanted to know what was taught to young children, Boas wanted to know who taught them and who decided what they should be taught.

When Underhill returned from Arizona, she remembered that each had different criticisms and advice:

Boas, of course, wanted very practical things . . . something about government and social development. How long had they been there? What was the attitude toward Spain? How had Spain treated them? Lots of things like that. Ruth [Benedict] wanted to know, how did they feel? I told her a great deal about the marriage system and the bringing up of children and that was what I was interested in, too.<sup>81</sup>

The conflicting directions given Underhill by Boas and Benedict might well have upset a less mature and less determined student. But Underhill saw merit in both approaches and balanced her research accordingly. In Underhill’s dissertation, *Social Organization of the Papago Indians*, Boas’s model clearly won out. But in Underhill’s later works, *Papago Woman* among them, Benedict’s style of focusing on

interesting personalities and what they felt comes through as Underhill incorporated both approaches.

In *Papago Woman* especially, Benedict's model of seeing cultures as "personality writ large" dominated. By telling the story of one woman and presenting it as typical, Underhill set out to look at the quintessential nature of Tohono O'Odham women and Tohono O'Odham culture. Underhill did indicate that in some ways her subject, Maria Chona, was not the norm of Tohono O'Odham culture, in part because she was not intimidated by Underhill's status. Underhill felt that most "primitive" women saw the anthropologist as a threat and that this impeded the anthropologist's work. Underhill said, "So many of those primitive women think . . . either they're not so good as you are, they're not as powerful, or you represent a terrible power that they can't calculate on, so they better not confide in you."<sup>82</sup> Chona never had this reticence. Underhill recalled, "She wasn't at all afraid of me. Now, some primitive women are just afraid of somebody from another culture. She might have witchcraft or all sorts of awful things, but Chona was a person of such personal power, she felt she didn't have to be afraid of anybody." Though Underhill saw Chona as "atypical" because of this lack of fear, Underhill nonetheless saw Chona as representative of Tohono O'Odham women. She attributed Chona's lack of fear not to some cultural "deviation," but to cultural standards within Tohono O'Odham society. Chona did not fear Underhill, Underhill argued, because her age and her life experiences had shown her not to. Chona was Underhill's elder, after all, and if she, as an elder, would be unyielding with younger males, she certainly would not shy from younger females, even if they were anthropologists. As Underhill stated, "Nobody was going to put anything over on her; she was as good as I was, right to begin with."<sup>83</sup> Besides, Chona saw Underhill as clearly uneducated. She simply did not know the things that any woman ought to know, such as how to grind and prepare meal, or what menstrual huts were, or how men and women interacted. As Underhill put it, Chona "found out that I was harmless, a queer kind of bumbling creature that didn't know much. . . . Of course, I didn't know about life. Apparently I didn't; I had to be told."<sup>84</sup> Thus, Underhill chose a woman whom even Underhill indicated was "executive," but then sought out to indicate that this characteristic fit

within Tohono O'Odham cultural norms.<sup>85</sup> In doing so, Underhill supported her claim that the life of Maria Chona shed light on the lives of all Tohono O'Odham women, even those who did not adopt the same "executive stance" that Chona did. Using Benedict's standard of choosing an outstanding individual and extrapolating the culture's values and history from his or her life story, Underhill set out to present Chona not only as *a* Papago woman, but as *the* Papago woman.

In addition to the ways in which Boas and Benedict shaped Underhill's work, she also benefited from mentoring by Gladys Reichard. Reichard shared with Underhill the benefits of a participant-observer approach, encouraging Underhill to apprentice herself to a Tohono O'Odham artist. Underhill learned this technique well, and this style is especially evident in *Papago Woman*. The first thing that she asked Maria Chona was if Chona would teach her how to make baskets. From the beginning of *Papago Woman*, Underhill reflected on her own reactions to Tohono O'Odham culture, illustrating the Tohono O'Odham's reactions to her. Underhill was conscious of how this involvement affected the story Underhill wrote and the story Chona told. For one thing, her apprenticeship meant that Chona set out to teach her the proprieties of Tohono O'Odham culture, passing quickly over what she viewed as improprieties like "Wild Women," which figure only very briefly in both the text of *Papago Woman* and the field notes. Underhill argued that Chona felt free to express herself to Underhill; whether she trusted her or simply found her nonthreatening is open to debate. Thus Chona set out to teach Underhill right from wrong, and as Underhill recalled, "If I was wrong about anything she would be glad to tell me. And I always let her."<sup>86</sup>

After the completion of her dissertation, Underhill hoped to find a teaching position at a university. While she did have an adjunct position at Barnard, she was unable to secure a stable academic position. Instead, she took on several short-term assignments to tide her over. In 1934, Underhill took part in Gladys Reichard's experimental "Hogan School" on the Navajo reservation, sponsored by the Bureau of Indian Affairs. In this school, Reichard and her interpreter, Adolph Bittany, aimed to teach Navajo students how to write the Navajo language using the system that Reichard had devised. She also taught an in-service course in applied ethnology for the Bureau of Indian Affairs.<sup>87</sup>

This course began a thirteen-year career with the Bureau of Indian Affairs, mostly spent as an expert on Indian education. These appointments introduced her to officials of the Bureau of Indian Affairs at just the moment that the Bureau was trying to address growing conflicts between Bureau staff members and academic social scientists.

In late 1935, Commissioner of Indian Affairs John Collier set up the Applied Anthropology Unit to reduce BIA-academic friction. While the program, headed by Scudder Mekeel, only lasted two years, it did offer an opportunity to young anthropologists to join the Bureau while it lasted. Under its auspices, the BIA hired six young anthropologists to conduct in-service training for BIA employees. Two of these anthropologists, Sophie D. Aberle and Ruth Underhill, were women.<sup>88</sup>

In the summer of 1935, Underhill led in-service seminars at the Sherman Indian Institute in Riverside, California, and in Santa Fe, New Mexico. At these seminars, Underhill taught BIA employees about the history and cultures of tribes with whom they would be working. That fall, the Bureau charged Underhill with heading a team investigating whether the constitution for self-governance that the BIA had written for the Tohono O'Odham reflected Tohono O'Odham social and political organization. Underhill argued that it did not, and as a result, John Collier removed her from the project. In fact, Collier barred her from further negotiations between the BIA and the Tohono O'Odham. Collier reacted in such a harsh fashion to Underhill's "insubordination" for two reasons. First of all, the Indian Reorganization Act required Collier to provide self-governance and constitutions quickly, and Underhill's objections created what he saw as unnecessary delays. Second, Joyce Griffin has written that Collier removed Underhill from the head of the project because "his personal experience with professional women in administrative and executive positions had been most discouraging."<sup>89</sup> Collier expressed dismay that Underhill proposed in-depth ethnographic studies before issuing a recommendation, a delay that Collier felt would prevent his carrying out the Indian Reorganization Act. Further, her proposal of such studies, and the growing support among tribal officials for Underhill's side, indicated growing disagreement between tribal governments and the Bureau of Indian Affairs. Wishing to present a united BIA front in the debate, Collier removed Underhill, whom he viewed as something of an agent provocateur.

Barred from taking an active role in making BIA policy by Collier's order, Underhill nonetheless found work within the BIA as a writer of educational ethnological manuals that would be used to train BIA employees and Public Health Service workers. After the dissolution of the Applied Anthropology Unit in 1937, Underhill became part of the Education Division. Throughout the remainder of her career as a public anthropologist, she wrote seven pamphlets for the BIA's Indian Life and Customs Series. Several of these later became books published for a wider public readership. These studies included *Southwest Indians* (1934), *The Papago Indians of Arizona and Their Relatives, the Pima* (1941), *Navaho Weaving* (1944), *Pueblo Crafts* (1944), *Work-a-Day Life of the Pueblo* (1946), *People of the Crimson Evening* (1951), and *Here Come the Navaho!* (1953). These books, many of which were illustrated by Indian artists, were widely read. More importantly, many Indian school districts adopted them as textbooks for high school use. As a result, Underhill ended up teaching, in many cases, Native American readers her own version of their history.

Underhill's prolific publication record throughout her career is remarkable. While working for the BIA, Underhill also published ethnographical studies independently, using fieldwork conducted under the auspices of her position with the BIA to gather data. Between 1934 and 1948, Underhill published *Papago Woman* (1936), *Singing for Power: The Song Magic of the Papago Indians* (1938), *The Social Organization of the Papago Indians* (1939), and *Papago Indian Religion* (1946). After leaving the Bureau of Indian Affairs, Underhill took an adjunct position at the University of Denver. While teaching sporadically there, she continued to publish, and during this period produced *Ceremonial Patterns of the Greater Southwest* (1948), *Red Man's America* (1953), *The Navahos* (1956), and *Red Man's Religion* (1965).

Running through all of Underhill's works, but especially expressed in *Papago Woman*, was a strong vein of intellectual feminism. In her examinations of women's roles among the Tohono O'Odham, Underhill emphasized power inequities between men and women, and applauded women's abilities to negotiate power for themselves. Maria Chona appealed to Underhill because she did not bend to male authority; she had divorced her first husband when he—following Tohono O'Odham custom—had brought home a second wife. She had refused



to marry again after her second husband's death, instead establishing herself as an independent woman, living away from the constraints of family in Tucson. Underhill's work provided a critique of patriarchy, emphasizing the ways in which restrictions on women in patriarchal Tohono O'Odham society echoed those on Anglo women, and drawing from Chona's stories lessons for resistance to "take home" with her to New York.

While Underhill's feminism was well established before she entered Columbia, there she found a network of other feminist women scholars who encouraged her. At Columbia, her growing feminist consciousness—reflected in *The White Moth*, for example—became a social science focus on social structures and gender inequity. Her most important woman mentor was Ruth Benedict, who from the beginning took Underhill under her wing. Ruth Benedict introduced Ruth Underhill to an influential circle of Columbia women anthropology students, including Gladys Reichard, Margaret Mead, and Ruth Bunzel. In addition, at Columbia's student gatherings, Underhill met Parsons, who later funded much of her Southwestern research through the Southwest Society. Once Underhill began to publish her research, Parsons admitted to being "envious" of Ruth Underhill, referring to *Singing for Power* as an "aperitif."<sup>90</sup> For her part, Underhill was a great admirer of Parsons's work, especially Parsons's encyclopedic comparative study, *Pueblo Indian Religion*, published in 1939.<sup>91</sup>

Her contact with other women anthropologists, especially Parsons and Reichard, as well as her feminist interest in addressing issues of gender, encouraged her to choose women informants once she went into the field. In addition, she found it easier to talk to women informants, something which Underhill referred to as a "fortunate accident" of her methodology.<sup>92</sup> "We were just feeling our way, all of us, at first," she later said. "Little by little, then, I came to see why this business of the women is the right thing. I'm getting a lot more than I would with the men and I shall keep right on with it. Then, it became my specialty."<sup>93</sup> Asked in 1980 whether she or her mentors understood how new her methods of working with women informants were, Underhill said,

I don't think they realized how different it was. One of the boys said to me once, "Ruth, you look so harmless." I said,

"Well, don't you understand that's technique? I'm not really harmless am I?" "Oh no," he said, "I just hadn't thought of that." Because, the boys didn't think at all of technique and how they could get the stuff. They just went on and asked questions—why not?<sup>94</sup>

Underhill seemed conscious of the role that manipulation played in the gathering of ethnographic information. However, like Parsons, Reichard, and Benedict, she had certain standards and shied away from cash payments to informants. However, she would trade goods and services for sessions, for example providing transportation to Chona and her family, driving her home from Tucson to be among her kin for the interviewing process. She gave her informants' families sacks of flour and beans, and then watched in dismay as they prepared the entire bounty for a feast the same day; her New England sensibilities dictated that the supplies were meant to last for her entire visit. She went to great pains to indicate that she would not allow her information to be used by reservation officials to repress informants, especially when she was researching religious rituals for *Singing for Power*. Like Parsons before her, Underhill was aware that being an informant could be costly in a society that valued secrecy.

Underhill felt she had other advantages that made it unnecessary to use unsavory forms of manipulation. Her age afforded her a level of respect that a younger researcher might not have found; she was in her late forties when she conducted the research for *Papago Woman*. By that age, she stated,

I'd had some social training. . . . I brought the conversation around. . . . Lots of men, you know, don't know how to do that. I'm amazed at the young, brash young fellows that have no social ease at all. They just, "Now, let's see, what do you do when you want to choose a new chief?" Then they ask the questions one after the other. I would say, "Oh, you had a new chief. How did you get him?" And slowly, slowly . . .<sup>95</sup>

Underhill also argued that her gender helped her in her efforts at recording women informants. Working in a patriarchal society like that of

the Tohono O'Odham—not to mention her own and the American Anthropological Association—she also credited her Quaker upbringing. While “having been taught in my youth that women should always be rather quiet and not push themselves” helped somewhat, Underhill stated, “Perhaps it was not just because I was female but a Quaker. Quakers don't push themselves anyway.”<sup>96</sup>

Unlike Parsons and Benedict, who struggled with Pueblo “superstitious” secretiveness in their research, Underhill found less reticence about sharing most information with her. Instead, the one problem she encountered was the fear of reservation officials using her research against informants. She found it difficult to convince prospective informants that she would not get them in trouble. “I told many of them that I never told the superintendent anything, that he was not my friend. They were glad to hear that, but they didn't really trust me,” Underhill recalled later. This was not a problem in Chona's case, however, because “Chona wasn't afraid of the superintendent—what the heck if he wanted to tell her he didn't like her smoking or whatever she did; well, that was her business and she'd tell him.”<sup>97</sup> Not only was Chona not afraid of the superintendent; Chona was not afraid of Underhill. Chona did not place the anthropologist on a pedestal. For her part, Chona felt that she was training Underhill. Underhill also stated that Chona remained unawed by the actions of the federal government and the reservation agents. Referring to Washington as “Vasindone,” Chona asked Underhill, “But did Vasindone make our land? No! That was Earthmaker. In the Beginning. He make it for us—Papagos, Desert People! Bean People!”<sup>98</sup>

To read Underhill's subtext, Chona treated Underhill as if she were the first Euro-American with whom she had shared an intimacy. This may have been true, but we cannot trust Underhill's word for it. As an anthropologist, and more importantly, as the student of Franz Boas, Underhill felt that “pristineness”—isolation from the cultural adulteration brought on by intense contact with outsiders—was an important element in legitimizing native voices. The more pristine the subject, the purer, and therefore more valuable, their testimony.

Chona probably did possess a core belief that she was more Papago than any cultural hybrid. She certainly saw the Papago way as the “right” way. She would have been a rare human indeed if she did

not have some sense of superiority about her own culture. She did see the Tohono O'Odham "Land" as made by I'itoi for the Tohono O'Odham. Despite this, though, she did not express bitterness over the Euro-American territorial encroachments on Tohono O'Odham lands, instead arguing that the Tohono O'Odham had moved into the desert "To be alone."<sup>99</sup> Perhaps that tribal preference to be alone echoed Underhill's experiences as a happily solitary child, or perhaps more to the point, as a relieved divorcee on a solo "Unwedding" trip; for whatever reason, in all of her studies of the Tohono O'Odham, Underhill stressed that the Tohono O'Odham consciously chose to "retire" when other groups encroached upon their territory. She saw this avoidance of conflict as Tohono O'Odham activism rather than passivity, and she may have felt that it positively complimented her own Quaker beliefs.

Underhill strove throughout her research to find commonalities of this kind with her Native American subjects, especially with women informants. She was forever "translating" Indian civilizations into a cultural language that could be understood by the average, non-Indian, lay reader. Even her book titles, for example, *America's First Penthouse Dwellers*—a title with immediate iconic recognition for the majority of middle-class Americans—stressed commonalities of experience; here Underhill equated cliff dwelling with modern apartment living. In *Papago Woman*, she strove to establish a model of universal women's experience, despite Chona's thoughts on the matter.

To some extent, this led her to romanticize Tohono O'Odham values, especially in contrast to the values of the majority, Euro-American culture. Comparing her childhood in a traditional upper-class Anglo household to the openness of the one-room Tohono O'Odham family existence, Underhill wrote,

I remembered my own youth when, tiptoeing past my parents' door at night, I heard low voices. They were discussing us children, perhaps, our faults and what must be done about them. Or Father's financial situation. That was something about which we were never told. The result with us was a latent hotbed of curiosity and rebellion. Are my people so complex that they cannot afford to share their interests with their children?"<sup>100</sup>

By evaluating her family as dysfunctional by Tohono O'Odham standards, Underhill tried to link the childhood of a white upper-class girl with that of a Tohono O'Odham child. Both had the same needs, Underhill assumed, and both had the same concerns, motivations, and anxieties. And, Underhill implied, Tohono O'Odham culture answered the needs of children more humanely than did Euro-American culture.

Underhill's emphasis on the lessons that non-Indians could draw from Native American testimony made her work popular. Further, Underhill's career as a civil servant led her toward public scholarship. The fact of her popular readership, as happened to Ruth Benedict, caused many anthropologists to dismiss her work as overly personal and literary. Thus anthropologists have tended to take her contributions to the ethnography of the Southwest and the Tohono O'Odham less seriously than they do the work of someone like Elsie Clews Parsons, for example.<sup>101</sup> It is ironic, then, that her work has been so embraced by Native American tribal governments as authoritative. One Tohono O'Odham linguist referred to how her works on the Tohono O'Odham had become references when he stated, "Sometimes we don't know the whole story and we can look at her books and see what it should be. We trust Underhill."<sup>102</sup> When the Tohono O'Odham people wrote their own tribal history in 1985, much of their information came from Underhill's research.<sup>103</sup> In 1979, the Tohono O'Odham Tribal Council recognized her and had a celebration in her honor. The photographs of Underhill riding in the parade at Sells, Arizona, show a happy woman about the age of Chona when she told her autobiography.<sup>104</sup> The tribal testimonial for her read, in part, "We, the People of the Crimson Evening, the O'Odham, recognize your efforts and your talents in preserving and capturing the spirit of our people, for this generation and for future generations to come."<sup>105</sup> For her part, Underhill recalled that the Tohono O'Odham "were so good to me. They were always kind and willing and caring and helped me whenever I needed help so that I sort of remembered it as an ideal place to go."<sup>106</sup>

More than any other woman or feminist anthropologist of her generation, Underhill wrote for an Indian audience as well as a scholarly or Euro-American public audience. She did so in part because the form her career took dictated that she produce largely public-funded and public-accessible documents. But she also did so because of her own

literary aspirations. She longed quite openly to be respected not only for her research but also for her eloquence in writing about it. The fact that her intellectual career began as a novelist indicated where her true passions lay. Further, the way in which she wrote her studies indicates the value she placed on making the texts beautiful. *Papago Woman* illustrates this literary bent most clearly, while indicating Underhill's culturally determined standards of literary beauty.

Underhill felt that she had to reorder Chona's story because Chona told Underhill her story in the Tohono O'Odham narrative style—time and chronology, in the European sense, were not the most important considerations to the meaning of the story. Relationships, of places and people to each other, carried much more importance in the traditional Papago story than did chronology.<sup>107</sup> From Underhill's perspective, Chona "did not tell a straight story." Remembering later the process of writing down Chona's stories, Underhill said:

Wiggling her big horny feet in the sand and gazing off at the distant cliffs she might say, "So, when I was in the garden picking squash, this woman came out of the house." "This woman?" I would inquire. "Yes, his second wife." "Oh, you didn't tell me he had a second wife." "Yes, the year after he married me." "Was this your first husband or your second?" "It was the first and I think it was more than a year before he took that woman."<sup>108</sup>

Underhill then attempted to place Chona's memories in a chronological order, an order more in keeping with the world of Ruth Underhill than that of Maria Chona. Underhill remembered, "Chona did not trouble with the sequence of events but I tried to put them into my own scheme of time."<sup>109</sup> Underhill did so by trying to get Chona to key her experiences to external changes—the coming of the railroad, the birth or death of a tribal member, or the moving of the tribe to different lands. Underhill found this difficult because, as she wrote, Chona "had little sense of time."<sup>110</sup>

As a result, Underhill wrote Chona's autobiography not as Chona told it, but as a Euro-American might understand it. This, however, does not negate its importance as a document illuminating the lifeways

of the Tohono O'Odham. While the way of telling her life has been lost, Underhill has attempted to preserve Maria Chona's words, syntax and meaning. Even while she reordered the narrative to appeal to a non-Tohono O'Odham readership, she preserved elements of the story that would be recognizable to Tohono O'Odham readers. Her use of Chona's words—in large sections where the actual stories that Chona told come almost verbatim from her field notes—indicates how seriously Underhill took her role of preserving Chona's life story. Perhaps that is her most important legacy.

## Executive Females and Matriarchs

[María Chona] was inclined to be independent and executive.  
 —Ruth Underhill, *Papago Woman* (1936)<sup>1</sup>

For many of the feminist ethnographers who conducted research before World War II in the American Southwest, gender roles became a chief focus of research. By examining the gender roles of their informants and of Native American women in general, feminist ethnographers sought to investigate the meaning and structure of being female in different cultures. In the process of this research, some feminist ethnographers gravitated to a particular kind of female informant. This, in turn, shaped their research, because these women tended to be what Ruth Underhill later termed “executive women.”

As a rule, those Native American women who served as informants tended to have greater experience with outsiders. In addition, other characteristics, such as age, marital status, or social status, tended to make them less hesitant to serve as informants. It is difficult to be precise about such characteristics when speaking of Native American informants in general terms because different tribes had different ways of apportioning status for particular characteristics. Historical developments could also alter the ways in which these characteristics affected an informant’s status. For example, younger Pueblo women who worked for wages outside the tribe did not enjoy an increase in status because of this, but as a result of their contact with the world



beyond their tribes risked less comparatively when speaking to ethnographers. Older women or women of high status within the tribe might speak to ethnographers because their status placed them above reproach. For example, the women who served as informants to Elsie Clews Parsons tended to be either of great status within the tribe—the wife of a tribal official—or to have formed an identity outside the social confines of the tribe—women working as servants outside the Pueblo. However, feminist ethnographers most usually sought out informants who had “authority” within their tribes—especially when undertaking the reconstruction of life narratives—and as a result tended to choose informants of a certain personality type. These informants tended to be forceful women whom ethnographers could recognize as powerful, and even as “executive.”

These informants thus played what could sometimes be quite atypical roles within their own tribes. However, those roles were familiar to feminist scholars who themselves were engaged in the intellectual pursuit of proving women’s capabilities to play new and different roles in their own society. As a result, feminist ethnographers who wrote extensively about individual women within Southwestern tribes tended to focus on what Ruth Underhill termed the “executive woman” as a common and admirable gender role for Native American women. In order to present ideal feminist heroines, the ethnographers constructed an “executive woman” identity for their informants. The executive woman was one who acted economically on her own behalf (or on behalf of her family), who resisted limitations placed on her because of her gender, and who took on the role of leader in her kinship group or her tribe. The executive woman image that feminist ethnographers constructed when interviewing Southwestern Native American women was also one that assigned a “protofeminist” identity to women informants. This is especially true of Underhill’s treatment of María Chona, Gladys Reichard’s treatment of Maria Antonia, and Parsons’s treatment of Margaret Lewis. Each ethnographer placed emphases on ways in which these women mediated inequity or patriarchy and downplayed instances of accommodation or even “submissive” behaviors in their testimonies. They did this, however, in varying ways, indicating some of the vagaries of their feminisms as well as continuing contradictions within their views of women’s roles and experiences.

The relationships that Parsons, Reichard, and Underhill formed with their informants focused their research on this issue. Each chose a main woman informant with whom they felt a personal sympathy. Elsie Clews Parsons, a congressman's wife, chose as her main informant the wife of the Zuñi governor. According to Parsons, her informant, Margaret Lewis, took an active political role as the governor's wife, something that Parsons, who continued her political activism during her husband's political career, respected and understood. Lewis was also Cherokee rather than Zuñi, and as such, had learned to negotiate cultural difference. This provided Parsons with valuable insights, as Lewis was in essence a Cherokee ethnographer among the Zuñi. As a Cherokee—and as a woman of great status among the Zuñi—Lewis was less bound than most Zuñi women by rules of secrecy that Parsons often encountered in her research. Gladys Reichard's informant, Maria Antonia, whom she represented as Dezba, was the matriarch of a family of Navajo weavers. She acted independently on behalf of her family, something that Reichard, a single woman who maintained close ties to her sister throughout her life, also respected and understood. As the matriarch, Antonia managed her family's large herd of sheep and the family's production of wool and weavings; in addition, she helped defend her family from the onslaught of Anglo-American cultural incursion. In essence, Antonia seemed to Reichard to be a "career woman" who negotiated both economically and ideologically on behalf of her family, and who controlled her own economic well-being. Ruth Underhill's informant, María Chona, was an elderly divorced and widowed basket weaver who used her role as elder within the tribe and as a healer to overcome the patriarchal limits on her rights to speak about the tribe's future. Underhill, divorced and much older than most of the other students in Columbia's doctoral program, respected Chona's ability to fend for herself in Tohono O'Odham as well as Anglo society. All were assertive women whom the anthropologists could see as feminists—or profeminists—in their own right. Parsons, Reichard, and Underhill assigned the term *feminist* to their informants. Parsons did so when tracing Margaret Lewis's de facto governance of the Zuñi tribe in the teens and twenties; Reichard did so when arguing that Maria Antonia defended a Navajo definition of women's culture under attack from Euro-American definitions of

womanhood; Underhill did so when reflecting on Chona's views of women's ceremonial roles in causing rain to fall.<sup>2</sup>

As one of the first ethnographers to focus on gender roles in her research, Parsons represented the intellectual focus of the emerging circle of women social activists and social science academics in New York, which played such an important part in later scholars' work, including Benedict's, Reichard's, and Underhill's. Many of these women saw as their academic mission the definition of gender difference.<sup>3</sup> Further, these scholars also took on the task of finding cross-cultural cases to support their feminist agenda. By arguing that executive women existed among "primitive" populations, Parsons and other feminist ethnographers set out to argue that feminist ideology about women's abilities was natural. This argument played a significant part in feminist ethnographers' critique of patriarchy, as it sought to replace classifications of women as innately domestic with an argument for women's "natural" abilities to take on public roles.

For Parsons, feminism was as important as anthropology. She succinctly defined her feminism in an unpublished manuscript, "The Journal of a Feminist, 1913-1914." She defined feminism as the commitment to establishing women's right to be flexible in the roles that they wish to play. When Parsons's mother asked her for a definition of feminism she replied,

When I would play with little boys in Bryant Park although you said it was rough and unladylike, that was feminism. When I took off my veil or gloves whenever your back was turned or when I stayed in my room for two days rather than put on stays, that was feminism. When I got out of paying calls to go riding or sailing, that was feminism. When I kept to regular hours of work in spite of all your protests that I was selfish, that was feminism. When I had a baby when I wanted one . . . that was feminism.<sup>4</sup>

Parsons went on to connect restrictive gender roles for women to those assigned to men:

The taboo on a man acting like a woman has ever been even

stronger than the taboo on a woman acting like a man. Men who question it are ridiculed as effeminate or damned as perverts. But I know men who are neither "effeminate" nor pervert who feel the woman nature in them and are more or less tried by having to suppress it. Some day there may be a "masculism" movement to allow men to act "like women."<sup>5</sup>

However, Parsons found it difficult to incorporate the kind of feminist critique she had used often in her sociological writings when writing ethnographies. For one thing, other anthropologists harassed her about the inclusion of overtly feminist critiques.<sup>6</sup> In 1916 she told Robert Lowie that he and Alfred Kroeber "make the life of the psychologist not worth living. I see plainly I shall have to keep to the straight and narrow path of kinship nomenclature and folklore collecting."<sup>7</sup> Kroeber had written to her in 1916, saying, "Dear Propagandist: You write provocative books that are distinctly good and very clever unethnological articles on ethnological subjects."<sup>8</sup>

One aspect of Parsons's earlier writing that Kroeber found most "unethnological" was her proclivity for listing outrageous cases of discrimination against women. In *Old Fashioned Woman*, for example, she discussed the treatment of the new professional women, providing several examples of perfectly capable women whose lives were made miserable by those who viewed them only in terms of their gender. One example Parsons cited was that of Dr. Hannah Myers Longshore, the first woman doctor in Philadelphia. A druggist, refusing to fill her prescriptions, told her "to go home 'to look after her house and darn her husband's stockings."<sup>9</sup>

While Parsons's discussions of feminism tended to highlight women's activities beyond their identities as mothers and wives, Gladys Reichard tended to present powerful women as improved mothers and wives by virtue of their strength and standing. Reichard's discussions of feminism wrapped around her discussions of Navajo women's roles in perpetuating Navajo cultural values in the face of an onslaught by Euro-American culture. In one telling section of her study of Dezba, Reichard recorded a discussion between Dezba and one of her friends about the proper behavior of young women. Upon hearing a report that a Navajo girl was going to ride a wild bull at the tribal fair, Dezba

and her elderly companion discussed how this was an unfeminine act. With Alnaba, Dezba's granddaughter, listening in, Dezba said to her friend, "That girl has always acted exactly like a boy." Dezba argued that this cross-gendered activity resulted from the girl's exposure to Euro-American culture. "It may be alright for her, because she goes to school and acts just like a white girl," Dezba argued. "But I would not like to have one of my girls act like that. Such sports are too rough for women."<sup>10</sup> Thus, for Reichard, the Navajo definition of ideal womanhood had to do mostly with resistance to cultural change.

Dezba's daughter Alnaba, listening to her matriarch, knew, as she told herself, "Navajo women were above all things dignified." Still, Alnaba enjoyed some things that Dezba taught her not to. She enjoyed the women's sports, watching the "sober-face[d]" "determined quiet women" racing their horses or taking part in a tug-of-war. Before the tug of war, Alnaba had succeeded in remaining properly silent, but when she saw the groups gathering for the tug-of-war, she couldn't help herself; instead, "She bet aloud on the north side because on it there were three very fat women." Later, after she lost her bet, she thought to herself, "that she would join in sports of this sort when she grew up." Upon hearing of the girl who would ride the bull, Alnaba thought to herself that "this girl had reached the peak in accomplishment and . . . she vowed to emulate her." Even after Dezba and her friend stated that they would never allow such white behavior among their daughters, Alnaba still thought she would like to do it.<sup>11</sup>

The dialogue between Alnaba's silent thoughts and Dezba's spoken words illustrates the ways in which Reichard saw Navajo constructs as shaping female behaviors in positive ways. The struggle between what Dezba saw as proper womanly behavior and what Alnaba wanted to do echoes, to some extent, the dialogue between Elsie Clews Parsons' mother and her rebellious, feminist daughter. Mrs. Clews told Elsie to wear stays and to play "decently" with the girls instead of the boys; Elsie, the feminist, rejected these rules as examples of patriarchal control over her and limits placed on her because of her gender. The exchange between Dezba and Alnaba followed the same contours, but with notable differences. Dezba did not tell Alnaba not to ride the bull, and she did not say women should not ride bulls because they cannot. Instead, she explained how it is not the Navajo woman's way of behaving and

left it to her daughter to choose the right—Navajo—option. As such, Reichard presented Dezba's rules of female propriety not as an example of ways in which gender limited women's possibilities, but instead as examples of how womanhood dignified Navajo women.

In *The Social Life of the Navajo Indians*, Reichard had written, "Economically, socially, religiously and politically women are on a par with men." In the context of that study, she argued that the status of women among the Navajo was equal to that of Navajo men. In *Dezba*, Reichard set out to illustrate this in one Navajo woman's life. However, Reichard also set out to assert that Dezba's abilities matched those of not only Navajo men, but all men in general. Reichard emphasized the way in which Dezba performed as the head of her large family. Introducing Dezba in a chapter titled "Matron," Reichard listed the ways in which Dezba controlled the labor of men in her family as well as the family as a unit. Reichard also compared Dezba, who was successful in her duties as a matriarch, with less successful Navajo women.<sup>12</sup> In doing so, Reichard underscored the executive characteristics that resulted in Dezba's ability to unite and mobilize her family as an economic unit. Further, she illustrated how Dezba's executive style served to spread authority among the women in her kinship group and thus served as an efficient way to organize the family's labor and resources. Dezba coordinated the complicated business of sheepdipping, marshalling her family's labor and putting it to its best use. Reichard wrote, "Dipping was a task which required great patience, strength and skilful management." Sheepdipping was difficult, because when the government set up the dipping sites, many families traveled there with flocks of sheep and goats, and the stock ate all the grass in the area. An inefficient matriarch might have to wait several days for her flock's turn, and the sheep and goats could be weakened from lack of food and water. Dezba, Reichard argued, made a point to be "among the first to set up her camp on the bare plain at the side of a deep arroyo where the dip had been built."<sup>13</sup> Once she was there, she divided the work up among her family group so that some of the members made the camp—gathered firewood, got water, and started cooking stew to feed the family—while others focused on the sheepdip. "Sheepdipping was an activity which demanded coordinated strength," Reichard wrote, and so Dezba called on all those who owed

her their labor to help. "Besides her brother, Lassos-a-Warrior, and her husband, Dezba's three sons-in-law and her [eldest] son, Tuli, were to help her, as would all her female relatives. They formed a closely-knit group."<sup>14</sup> Once the sheep had been dipped, Dezba oversaw her family's dinner, and then remained at her camp while her daughters traveled to other family's camps to visit and exchange information. In the evening, they returned to Dezba and told her all of the latest news. The similarities between the way Reichard presents Dezba's behavior at the sheepdip and standard business practices for the 1920s are striking. The sheepdip is like a busy office with advertising and research and development departments, with Dezba as the chief executive. Through such striking images as these, Reichard presented Dezba, and thus her informant Maria Antonia, as an "executive woman."

Ruth Underhill went farther than either Parsons or Reichard in describing the "executive woman" role when examining the gender role of María Chona in *Papago Woman*. Like Parsons and her Zuñi informant Margaret Lewis, and Reichard and her Navajo informant Maria Antonia, Ruth Underhill and María Chona had several discussions about definitions of femininity and gender roles. Most of these interviews ended with Chona and the Tohono O'Odham women onlookers thinking Underhill strange, because Underhill always told them about the power differentials in Tohono O'Odham male-female interactions that Tohono O'Odham women never perceived as such.<sup>15</sup> Equally confusing for Chona and the Papago women were Underhill's analyses of Papago gender roles as restrictive.

When discussing women's foot races, Underhill asked why the women were not allowed to run against men. Chona replied, "'We were too unlike. It would be like putting quails and hawks together.' 'And you were the quails, I suppose. Didn't you think the hawks had more fun?' I had strayed into the sort of talk heard from my own female friends. It did not register with Chona."<sup>16</sup> On many occasions Underhill pointed out what she saw as gender inequities in Papago practices, values, and traditions, only to have the Papago women point out that Tohono O'Odham women had their *own* practices, values, and traditions, in which Papago men were not allowed to participate.

For Underhill, the Papago women's conviction that they had power was deeply confusing. Underhill found herself trapped again and again

in the ethic snare of assigning her own motivations to people from distinct cultures. To Underhill's credit, she did realize this and presented herself in a somewhat unflattering light by explaining Chona's reactions to her flawed assumptions. "'You see,'" Chona and the women told her,

"we *have* power. Men have to dream to get power from the spirits and they think of everything they can—songs and speeches and marching around, hoping that the spirits will notice them and give them some power. But we *have* power." When I looked a little surprised, the answer was: "Children. Can any warrior make a child, no matter how brave and wonderful he is?" "Warriors *do* take a little part in starting the children." They sniffed. "A very small part. It's nothing compared to the months that a woman goes through to make a child. Don't you see that without us, there would be no men? Why should we envy the men? We *made* the men."<sup>17</sup>

To Underhill, Papago women did not exercise power as matriarchs from the Navajo or the Pueblos would, and therefore, they did not exercise power. However, once she noted the ways in which they valued their own roles as women, she did begin to accept their argument as an essentially feminist argument about the sources of women's power. She later wrote, "That delightful attitude I should have been glad to take home with me."<sup>18</sup>

Underhill's embrace of this argument presents one of the ironies apparent in her—and other feminist ethnographers—writing about gender roles. The Tohono O'Odham women made an essentialist argument about what made a woman a woman—their biological ability to carry and give birth to children. They further argued that a woman's power derived from her biological difference from men, rather than an inherent equality with men. While feminist ethnographers set out to define gender as a social construct shaped by the cultural norms of any given society, they commonly ended up embracing essentialist arguments. This is especially ironic since much of the feminist agenda in the sciences in the first half of the twentieth century focused on breaking down classifications that had used essentialist definitions



of femininity to argue against women's equality. The ironies inherent in a feminist ethnographer's adoption of an essentialist argument indicate some of the ongoing contradictions within early twentieth-century feminist thought.

Underhill's feminism led her to expect Tohono O'Odham women to have shared a universal "women's' experience"; Underhill viewed Tohono O'Odham women as simply unaware of the realities of the limiting nature of Tohono O'Odham gender roles. As Underhill interviewed María Chona and other Tohono O'Odham women about gender issues, she hoped to find analogues to the experience of middle-class Anglo-American women. Chona, however, seemed less convinced of the similarity between Underhill's life experiences and her own.

At one point Chona intimated that she was unsure that Underhill and other "Milgahn" women menstruated like Tohono O'Odham women did. In this case, however, Underhill presented Chona's question not as Chona challenging the similarity of her experience to Underhill's, but instead to illustrate Chona's "primitive" identity, her innocence, cultural purity, and isolation. As Underhill controlled the narrative of *Papago Woman*, few of Chona's rejections of gender similarities slipped through. In fact, Underhill emphasized what she saw as Chona's recognitions of shared gender experience; she related, for instance, how Chona came to call her "sister."

The use of the term "sister" raises questions. Underhill implied that Chona's use of the term indicated Chona's consciousness of a sisterhood or solidarity of experience between the two women. For Underhill, the term "sisterhood" carried with it specific meanings gleaned from the women's movement. It indicated shared perspectives and even more importantly shared struggles. It linked women together as women; it was importantly not "brotherhood" or even fellow feeling. However, linguistic studies of the Piman language indicate that the term "sister" was commonly used to address other females, even those with whom only the faintest acquaintance existed, without the implication of "sisterhood," rather than—as was more common in Underhill's culture—to denote a special relationship between women.

The greatest point of divergence between Chona's understanding of reality and Underhill's came when the two women discussed issues of women's roles, women's place, and women's power. Still,

they found moments of concurrence. One point that they shared was divorce or its equivalent. Ruth Underhill left her husband, as María Chona left her first husband. Perhaps because of this mutual experience, they had both reached a similar conclusion by the time that they met in the 1930s: "Men are work." Underhill later said that in the Papago worldview, as well as her own, "Life was not so serious that you had to make a fuss about your man not being what you wanted. Oh no, men are never what you want. The one thing I said which she thought worth remembering was 'Men are work.'"<sup>19</sup>

Despite their shared appreciation for divorce, Chona and Underhill's ideas about women's roles diverged widely. It seems that they both started with similar ideas about how and what "proper" women should be. Chona stressed her homemaking skills and defined women as good based on both their domestic expertise and their willingness to perform domestic labor. It is significant that on the several occasions that she discussed so-called "wild women," Chona mentioned that the women did little or no domestic work. "There were women who went alone to those dances," Chona recalled, "the wild women, who did not work and who went about painted every day. Corn ears they painted on their breasts, and birds and butterflies, each breast different for the men to see. . . . They make men and women crazy [with lust and jealousy], sometimes so that they run out and die."<sup>20</sup>

Chona defined "good" women by their ability to work; work, she implied, dictated even how women wore their hair: "We women cannot have hair as long as the men; it would get in our way when we work."<sup>21</sup> Underhill concluded that "Chona accepted her status without stress or rancor."<sup>22</sup> It is important to point out that Underhill recorded elsewhere numerous examples of Chona not accepting her status without rancor. For example, Chona spoke somewhat bitterly of her sense of betrayal when her first husband took an additional wife, and mentioned several times that she resented the forced loss of her "divining crystals," which her family had removed from her body because they felt her visions were dangerous to her as a woman. What Underhill may have meant by saying that "Chona accepted her status without stress or rancor," then, is that Chona accepted the societal norms that governed women's lives even while she rebelled as an individual. Underhill, despite her argument that Chona was a representative Tohono O'Odham woman, also

indicated that Chona saw herself as somewhat special—she emphasized that she had crystals where most other women never could have; she compared her own abilities as a healer to those of male healers; she challenged the authority of men who were younger than she—and felt that she should not have been subject to such a strict interpretation of her society's rules. But Underhill could not accept Chona's total acquiescence to what she saw as unfair limitations based on gender. As a result Underhill stressed Chona's "rebellions" against traditional Papago gender roles. "She was," Underhill concluded, "not the ideal Papago female type, for she was inclined to be independent and executive. In her old age, when such qualities were not taboo, she ruled her whole connection with a competent hand."<sup>23</sup>

Underhill focused on how growing older had given Chona new freedom as a woman. As an example of the "new freedom" afforded Chona with old age, Underhill recounted ongoing conflicts between Chona and an elderly—but younger than Chona—relative, Salt-in-the-Coffee: "[he] had once been the family's oldest honored member. The advent of Chona had subdued him. As time went on, I came to see that they carried on a rivalry for precedence. They fenced constantly with gentle, soft-spoken words."<sup>24</sup> By emphasizing the ways in which age had emboldened Chona, leading her to challenge the masculine authority of a clan member, Underhill presented a model of what she deemed to be appropriate female behavior in the face of patriarchal control.

Underhill consciously tried to encourage Chona to complain about gender inequities in her life. From Underhill's perspective, Chona consistently resisted doing so. Underhill equated this perceived resistance with a lack of consciousness. To illustrate, Underhill recalled that she held beliefs about gender when she was younger that were similar to Chona's:

I was a very meek and good little girl. I was told that women were to take a second place in the world and be helpless behind men—be very useful and able, but not to push themselves forward. And I was little when I was told that, and it stuck for a long time. . . . You do get rid of it before you die. It took me a while.<sup>25</sup>

But by the time that Underhill got to Arizona, she had a very different view of how women should behave. Still, while Underhill presented Chona as lacking a feminist consciousness, she also emphasized the ways in which Chona was “naturally” feminist, a “protofeminist.”

Much of Underhill’s presentation of Chona as a “protofeminist” emerges in Underhill’s discussion of Chona’s reactions to limitations placed on her abilities to take part in the ritual life of her tribe because she was a woman. Somewhere between her tenth birthday and the onset of puberty, Chona experienced a crisis when she began making songs and having visions. This was considered rare, although not unheard of, in women. Had Chona been a young boy, her family would have been overjoyed, as when her brother began to have visions and make songs. Her family would have rejoiced at having another medicine man in the family. But because Chona was a girl, and was not supposed to have visions, her family was deeply concerned. Her family called in a medicine man who examined her and proclaimed that Chona could be a “medicine woman.”<sup>26</sup> Chona recalled that her father believed that one seer in the family—Chona’s brother—was enough. Although Chona was immensely proud of her visions and felt that they made her special, for Chona’s family and the medicine men who advised them, Chona’s visions were more of a torment than a gift.

The theme of Chona’s spiritual “openness” as a threat to her mental health and the physical well-being of those around her recurred throughout her life.<sup>27</sup> At ten, Chona had a ritual operation during which the medicine man removed the “crystals” that enabled her to see, although the medicine man warned that they would always grow back. She described the crystals as “long as the joint of my little finger, white and moving a little.”<sup>28</sup> After this operation, Chona’s visions decreased but did not cease completely. Chona remained, as she put it, “one who understands things.”<sup>29</sup>

In *Papago Woman*, Underhill stressed Chona’s rebellion over the loss of her crystals and the struggle for dominance between Chona and a male elder. Thus Underhill could write in what seem like contradictory terms that Chona “accepted her status without stress or rancor,” even while illustrating that stress. Arguing that Chona accepted her status, Underhill meant that she did not place her frustrations into terms associated with gender or gender consciousness; she would not

have expressed that her thwarted ambitions were the result of sexism in her society or even from patriarchal control over her. However, she did express frustration over being thwarted, and that frustration Underhill viewed as protofeminist. Underhill's presentation of Chona's protofeminism underscored the "naturalness" of the feminist argument, as well as the cross-cultural nature of gender experience and roles.

Underhill implied in *Papago Woman* that women shared a gender experience across cultures, even while she looked at gender roles as determined by individual cultural differences. María Chona defined womanhood in Tohono O'Odham terms, and questioned the womanhood of women from other cultures who did not fit the Tohono O'Odham definition of womanhood. Underhill presented herself in the text of *Papago Woman* as more conscious of cultural difference in defining gender roles than was María Chona. Yet Underhill clung to the belief that women from differing cultures shared the same experiences—because they experienced patriarchy in roughly the same way—despite cultural differences. Thus, the text of *Papago Woman* presents an irony between gender roles and gender experience. With regard to gender roles Underhill is the better cultural relativist and Chona the essentialist, while with regard to gender experience Chona is the better cultural relativist and Underhill the essentialist.

To no small extent, Reichard's, and for that matter Ruth Underhill's, focus on an "executive woman" held true to a more general feminist voice in American culture in the period. For feminists, the presentation of an executive image was an important way to resist cultural pressures to surrender their jobs to men during the economic crisis of the Great Depression. As Linda Gordon has illustrated in her study of Boston poor relief, social agencies during the Great Depression tended to argue that men had been demasculinized by their loss of jobs or economic opportunities. These social agencies downplayed the seriousness of husbands' violence and marital conflict and encouraged families to remain together. Men who had lost their jobs became the subjects of social workers' sympathy, and most family aid focused on finding ways to keep families together and to bolster men's positions within them, thus remasculinizing them. These efforts in essence blamed women for marital violence, which social workers attributed to

the demasculinization of husbands. Thus, "women were consistently held responsible for . . . the general mood of the family, as men were not."<sup>30</sup> Social agencies assumed the naturalness of men's control over women, and saw patriarchy as the only form of stable family structure. According to these arguments, married women should sacrifice their positions in favor of unemployed men, and single women should do the same and seek men's protection by aligning themselves with men.

Such pressures came to fall most heavily on married women workers. Lois Scharf has argued that while married women's employment rates went up dramatically during the Depression, husbands, society, and even the government expressed disapproval for and tried to discourage such employment.<sup>31</sup> Julia Blackwelder, in her study of women's Depression-era experience in San Antonio, Texas, argues, "The Depression elicited unprecedented public pressure to drive women, particularly married women, from the workforce."<sup>32</sup> Some of the pressures on married women to leave the workforce came from employers themselves, acting on what Winifred D. Wandersee terms the belief that they were "undermining family life by hiring wives and mothers."<sup>33</sup> In addition, labor unions also opposed the equal hiring of married women. The American Federation of Labor drafted a resolution in 1931 that endorsed discrimination in hiring against women with employed husbands.<sup>34</sup> Even the federal government acted on fears that the employment of married women prevented access to employment for men by passing a national law that discriminated against married women. Section 213 of the Economy Act of 1932 prohibited wives from employment in the civil service if their husbands were also employed by the federal government.<sup>35</sup> State and local governments as well practiced discrimination of this sort; Lois Scharf has documented that the majority of those released from service by state and local governments were married women.<sup>36</sup> In one example discussed by Julia Blackwelder, Bexar County, Texas, terminated all married female employees of the county courts in 1931, and all married female teachers whose husbands made over two thousand dollars per year in June of 1932.<sup>37</sup>

Susan Ware has documented public opinion regarding married women's employment during the Depression. In one poll taken by *Fortune* in 1936, respondents were asked, "Do you believe that married

women should have a full time job outside the home?" The responses, Ware points out, indicate, "Working women, especially if they were married, faced strong public hostility to their very participation in the work force."<sup>38</sup> Statistically, 15 percent approved, 48 percent disapproved, and 37 percent gave it conditional approval (if, say, the husband held no job).<sup>39</sup> According to Ware, "The three most frequently cited reasons for opposing married women's work were that it took jobs otherwise filled by men, that the woman's place was in the home, and that children were healthier and home life happier if women did not work."<sup>40</sup> A 1936 Gallup poll showed public disapproval of married women's employment even more distinctly. Asked if wives should work if their husbands had jobs, 82 percent of respondents said no.<sup>41</sup>

Feminists responded by pointing out that the idea that married women took away men's jobs was mistaken, because women worked in jobs that were designated as women's positions. Ruth Shallcross, in a 1940 study entitled *Should Married Women Work?*, argued, "Few of the people who oppose married women's employment seem to realize that a coal miner or steel worker cannot very well fill the jobs of nursemaids, cleaning women, or the factory and clerical occupations now filled by women."<sup>42</sup> Those who remained critical of patriarchal control argued for women's empowerment on its own terms, rather than always as part of a zero-sum game in which women gained power at the expense of men.

The tension between these two views of women and power come out in many forms throughout the 1930s, but they especially bubbled to the surface in films about executive women. In these films executive women characters—Mae West in *I'm No Angel* (1933), Tallulah Bankhead in *Faithless* (1932), Dorothy MacKail in *Safe in Hell* (1931), for example—played out the tensions between presenting women as strong and independent and arguing for their containment within traditional boundaries of patriarchy.<sup>43</sup> In the executive woman genre, women who chose to place career ahead of domesticity appeared in a variety of ways. In comedic films, such as Mae West's, the executive woman tended to triumph, and such films played her victory for laughs. In dramatic films, however, it was much more common that the executive woman usually lost the love of a "good man" and either became enslaved by a "bad man" or died. Thus, the fates of executive

women in Depression-era films alternated between doom and victory. Against this popular culture backdrop, feminist anthropologists examining the lives of women who were by the anthropologists' standards "executive women" provided case studies with which to examine several questions about gender roles.

By focusing on executive women's gender roles, feminist ethnographers examined the social construction of gender roles by asking whether "executive women" were made or born. They questioned whether their executiveness resulted from some character flaw in them—a lack of "mother love," or inappropriate ambition—or from cultural training. Ultimately, they sought to remake the image of the executive woman, to legitimize it by linking executive behaviors to the maintenance of the family and, finally, to motherhood and female identity.

By presenting Dezba as an executive woman whose acuity helped her family to remain cohesive even in the face of hardship, Reichard provided a significant counterargument to advocates of patriarchal control as the glue that held together families. Dezba's strengths as an executive made her a *better* mother, as she helped her family retain its identity as a Navajo family. In contrast to the dominant attitudes documented by women's historians, Dezba's executiveness created a *more* cohesive family in which men could be manly without having to establish their manhood at the expense of women. Similarly, when Underhill wrote about María Chona as an executive woman, she emphasized the ways in which her use of her elder status to resist patriarchal control provided a model for non-Indian women. In her definition of Chona as an executive woman, Underhill tended to use the term "executive" as a stand-in for labeling Chona a "feminist."

By focusing on an "executive woman" role in Native American cultures, Parsons, Reichard, and Underhill presented role models for feminists struggling against patriarchy in their own culture. Despite the fact that each "executive woman" figure presented by these ethnographers came from widely varying cultures, the overall positive image of these powerful women informants was relatively uniform. Parsons's informant Margaret Lewis lived in a more egalitarian society than Parsons did, Reichard's informant Maria Antonia lived in a matrilineal society, and Underhill's informant María Chona lived in a patriarchy. Still, the ways in which each mediated power in their lives



followed common patterns. First of all, the ethnographers stressed ways in which each woman claimed political power by using other identities—governor’s wife, matriarch, or elder, respectively—to legitimate their public roles. Secondly, the ethnographers indicated that economic self-sufficiency gave women power in each of the societies in question. Finally, each emphasized ways in which executive women’s power cemented rather than shattered familial bonds, stressing ways that executive women’s actions and roles benefited the families to which they belonged.

With each of these arguments, Parsons, Reichard, and Underhill undergirded the often conflicting agendas of feminism in the early twentieth century. To some extent, each quality presented with regard to executive women’s gender roles directly addressed the arguments of those who feared that women’s equality was gained at the expense of men’s identities and standing. By emphasizing how women’s economic independence and relatively high social standing benefited their families and thus their husbands as well, Parsons, Reichard, and Underhill argued against economic and political limitations on women in general. Each, however, drew the family and the women’s roles within it in differing ways, in part because of their own personal experience. Parsons, a wife and mother, emphasized ways in which Margaret Lewis acted as wife and mother and focused on ways in which her executive role made her a better wife to her political husband and a better mother to her children, who would inherit the stronger culture she helped make. Reichard, however, as a single woman with close ties to her own kin, focused more on how Maria Antonia’s executive nature benefited her entire family. Rather than emphasizing only Antonia’s status as mother and wife, Reichard looked at broad kinship networks and presented Antonia as marshalling the forces of a much broader community of relatives. Underhill, an elder divorced woman without children, focused on how María Chona gained power during the later periods of her life, when she had decided not to remarry again and after her children were grown and had left her. Examining the gender roles their informants played in their societies, feminist ethnographers created heroines who seemed to have figured out the answers to the struggles over gender roles that continued in Euro-American society. By writing about these women, feminist ethnographers answered

a twin agenda. First of all, they presented executive women as arguments that “pure” “primitive” cultures often treated women with a greater degree of respect than did Euro-American society. By presenting executive women in such cultures, they could also assert that such roles were not simply the products of modern society, but instead a more natural part of human cultures. Further, by emphasizing ways in which such roles increased the strength of families, they could argue against common Euro-American assertions critical of greater equality for women that such roles destroyed the family, challenged men’s manliness, and ultimately shattered social cohesion. While they each presented the executive women in a slightly different light, overall feminist ethnographers argued that Native American women’s executive roles provided additional social glue. They strengthened family cohesion, improved family standing, and provided an important tool for resisting destructive outside social forces like forced acculturation and culture loss.

The executive woman gender role feminist ethnographers constructed in Southwestern Native American cultures was familiar to them. They connected these roles to a broader feminist struggle for women’s economic and political independence even while they emphasized the ways in which such roles served to link women to their own cultures and families in positive ways. Buried within their discussions of executive roles and of gender roles lay other vital questions as well, including the question of gender identity. Feminist ethnographers might have asked, but did not ask, if executive females, for example, were identified in their own cultures as masculinized, as were many executive females in Euro-American culture. However, rather than examining the question of gender identity in detail with regard to the executive female, feminist ethnographers found another Native American topic that addressed these questions in an extremely challenging way. Thus, in examining gender identities, feminist ethnographers focused not on their women informants or on executive women, but instead on cross-gendered persons, or man-women.

## 6

### “Is She Not a Man?”

Elsie Clews Parsons (to Zuñi informant Margaret Lewis in 1915):  
“In referring to a *klath’mana* (man-woman) is the pronoun ‘she’  
always used?”

Margaret Lewis: “They do not use the word ‘she’ in their lan-  
guage. The word explains itself. “*Kwash lu otsi deáme?* (Is she not  
a man?)”<sup>1</sup>

Research into gender identity came to fascinate a large number of feminist women anthropologists who worked in the American Southwest. In part, feminist anthropologists studied gender identity because to do so gave them the opportunity to critique patriarchy, the core of their feminist anthropological agenda. With these critiques, they sought to make arguments about the unity of women across cultures. The study of gender also legitimated women scholars’ presence in the anthropological profession. It emphasized women’s fitness for a variety of tasks in their own Euro-American society, including anthropological research. It also illustrated that female anthropologists, as women, had special access to women informants in other cultures.

Feminist anthropologists’ research regarding gender identity focused on such “traditional” aspects of women’s lives as marriage, childrearing, and women’s power relative to men’s. However, feminist

anthropologists' assumptions about gender come out most clearly in their writing about man-women and cross-gendered individuals. By focusing on man-woman gender identity, women anthropologists depicted Native American cultures as more open than Euro-American culture to gender (and sexual) variance. In doing so, they expressed their beliefs that "primitive" cultures owed much of their stability to their ability to accommodate a broad variety of gender identities. With this argument, feminist ethnographers put forward a feminist agenda for their own culture: that the Euro-American cultural inability to deal with women's ambition and abilities prevented not only the personal achievement of women, but the harmony of the entire culture.

Native American traditions of gender crossing, they argued, provided positive examples of ways to channel women's ambitions and abilities. The cross-gender traditions—called "man-women," "two-spirits," or, offensively, "berdache"—on which Parsons, Benedict, Reichard, and Underhill focused in their work illustrated the construction of both masculinity and femininity in Native American cultures.<sup>2</sup> Leslie Marmon Silko has documented this tradition of comfort with gender fluidity in her essay, "Yellow Woman and a Beauty of the Spirit." Before the arrival of Christians, Silko argues, "a man could dress as a woman and work with the women, or even marry a man without any fanfare. Likewise, a woman was free to dress like a man, to hunt and go to war with the men, and to marry a woman. In the old Pueblo worldview, we are all a mixture of male and female, and the sexual identity is changing constantly."<sup>3</sup> A man-woman is a morphological, or biologically masculine, male who occupies a role that European and Euro-American witnesses do not traditionally associate with male gender.<sup>4</sup> Man-women lived as women, and informants reflected their status as women within the tribe; as a result, informants present the man-woman role as a gender identity. However, because it was often so difficult for anthropologists to accept the legitimacy of man-woman femininity, ethnographic texts define "man-woman" as a sexual identity, in direct contradiction to both informants' perspectives and anthropologists' own field notes.

More recent studies of the man-woman have perpetuated this focus on the man-woman as a sexual identity. They focus on man-woman same-sex sexual relations, without any concrete evidence disproving

the possibility of heterosexual relations between man-women and women and despite evidence that at least raises the question of its possibility. Ramón Gutiérrez, who focuses on power relations related to sexuality, presents the man-woman role alternately as a gender identity (when discussing interaction with the Spanish) and as a sexual identity (when discussing the man-woman within the tribe) and so tends to defy classification.<sup>5</sup> Many scholars of gay history or the history of sexuality, such as Will Roscoe and Walter Williams, tend to group man-woman gender experiences under a “gay” umbrella.<sup>6</sup> Some scholars, including the anthropologist Ralph Linton and feminist historian Harriet Whitehead, have challenged the definition of man-woman as a sexual identity and have argued against man-women’s homosexuality.<sup>7</sup> Whitehead and Walter Williams, especially, emphasize man-woman identity as a “third gender” within Native American societies, with “male” and “female” as the first and second genders. However, this “third gender” identity makes sense only if Native American cultures do indeed have three distinct genders; Whitehead’s assessment requires the acceptance of a European conceptualization of opposed male and female genders. Women anthropologists’ informants, however, seem to indicate that they saw gender as less strictly divided along these opposed lines; in fact, their conceptions of gender seem to reflect a spectrum or continuum more than an opposed pairing. In recognition of this continuity, recent scholars, such as Sabine Lang and Kim Anderson, have argued for the presence of four genders in Native American cultures. As Anderson describes it, “The fluidity of gender was inherent in Native cultural views of the world. Some Native cultures understood that there were four genders rather than two: man; woman; the two spirit womanly males; the two spirit manly females. A wide variety of Native American languages have words to describe people that are a combination of the masculine and the feminine.”<sup>8</sup>

The difficulty of defining the gender identity of the man-woman has coaxed some scholars to focus more intensively on their sexual identities as more straightforwardly categorical. This in turn has led to the adoption of the man-woman role as a “pre-history” of contemporary gay identity. In part, this emphasis on man-woman sexual identity at the expense of man-woman gender identity results from reading

published ethnographic texts alone, without comparing them to field notes, ethnographers' unpublished papers, and oral traditions. In the course of their interviews in the field, feminist ethnographers formulated a man-woman identity that illuminated their constructions of femininity in Native American narratives. While anthropologists' published texts tend to emphasize sexual identity—because this is the way that ethnographers conceptualized the identity—informants' statements in those same ethnographers' field notes and unpublished writings directly contradict that conceptualization. Instead, informants emphasized gender identity rather than sexual identity for the man-woman.

Ethnographic writing about the man-woman illustrates the lengths to which feminist ethnographers went to restructure Native American lifeways in order to make sense of their own lives. Questions of the meaning of gender identities arose often in feminist ethnographers' lives and work. Often, some of their most evocative writing about the meaning of gender identity appeared in nonethnographic writings, letters, or even interviews. For many feminist ethnographers, asking questions about gender identity grew naturally out of their own experiences as professional women and as feminists. Their ethnographic interrogation of gender identities resulted in part from their own discomfort within the strict gender identities within their own society. For example, Elsie Clews Parsons wrote of her desire to be flexible in the roles that she wished to play, so that "there will be no common measure. This morning perhaps I feel like a male; let me act like one. This afternoon I may feel like a female; let me act like one. At mid-day or at midnight I may feel sexless; let me therefore act sexlessly."<sup>9</sup> Ruth Underhill, as well, recorded her anxieties about her own gender identity in her own culture, recording that she tended to feel out of place among other women.<sup>10</sup>

The more general questions of gender that women anthropologists sought to examine led them to focus a good portion of their research on man-women and their identities within Southwestern Native American cultures. Parsons and the other women anthropologists examined here argued that Indian cultures allowed a broader range of gender identities than their own culture did. As one example of that, they examined the identity of the man-woman. However,

they concluded that the man-woman identity was the reflection of a sexual identity, rather than the result of cross-gendered behavior. This conflation of gender and sexuality certainly makes it difficult to separate out anthropological writing about each. However, despite the difficulties in teasing out the differences between writing about sexuality and writing about gender, the texts do reveal a wealth of information about gender attitudes within this community of women anthropologists.

First of all, women anthropologists' writing about man-women reveals the extent to which they identified gender as a category of production. They emphasized women's productive roles in gendered labor, such as weaving among the Navajo, pottery among the Pueblos, and basketry among the Papago. By focusing on man-women's labor, they illustrated how man-women worked as women within the contexts of their own tribes. Thus, gender identity, according to these anthropologists, derived from one's role in gendered production. However, this alone was not enough to make women out of man-women; if anthropologists had derived gender from labor alone, then they would have used female pronouns when referring to man-women and identified them in terms of their female gender. Informants, who consistently referred to man-women as "she," had certainly laid the groundwork for anthropologists to use female pronouns in their writings about man-women.<sup>11</sup> In contrast, though, women anthropologists wrote about man-women as men who played female gendered roles, rather than working them; ethnographers consistently used male pronouns when referring to man-women, despite what their informants had said. They did so because the performance of female labor alone was not enough to determine female gender identity.

Anthropologists illustrated that there were other determinants of gender, in their estimation. These included dress and demeanor in addition to productive work. Parsons, Underhill, and Reichard emphasized the dress of the man-woman—her adoption of female dress and hairstyles—as indicative of gender. They argued that man-women "passed" for female in terms of their dress. Benedict, Parsons, and Underhill focused on the man-woman's demeanor—the political and ritual roles they played, for example—in order to determine man-woman gender. They argued that man-women occupied a different political and ritual

niche than either men or women in their tribes. They emphasized ways in which man-women could transcend the usual limitations placed on both men and women to serve as mediators and peacemakers within their tribes. Still, they disagreed as to whether man-women finally held the rights of men or women in their tribes. In the end, however, whether man-women passed for female or not, women anthropologists defined them as male, merely performing a dance of female gender identity.

Thus, performing women's labor and acting like a woman were not enough to determine one's gender as female. Instead, women anthropologists defined female gender by reproductive behavior. Man-women, who engaged in sexual relationships with males, could not, according to anthropologists, play the reproductive role of women. While they might raise other women's children, they could not themselves produce children as a "real" woman could.<sup>12</sup> Therefore, man-women were not women, were not female, and would not be referred to as "she." This determination left women anthropologists to define man-women by their sexuality, as homosexuals or as transvestites, rather than as truly cross-gendered individuals. Parsons, however, went farther in examining this phenomenon when discussing the woman-man. In that examination, she concluded that morphological females who played males were acting as "tomboys" might in Euro-American culture; they were engaging in cross-gender behavior rather than cross-sexual behavior. However, according to Parsons, morphological males who played women were not engaging in cross-gendered but instead cross-sexual behaviors.

The irony of feminist anthropologists defining gender identity as tied to biology is striking. In doing so, they in effect embraced biological-determinist arguments that they, in more general terms, set out to displace with cultural relativism. This internal contradiction indicates some of the various vagaries that existed in their lives and work. It illustrates that cultural relativism did not, at least in this case, fully supplant biological determinism in anthropological thought. Instead, cultural relativism progressed in fits and starts as, in some cases, even culturally relativist researchers clung to the older models of biological determinism. This irony also illustrates the uneven emergence of modernism in feminist anthropology, as researchers combined a modernist



examination of the cultural construction of gender identity with a Victorian emphasis on biological determinism. Finally, this irony indicates some of the core struggles of feminism in the early twentieth century, as feminists wrestled simultaneously with arguments for women's equality with men and women's difference from men.

Because feminist ethnographies classified the man-woman identity as a sexual rather than a gender identity, writing about it allowed feminist anthropologists to address concerns about sexual identity. Coming from a society that classified homosexuality as sexually deviant and rarely tolerated such so-called gender deviations as cross-dressing or transvestitism, women anthropologists may well have been surprised by the openness with which their Native American informants discussed man-women. But in a sense, their training as social scientists had primed them for that discussion.

Most women social scientists of this period concerned themselves with questions of sexuality. According to Rosalind Rosenberg, "Even those not principally interested in the problem of sexual nature often revealed in their work a preoccupation with sexual themes."<sup>13</sup> At the time that Parsons did her fieldwork in the mid-1910s, and certainly by the time that Reichard and Underhill were in the field in the early 1930s, general social science views of sexuality had begun to change. With the studies of Katherine Bement Davis in the 1920s and Dr. Robert Latou Dickinson in the 1930s, social scientists began to recognize the prevalence of same-sex sexuality and even to view homoeroticism as a normal variant of heterosexual behavior.<sup>14</sup> Davis suggested that homoeroticism was much more prevalent than earlier thought, calling into question the designation of homosexual practice and identity as a "freak" occurrence. Dickinson came to view homosexuality, by the later years of his career, as fully "normal." However, while the views of social theorists may have opened up ethnographers to view homoeroticism as part of a spectrum of "normal" sexual practices, the general public in the United States continued to view homosexuality as deviant—neither prevalent nor normal.<sup>15</sup> As both members of the public and social scientists, women anthropologists struggled with this dialogue about sexual practice. By examining sexuality in societies they deemed to be "primitive," and therefore closer to the natural, they hoped to determine what "natural" sexual practices might

include. Man-woman identities, especially when defined as sexual, provided an opportunity to examine this question in some detail.

Parsons especially took a deep interest in sexual practice, even before her fieldwork in the Southwest. Parsons wrote quite extensively on sexuality, and her ideas about what was sexually “deviant” and “normal” were probably ahead of the mainstream. Moral reformers and ministers attacked both her 1906 *The Family* and her 1913 *The Old-Fashioned Woman: Primitive Fancies About Her Sex* as advocating free love, trial and open marriage, and other “deviant” sexual practices, although neither address same-sex sexuality, transvestitism, or gender-crossing directly.<sup>16</sup> Parsons’s daughter Elsie (Lissa) Parsons Kennedy recalled the scandal following Parsons’s publication of *The Family*: “In one of the chapters . . . she did use the term ‘trial marriage.’ That was taken out of context and was preached about in sermons throughout the United States. Around 1907 the term ‘trial marriage’ was horrifying in this country.”<sup>17</sup> Parsons’s letters and papers reveal little direct evidence of her attitude toward same-sex sexuality, but she regularly used exclamation marks in her field notebook when a man-woman was mentioned.<sup>18</sup> Parsons became excited because this was a particular interest, and also a way to directly challenge Matilda Coxe Stevenson’s work with the Zuñi especially, rather than because she was shocked by informants’ mentions of the phenomenon. Parsons regularly denoted information that directly challenged the findings published by Stevenson with exclamation marks, sometimes even noting informant laughter at Stevenson’s assertions.<sup>19</sup> Parsons asked one Hopi informant, Lucy, to directly respond to Stevenson’s findings, challenging them where possible.<sup>20</sup> Parsons’s exclamation marks alongside the mentions of man-women indicate as well that Parsons found the topic exciting and valuable. Parsons also used exclamation marks at other points of special interest, when mentions of venereal disease, conjugal continence, and sexual crimes arose in informant testimony.<sup>21</sup>

Ruth Benedict, for her part, included discussions of man-women in many of her works. In *Patterns of Culture*, for example, she set out a discussion of cross-gendered persons as a plea for greater understanding of difference in Euro-American society. Referring to cross-gendered persons as homosexual, she argued that man-women in Native American

cultures had proven their fitness and necessity because there were not the same sorts of restrictions placed upon them as on gays in Euro-American culture. She wrote, "We have only to turn to other cultures . . . to realize that homosexuals have by no means been uniformly inadequate to the social situation." In fact, Benedict argued, "In some societies they have even been especially acclaimed."<sup>22</sup> While she gave little detailed discussion of man-woman identities in *Patterns of Culture*, she did use the brief mention of them to argue for tolerance for sexual variance. Benedict summarized the Zuñi man-woman We'Wha's career. She stated that "There are obviously several reasons why a person becomes a berdache in Zuñi." She went on to argue, "whatever the reason, men who have chosen openly to assume women's dress have the same chance as any other persons to establish themselves as functioning members of the society. Their response is socially recognized." In contrast to restrictions placed on cross-gendered persons in her own culture, she concluded, "If [man-women] had native ability, they can give it scope; if they are weak creatures, they fail in terms of their weakness of character, not in terms of their inversion."<sup>23</sup> Criticizing homophobia in her own culture, Benedict wrote, "When the homosexual response is regarded as a perversion, however, the invert is immediately exposed to all the conflicts to which aberrants are always exposed." Homophobes in Benedict's culture labeled cross-gendered, transvestite, or gay men as perverts; and once all avenues for success had been barred to them because of this "perversion," they were blamed for their own difficulties. Benedict countered,

His guilt, his sense of inadequacy, his failures, are consequences of the disrepute which social tradition visits upon him, and few people can achieve a satisfactory life unsupported by the standards of their society. The adjustments that society demands of them would strain any man's vitality.<sup>24</sup>

Benedict's plea for understanding fit her broader agenda in *Patterns of Culture* of arguing for racial harmony and tolerance of difference on a broader level.

At the same time, Benedict's arguments about man-women contrasted sharply with what informants told Parsons. In contrast to Zuñi

informants' use of "she" to refer to man-women, Benedict concluded that among the Zuni, "It was thought slightly ridiculous to address as 'she' a person who was known to be a man." Benedict emphasized, as did informants, that "men who took over women's occupations excelled by reason of their strength and initiative and were therefore leaders in women's techniques and in the accumulation of those forms of property made by women."<sup>25</sup> Thus, although Benedict pointed out that they were "regarded with a certain embarrassment," they provided valuable labors and were therefore tolerated. This is certainly different from what informants told Parsons at Zuñi, or even what Chona told Underhill in Arizona, where informants emphasized why these people would be not only tolerated but valued. Further, in her treatment of We'Wha's identity as a sexual one, Benedict failed to document her serious intercultural negotiation—especially when We'Wha accompanied Matilda Coxe Stevenson to Washington, D.C.—and her identity among the Zuñi as, in Will Roscoe's phrase, an "authoritative innovater." Indeed, as Eliza McFeely has indicated, We'Wha was "a respected person among the Zunis who combined imagination and insight to imagine a Zuni that survived its contact with the United States."<sup>26</sup>

Instead, as Reichard would, Benedict focused on man-woman impotence in order to present the identity as a sexually dysfunctional one rather than as a site of cultural resistance. As Benedict wrote, "Sometimes they were men with no inversion, persons of weak sexual endowment who chose this rôle to avoid the jeers of the women."<sup>27</sup> Still, Benedict argued, although man-women were dysfunctional sexually, the existence of the identity created a harmonious way for the Apollonian culture to address this dysfunction. Benedict argued that rather than fight to change the expectations that might have been made of them as men and thus buck the conformity that created Apollonian order and moderation, the man-woman identity became a safe haven that allowed these males to conform to a different standard. Benedict stated, "Some of them take this refuge to protect themselves against their inability to take part in men's activities." Referring to man-women at Zuñi, she stated, "One is almost a simpleton, and one, hardly more than a little boy, has delicate features like a girl's."<sup>28</sup> Her arguments about man-woman conformity take on a different light in the context of the debate over Benedict's own sexual orientation.

Considerable attention has been paid to her relationship with Margaret Mead in this light, especially after Mead's daughter, Mary Catherine Bateson, has argued that the relationship was a romantic attachment.<sup>29</sup> The correspondence between the two women, while intimate and personal, seems to fit well the style of women's affection addressed by Carroll Smith-Rosenberg.<sup>30</sup> With Mead, Benedict felt free to discuss her passions—poetry and ethnography, among other topics—and to express her strong affections for Mead. Mead offered Benedict an understanding she did not find with her husband, Stanley. For that matter, Benedict's correspondence with fellow anthropologist Edward Sapir, though it tapered off sharply after Benedict's divorce, shared much of the same style as her correspondence with Mead. Sapir, who valued Benedict's poetry as an aspiring poet himself, encouraged her to send him her verses. The two exchanged letters of encouragement that might just as easily be taken for a romantic attachment as the letters between Mead and Benedict. Benedict wrote to Sapir about feeling like a mother to his children when he left them with her during a visit to New York, and expressed her deep affection for him. While there is no direct indication of why their correspondence should end with her divorce—no falling out or disagreement—it makes sense to assume that the free affection she could express as a married woman to Sapir, a married man, was very different when she expressed those same affections as a newly single woman.<sup>31</sup>

It seems clear that Mead and Benedict enjoyed a romantic friendship. Benedict expressed sorrow and jealousy, as well, when Mead's marriage to Reo Fortune came between them and Mead's letters tapered off. The question of a physical relationship between the two is more problematic. It is possible that they were lovers; but they do not say so in their letters to each other, or in their journals. Nonetheless, without regard to the existence of a physical relationship, Benedict loved Mead, and saw her as a life partner. The fact that Mead was a woman must have at least raised questions in her mind as social ideas about women's friendships altered around them. The advent of sexuality and Freudian scholarship brought women's relationships with each other into a different focus. Where before World War I it had been commonly accepted for women to kiss or sleep together without being seen as stating some certain sexual orientation, by the early

1920s social scientists and the culture at large had begun to look at women with an eye to identifying these behaviors as Sapphic or lesbian. Further, lesbianism came to the fore as a “mental illness” which, like “hysteria,” especially plagued intellectuals and career women. As a result, women learned to be more circumspect in their passionate expressions of love for each other.

In the mid-1930s, Benedict carried on an extended correspondence with the neo-Freudian Karen Horney that addressed sexuality in some detail. In the winter of 1935, Benedict enrolled in a course with Horney. Through this interaction, Benedict developed the belief that sexuality—sexual practices and desires—were the battleground between personal expression and societal pressures for conformity. As such, sexuality played a central role in revealing any one culture’s definitions of normality and abnormality. These patterns of normality or abnormality addressed not only sexual practice, but the more general definition of what a society accepted as an appropriate amount of conformity and an insufficient amount. Thus, this contest over sexuality formed the key conduit between the individual and the society, the conduit through which values passed to create the personality, which was the society writ small. Horney’s influence also shaped the language Benedict would use when discussing sexuality. In later writings, she would avoid the use of terms like “inversion” to refer to homosexuality. In 1938, she read a paper before the New York Academy of Medicine that included a discussion of a broad range of human sexual behaviors; the paper ended with a call to end labeling such behaviors deviant or inverted.<sup>32</sup>

Reichard was less forthcoming about her attitudes about same-sex sexuality. However, her studies of the man-woman identity in the Navajo culture emphasized her surprise that those who adopted the identity did not suffer social stigma. In her fieldwork before 1925, she indicated that she had never seen one herself, but that she had been told a good deal about them by informants and knew the identity to be “common.”<sup>33</sup> Reichard presented the man-woman in Navajo culture as interchangeably a hermaphrodite or a transvestite.<sup>34</sup> Further, she argued that man-women were considered frightening in Navajo symbolism, because “such abnormal creatures are associated with death.”<sup>35</sup> Her later research, based mostly on her interaction with Hostiin Klah,

whom Reichard identified as “Tłǎh,” emphasized man-woman sexual dysfunction. She wrote with great respect of Tłǎh, who was a great singer and a holy person. Her definition of man-women focused on their “abnormality.” Tłǎh had been castrated during a battle with the Utes, and had thereafter adopted the role of a man-woman. However, he dressed and acted like a man, although he did weave blankets. Reichard argued that “had there been no rumors or whispers, no white person would have picked him out of a Navaho crowd as abnormal.” Reichard’s association of the adoption of a man-woman identity with castration or sexual dysfunction sets her apart from most ethnographical studies of the man-woman. Anthropologists of the era held the more general consensus about man-women’s sexual function that man-woman identity did not mean that man-women were not morphologically male and completely functional as males biologically. Reichard emphasized that despite Tłǎh’s double “abnormality”—childhood castration and the adoption of man-woman identity as a result—Tłǎh commanded great respect.<sup>36</sup> When Reichard wrote in *Dezba* about a man-woman called Lassos-a-Warrior, based on Tłǎh, she spoke of Tłǎh as a Christ figure who had sacrificed family life for a higher calling.<sup>37</sup>

Significantly, as well, Reichard presented Tłǎh/Lassos-a-Warrior as male. She did not present Lassos-a-Warrior as behaving as Dezba did as a female barrier to the incursion of white culture. Lassos-a-Warrior, in fact, became a vital middle ground between white and Navajo ways, performing the man-woman trick of crossing between two worlds.<sup>38</sup> In *Dezba*, Lassos-a-Warrior negotiated changes of culture, resulting in “authority” as a man-woman, as Reichard termed the chapter about this phenomenon. Lassos-a-Warrior provided a counterbalance to Dezba, who as the matriarch protected the family from the loss of Navajo culture. As Reichard explained it, people in the tribe came to think of Lassos-a-Warrior as impartial for reasons other than man-woman identity. Instead, “Because Dezba’s brother had given up his own property, he was regarded as being fair and reasonable in matters regarding that of others.”<sup>39</sup> As a result, on one occasion, a group came to the hogan and said, “Now we have come to you, our friend, to ask you to help us about this will. Some parts of it agree with Navajo custom, but some are more according to white man’s laws.”<sup>40</sup> A man had died, leaving part of his property to a favorite nephew who alone

among his kin learned to chant like himself, and portioning out the rest to his wife and family. Family members feared that more assimilated family members would take the will to court to get more from it, and that more Navajo members would be angry at being passed over. They approached Lassos-a-Warrior, who made a speech at the man's memorial that appeased everyone. Lassos-a-Warrior said,

Our friend made a will. . . . To you, his wife and his children, he left some stock. I hope this will cause no dissatisfaction on the part of his sisters and brothers and their children, for he did it to satisfy the law of the white man. On the other hand, you, his wife and children, could fight for a larger amount in the court at Fort Defiance, but I hope that you will not dishonor the memory of your husband and father by trying to change his last wishes or by dissension of any kind. To you, his nieces and nephews, he left a goodly sum, and it is to be hoped that you will be satisfied with it, rather than try to get something away from the young man, your nephew, to whom he willed it.<sup>41</sup>

By representing every claim, Lassos-a-Warrior prevented anyone from speaking. The final coup de grace came when Lassos-a-Warrior told them, "For the sake of respect to our friend, I beg of you, be bound by his word, do not let the private affairs of our brother be taken to the white court where they will surely be settled in a way strange to us, and certainly not according to his wishes." Lassos-a-Warrior's status as a man-woman let him negotiate as one who knew more than one world. Also, because he stood outside of "normal" existence, "The listeners had the feeling too that the old man himself had no stake in the decision, and they accepted the will exactly as it stood."<sup>42</sup>

This is a much different role than Dezba played. It is also a role that indicated Reichard's assignment of partially male, rather than female, gender to Lassos-a-Warrior. However, it also indicated the degree to which Reichard set Lassos-a-Warrior's masculinity in brackets as separate from the masculinity of "normal" Navajo men. Reichard associated male gender with willingness to assimilate to white ways. Unlike Dezba's son John, who Reichard argued proved his masculinity by



adapting to government schools, Lassos-a-Warrior did not go so far as to adopt white ways. Somewhere on the scale between Dezba and John, Lassos-a-Warrior allowed room for negotiation to keep cultural change from happening too immediately. As a mediator between assimilation, which Reichard represented as a male behavior, and resistance to assimilation, which Reichard represented as a female behavior, Lassos-a-Warrior played an important role outside both male and female gender identities. Thus, as neither wholly male nor remotely female, Lassos-a-Warrior became in Reichard's ethnography a sexual misfit. By representing Lassos-a-Warrior as lacking both a clear gender identity and a morphological identity, Reichard presented Lassos-a-Warrior as a figure outside Navajo culture. However, Reichard focused as well on the fact of Lassos-a-Warrior's castration, underlining that as identifying Lassos-a-Warrior as a *nadle*. Reichard did not dwell on Lassos-a-Warrior's sexual orientation. Indeed, she precluded discussion of the sexual orientation of man-women by choosing one who foreswore material life, including marriage of any kind, for a religious calling. Reichard nonetheless tied Lassos-a-Warrior's identity as a man-woman to castration and sexual disfigurement. Thus, she made Lassos-a-Warrior's identity into one of sexuality first (in this case, of sexual dysfunction) and of any other characteristics, including gender, second.

Underhill's attitudes about same-sex sexuality are more immediately evident. While a student at Columbia University, Underhill read Havelock Ellis's *Studies in the Psychology of Sex* (1900–1928) as well as Richard von Krafft-Ebing's *Psychopathia Sexualis* (1892). Both of these studies classify same-sex sexuality as deviant or as evidence of "inversion," and she later adopted this language in her own discussion of man-women.<sup>43</sup> Parsons and Benedict, too, for that matter, used the language of Ellis and Krafft-Ebing in their published works about man-women.

Discovering that one of María Chona's relatives was a man-woman provided Underhill with an opportunity to examine not only the question of the universality of gender constructs but also definitions of masculinity and femininity in Tohono O'Odham culture. María Chona's first husband had three brothers, one of whom, Shining Evening, was a man-woman. Shining Evening became a great friend to Chona, and Chona spoke of her with deep affection.

The relationship that developed between Chona and Shining Evening was a close and intimate friendship. This intimacy between Evening and her sisters was, at times, a source of jealousy and teasing from their husbands. The brothers commonly joked by accusing Shining Evening of having fathered their wives' children. They would tease their wives, saying, "How do we know these children running around the house belong to us? We are away in the mountains all the time and in the fields. It is Shining Evening who is with the women." They would tease their children as well, pointing toward Shining Evening and saying, "Run along! Over there is your daddy!" But there also seems to have been some serious anxiety behind the laughter. Chona remembered, "When they got us alone [our husbands] would say, 'Is he really all right?' We said, 'yes, just like a woman. We had forgotten he is a man.'"<sup>44</sup>

Chona spoke of Shining Evening as a sister, stressing Shining Evening's performance of "women's tasks" such as gathering and grinding seeds and caring for the children of her sisters-in-law.<sup>45</sup> Margaret Lewis, Parsons's Zuñi informant, also emphasized man-women's performance of women's work. According to Lewis, two of the Zuñi man-women alive when Parsons did fieldwork in 1915 were "Excellent plasterer[s], [who] made [the] chimney places in Margaret Lewis['s] house."<sup>46</sup> Margaret Lewis and another Zuñi informant, Niña, had told Parsons that plastering was a gender-specific job, restricted to women. "Women plaster," Niña told Parsons. "[A m]an plastering would be ridiculed."<sup>47</sup> Reichard's Navajo informants also emphasized man-women's labor as defining their gender. During fieldwork in 1923 to 1925, her informants told her of four man-women, giving various descriptions. One, "*nadle tcó* (Ugly Berdache)" lived at Tiznasbas. Reichard recorded that informants said, "He has a woman's voice, sits like a woman, tends sheep, does woman's work." Another, "*aloí* (Weaver)," came from Chinlee Wash. "He weaves, behaves like a woman, dances woman's dances, but wears men's clothes and has a man's voice." A third, simply known as *nadle*, "did woman's work, had woman's voice and beard, wore men's clothes, and knew no medicine."<sup>48</sup> Reichard's informants indicated that man-women might display a broad range of behaviors usually associated with defining genders, such as voice, comportment, dress, and even facial hair. However, they consistently

indicated that the performance of women's work was one thing that man-women always did.

Tohono O'Odham, Navajo, and Zuñi informants presented man-women as female in terms of their labors in the tribe. Other researchers in this period made similar arguments about man-women performing tasks that male tribal members could not have. For example, Harry Tschopik, Jr., who worked among the Navajo in 1937 and 1938, argued that among the Navajo man-women made pottery as "an exception to this rule" of women making pottery. "Men, lest they become impotent, did not make pottery."<sup>49</sup> Further, other contemporary researchers argued that the gender of the man-woman resulted from the gendered labor the man-woman provided. W. W. Hill argued that his Navajo informants had told him in 1935 that the Navajo word for "man-woman," "*nadle*," meant simply "weaver." Thus in the case of Hill's informants, the gender identity of the man-woman related directly to the labor that they performed.<sup>50</sup>

In contrast to these constructions of the man-woman's identity in the labor force as a strictly female one, Will Roscoe argues in a recent study that man-women participated in both male and female economic activities.<sup>51</sup> In his 1991 biography of the most famous Zuñi man-woman, We'Wha, who died in 1896, Roscoe argues that man-women participated in both male and female economic activities. Frank Hamilton Cushing's 1881 census lists We'Wha's occupations as "farmer, weaver, potter, [and] housekeeper."<sup>52</sup> Farmer and weaver were generally male occupations; potter and housekeeper were more generally female occupations. The Zuñi especially considered farming corn, as We'Wha did, a male activity. And although weaving was less sex-segregated at Zuñi than at many other pueblos, We'Wha knitted, which was a strictly male pursuit; also, We'Wha, as documented in photographs of her, wove on both the men's large blanket loom and on the women's smaller waist loom.<sup>53</sup> However, it is important to add that We'Wha never knitted until she left the reservation to live with anthropologist Matilda Coxe Stevenson for a period in Washington, D.C.; and that she, conscious that a woman would not be allowed to knit, planned to—and did—give it up upon her return to Zuñi.<sup>54</sup> Further, other—morphologically female—women without husbands or sons to do the "men's work" both farmed corn and used the larger

men's blanket loom without any gender sanctions at Zuñi. Flora Zuni, one of Parsons's Zuñi informants, told her that "women without husbands work like men."<sup>55</sup> Therefore, We'Wha's labor was, rather than simply a mixture of male and female identity, a mixture of a variety of often-fluid gender identities, sometimes affected by marital status as well. We'Wha's labor may also have included a little bit of gender rebellion as well, a sort of "going native" while in Washington, D.C.

Still, Margaret Lewis, María Chona, and Gladys Reichard's unnamed informants presented man-women as filling a niche that might otherwise have been filled by women's labor. Further, they emphasized the many ways in which man-women could be uniquely of service to other women, in ways that morphological females and male-gendered men could not have. Chona recalled, "I found the man-woman very convenient." Chona and her sisters-in-law would go out to gather plants with Shining Evening, and "she could carry more than any of us and dig longer."<sup>56</sup> Chona remembered Shining Evening as "a good worker. She was so strong! She did not get tired grinding corn as I did, so sometimes she did it for me." She helped Chona in ways that a man could not have; Shining Evening often ground Chona's corn, which her husband could not do, because, Chona stated, "It would look too bad for a man to grind corn."<sup>57</sup> Shining Evening was especially helpful to Chona when the family group moved.<sup>58</sup> María Chona remembered: "Shining Evening was good to me on those trips. She was stronger than we women and when I was tired she carried my baby for me. No man could do that; it would not be right. Shining Evening was a great help."<sup>59</sup> Other Native American women shared the view of man-women as useful coworkers; an Isleta informant, Mrs. Chavez, told Parsons in 1927 that man-women "do better woman's work than women [because they are] cleaner, [and] more skillful."<sup>60</sup> For that matter, Parsons argued that cross-gendered morphological females also exhibited extraordinary work skills that made them much sought after for their labor; she described Nancy, a "woman-man" from Zuñi, as "an unusually competent worker."<sup>61</sup>

In addition to vital labor they provided, man-women could also become close companions for other women and for children in the tribe. Margaret Lewis spoke fondly of one Zuñi man-woman who was well known for baking little sweet cakes for the children. She remembered another, Tsaladize, who, when he heard a song he liked at a sing

would proclaim that it "tickled his heart" and tear off his shirt; he did this year after year to the amusement of the children at the sing.<sup>62</sup> Shining Evening was also a companion and friend to Chona, and the two spent much of their time together. Chona spoke with sorrow of Shining Evening acquiring a taste for what she termed "white men's whiskey." Chona remembered, "Shining Evening got very crazy. She would flirt and laugh with the men more than ever, throw gravel at them and slap their faces."

Chona enjoyed Shining Evening's company, and found her humorous. She especially remembered the funny nicknames Shining Evening gave the men in the group. She gave her brother, Thundering Wings, the nickname "Skirt-string." Chona recalled,

Evening was the first of us women to make a skirt that was sewed and had a drawstring. Her brother pulled it out and said, "Hey! What's this!" She giggled and said in her funny high voice, "Skirt-string! Skirt-string!" So everybody called him that. Some men wanted to buy that name, it was so funny.<sup>63</sup>

The reason that the Papago found this name humorous bears some discussion. At first blush, the joke seems connected to the English idiom of being attached by "apron strings" to one's mother or wife.

However, more probably the joke centered around the surprise associated with finding something unexpected, either a skirt string or a penis, beneath a man-woman's skirt. This sort of humor has been recorded regarding man-women in other tribes. W. W. Hill, who did fieldwork regarding Navajo humor from 1934 to 1935, recorded a funny story that centered on this element of surprise. In the story, a man who never shared his tobacco was offered a bride if he did share. He sat and smoked his tobacco with a group of men and his new bride, who was a man-woman. Laden with his gifts of tobacco, the bride's family left the couple alone to consummate their marriage. Upon discovering the man-woman's penis, the groom came running after his bride's family and said, "I have felt something that I did not want to feel. I want my tobacco and pouches."<sup>64</sup>

Beyond the element of humor in the giving of nicknames, Shining Evening's granting of nicknames also indicated her gender identity.

By giving nicknames, Shining Evening engaged in a practice usually reserved for women in the tribe.<sup>65</sup> While men could give nicknames, it was much more usual for women to do so. María Chona recalled the practice of carrying water jars with small twigs between them so that they would not bounce against each other and break. Once during a water-carrying session, she told Underhill, “a girl said to one of my [male] cousins: ‘Give me that male thing you have and I will put it between my water jars instead of a stick.’ So we called that man Between-the-Jars.”<sup>66</sup> According to Peter Blaine, Sr., a Tohono O’Odham tribal official born in 1902, “when a woman gives you a nickname it sticks on you.”<sup>67</sup>

Parsons, Reichard, and Underhill used the specifics of their informants’ discussions of man-women to generalize about the meaning of gender, constructions of masculinity and femininity in cultural contexts, and the man-woman experience in the Southwest. In *Social Organization of the Papago Indians*, published in 1939, Underhill deemphasized the gender identities of man-women in direct contradiction to Chona’s telling of the story. She wrote, “the berdache performed women’s work,” but Underhill rejected Chona’s practice of referring to Shining Evening as “she.” Underhill referred to man-women as male, stating, “He made one of the group of women . . . and was treated as one of themselves.”<sup>68</sup> Parsons addressed this question of pronouns with Margaret Lewis directly. Parsons asked Lewis, “In referring to a *klath’mana* is the pronoun ‘she’ always used? [to which Lewis replied,] ‘They do not use the word ‘she’ in their language. The word explains itself.’” And then Lewis added in Zuñi, “*Kwash lu otsi deáme?* (Is she not a man?)”<sup>69</sup> This answer—is *she* not *male*—understandably confused and confounded Parsons. A similar response had stupefied Matilda Coxe Stevenson many years before, who explained her own confusion by saying, “One is led into this error by the Indians, who, when referring to men dressed as women, say ‘She is a man,’ which is certainly misleading.”<sup>70</sup> For Parsons, who believed in two opposed genders, male and female, the possibility that a man-woman could be simultaneously male *and* female and also neither male *nor* female was not one that Parsons could easily grasp. Therefore, Parsons consistently referred to man-women as “he.”

In addition, Parsons identified what could have been a woman-man

among the Zuñi, named Nancy.<sup>71</sup> Nancy filled the ritual role that We'Wha had, dancing as the *Kóthlana*, the kachina role danced exclusively by the man-woman, although Nancy was morphologically female.<sup>72</sup> Parsons later wrote, "Nancy is called in fact, in a teasing sort of way, 'the girl-boy,' *katsotse*." The term "girl-boy" came from the linguistic combining of the Zuñi words for girl (*ka'tsiki*) and male (*otse*). Parsons defined *katsotse* as "mannish, . . . a girl-man, a tomboy"—choosing words commonly applied to rebellious girls in Parsons's own culture—and consistently referred to Nancy as "she."<sup>73</sup> Parsons said of Nancy, "She had a rather lean, spare, build and her gait was comparatively quick and alert." Elsewhere, Parsons argued that men and women had different styles of walking, that the men had a "longer stride," while the Zuñi woman had a "slow and ponderous" gait.<sup>74</sup> Parsons concluded that Nancy's way of walking was masculine, and that "She was . . . a 'strong-minded woman,' a Zuñi 'new woman,' a large part of her male."<sup>75</sup> In direct contradiction to Lewis's telling of the story, Parsons referred to man-women as male, and woman-men as female.

Reichard presented the man-woman in Navajo culture as interchangeably a hermaphrodite or a transvestite.<sup>76</sup> Her depiction of Lassos-a-Warrior, a man-woman relative of Dezba, indicates Reichard's simultaneous sympathy for cross-gendered people and her judgment of the identity as abnormal.<sup>77</sup> Reichard depicted Lassos-a-Warrior as an otherworldly creature while indicating that his choice of the identity of *nadle* resulted from an accidental childhood castration.<sup>78</sup> Reichard presented Lassos-a-Warrior as a figure of authority as a way of emphasizing Dezba's authority as the matriarch, rather than as a way of arguing for the commonality of the man-woman in Navajo tribal life.<sup>79</sup> Further, Reichard indicated the ways in which Lassos-a-Warrior stepped outside of the confines of male and female gender identity to serve as a peacemaker within his tribe. Lassos-a-Warrior was called upon as a peacemaker to negotiate several conflicts over inheritance that originated in part of a family assimilating to white patrilineal inheritance practices while the other part preserved Navajo matrilineal inheritance. In contrast to Dezba, who was her family's first defense against forced assimilation, Lassos-a-Warrior, in Reichard's estimation, became a conduit for accommodation to assimilation.<sup>80</sup>

While informants defined man-woman gender identity as female,

Parsons, Reichard, Benedict and Underhill defined man-women by their sexual identity, which they likened to a transvestite or homosexual identity in their own culture. For her part, Underhill, a divorcee who struggled to support herself in graduate school, placed man-woman marriage in a context familiar to her own. Underhill wrote that "the berdache might marry, but often, since he was entirely able to work and support himself, he lived alone and was visited by the men." Parsons concluded that unions between men-women and male-gendered morphological males, which Parsons set apart from male-female marriage by isolating it in quotation marks as "'marriage,'" were "an economic arrangement, and with not the slightest hint of physical acts of perversion on the part of either 'husband' or 'wife.'"<sup>81</sup> Her informant Margaret Lewis did tell Parsons that man-women had sex with males, but never used the word "perversion."

Underhill's discussion of man-woman sexuality, despite her interviews with María Chona, stressed man-woman homosexual identity, in keeping with the standard 1930s social science perspective of conflating homosexual activity with homosexual identity.<sup>82</sup> Underhill introduced the man-woman in Tohono O'Odham society by saying that "the male transvestite was a common figure." She argued that the man-woman's "sex life with the men was a community institution [but that nonetheless no] scorn was felt for the berdache."<sup>83</sup> Underhill concluded that the man-woman identity was similar to the homosexual and transvestite identities that her own society viewed as deviations of sexuality and gender, respectively.

Parsons shared Underhill's conclusions about man-women's sexual identities. She referred to man-women as "a fairly accurate description of the hermaphrodite."<sup>84</sup> Benedict referred to the man-woman identity at Zuñi as an "inversion," using Havelock Ellis's language.<sup>85</sup> Reichard conflated man-woman identity with "hermaphroditism," and emphasized that the man-women she had known were sexually nonfunctional.<sup>86</sup> In discussing the original man-woman of Zuñi mythic tradition, Parsons referred to her as "sexually abnormal."<sup>87</sup> It is useful to compare the language Parsons used to refer to women-men ("tomboy," "New Woman") and man-women ("hermaphrodite," "sexually abnormal"); in her construction of femininity, gender crossing on the part of morphological females was a social act of rebellion, while the



same activity on the part of morphological males was a sexual act of perversion, and not a social activity at all.

Erased from this ethnographic discourse, as well, are many of the man-woman's social relationships. In her analysis, Underhill omitted the relationship between man-women and women that María Chona, for one, referred to as "sisterhood." Parsons, Benedict, Reichard, and Underhill all deemphasized man-women's female identity in favor of cross-gendered persons' homosexuality, transvestitism, hermaphroditism, and abnormality.

Interestingly, Underhill omitted any discussion of the possibility of morphologically female women-men among the Tohono O'Odham by saying, "When informants were asked about female transvestites, they burst into laughter at the possibility."<sup>88</sup> What might this laughter have meant? Did it, as Underhill concluded, mean that there existed no women-men among the Tohono O'Odham? Or could it mean something else? Photographs of Underhill from her research trips and Tohono O'Odham reflections on her presence on the reservation reveal another possible cause for informant laughter. In Tohono O'Odham culture, which regarded the outward semiotics of femaleness as having long hair, wearing dresses, and adopting a submissive stance, Ruth Underhill kept her hair very short, wore nothing but slacks and shirts, and adopted what she herself later termed an "authoritarian" stance in her interactions with informants. Tohono O'Odham informants perceived Ruth Underhill as a woman who displayed gender ambiguity in Tohono O'Odham terms; given those perceptions, informant laughter in response to Underhill's question about the existence of female "transvestites" seems much less conclusive proof against the existence of the woman-man identity and more an indication of the vagaries of ethnographic field research.

Elizabeth Faithorn, who did research in the early 1970s on gender definitions among the Kafe of the Eastern Highlands of Papua New Guinea, encountered this confusion as well. In 1986, she argued, "Female ethnographers often find themselves being perceived as having a masculine gender in the field, or even being thought of as a man or some curious kind of neutral category of person."<sup>89</sup> Faithorn experienced this herself in the course of her fieldwork, and found that other women ethnographers also reported similar experiences. While being

mistaken for a man was not a serious problem for her, there were some embarrassing questions from informants that made it more difficult. Faithorn recalled, "It is difficult . . . to have others actually wonder what sex a woman in the field is, and to be asked for proof of sexual identity, as I was."<sup>90</sup> Matilda Coxe Stevenson, while doing research at Zuñi, also encountered this questioning of her gender identity. During a ceremony in which women made offerings to the moon and men to the sun, her male and female Zuñi hosts gave her male offering sticks. They told her, Stevenson recalled, that "Though you are a woman you have a head and a heart like a man, and you work like a man, and you must therefore make offerings such as men make."<sup>91</sup> For her part, Parsons was also, on at least one occasion, the recipient of teasing about her own gender identity. On one of her field trips she chose to wear riding breeches in place of her more common simple dress. One day, she later recalled, "Two of the [Cochiti men returning from the hunt] spied me, and made a joke, "*Hombre?*" one called out. To them . . . a woman in breeches was a novel sight."<sup>92</sup>

Writing about man-women gave feminist anthropologists a chance to test their ideas about gender identity. In some ways, their informants presented them with definitions of gender that were incomprehensible to them. For this reason, they dismissed informants' use of female pronouns to refer to people who—in terms of the ethnographers' definitions—were clearly male. To be fair, they may have dismissed such distinctions as a confusion of translation. Benedict depended almost entirely on translators in the field. Parsons spoke enough Spanish that she could take down testimony in Spanish, but she usually paid interpreters when she took down Indian languages. Ruth Underhill learned to speak Spanish and Tohono O'Odham, but not until after her work with María Chona was done. Gladys Reichard spoke Navajo when she worked with Hostiin Klah, but when she took down more general testimony from informants, before she had met a man-woman herself, she could not speak Navajo and depended on interpreters. Even though translation may not have been a problem when she took down Klah's testimony, Klah was a special case, as a castrated man-woman especially. When Reichard sought more general information about man-women in her earlier work with informants, the language barrier may have played a role. It may be unfair to place so much

importance on a single pronoun. However, then, it should work in the other direction, and no special evidence of man-woman masculinity should be implied by the ethnographers' use of male pronouns. Beyond that, however, man-women showed what feminist ethnographers indicated as characteristics of female gender. They negotiated with outside cultures, as it was Shining Evening who introduced the drawstring skirt and Lassos-a-Warrior who negotiated inheritance to accommodate the incursion of white-style inheritance. In addition, they exhibited what the informants argued were signs of female gender as well. They worked as women, alongside women. In order to understand the construction of feminist ethnography, the most significant question is not, in the end, which gender a man-woman was or if a man-woman was a third or mixed or single gender. The most significant reality is that, to anthropologists, the identity was a sexual rather than a gender identity. Informants, however, classified the identity as a gender identity first of all. Thus, feminist ethnographers called man-women "he" and conflated man-woman identity with transvestite and homosexual identities with which they were familiar from their own culture. They did so even though their informants referred to man-women as she, and did not define them as either transvestites or homosexuals. As a result, these scholars' ethnographic texts created a different man-women identity than would have been familiar to their informants.

## Making It New by Making It Old

For many feminist ethnographers, an examination of cultural “pristineness” was of great interest. In part, they conducted their research in the Southwestern United States because they saw the region as more pristine and primitive than most of the rest of the United States. The region’s distance from major eastern metropolitan areas and the later date of Anglo-American expansion into the region meant that the Native American populations of the area had not, for the most part, been removed from their own lands. In contrast, most eastern tribes had been geographically disrupted, removed to reservations, or pushed to other areas. In the Southwest, however, many tribes had been able to maintain their lands. As a result, ethnographers saw them as more closely connected to the “primitive” origins of the region, and thus more culturally pristine. In the Southwest, then, ethnographers could seek out cultures that had not been “destroyed” by dispersal or massive adaptation to Euro-American culture.

Ethnographers were not—and indeed could not be—ignorant of the presence of Spanish influence in the region. In fact, many of their interviews with Native American informants in the region took place in Spanish. Yet they still sought to examine the “pristine” elements of the Native American cultures they encountered in the region. They weeded out elements they saw as being Spanish and dismissed them as inauthentic elements of these cultures. They also downplayed ways in which Native American informants had adapted to the various forms

of cultural incursion on the part of Euro-Americans, including industrialization and the market economy.

Further, they sought to explain that they were the first ethnographers among the people they encountered in the Southwest. Finding “virgin soil” among Native American informants was significant to legitimizing the scientific nature of their ethnographic research. Thus much of Elsie Clews Parsons’s work among the Zuñi focused on disproving assertions made by Matilda Coxe Stevenson, in order to present the previous researcher as unscientific; Parsons could thus argue that she was the first truly scientific researcher to work among the Zuñi. So Gladys Reichard undergirded her field research on ritual with a scientific analysis of kinship and linguistics. And Ruth Underhill sought to prove to her advisor, Franz Boas, that she was the first to work with the Tohono O’Odham, despite the fact that several researchers—including the Smithsonian ethnomusicologist Frances Densmore—were working among the Tohono O’Odham at the same time she was.<sup>1</sup>

Feminist ethnographers strove to recognize cultural relativism and to determine the ways in which cultures came to differ from each other. To examine this, they focused on ways in which cultures preserved their distinctiveness in the face of contact with other cultures. They also had assumptions about cultural purity as a source of authenticity. Parsons, especially, attempted to indicate the degree to which the Native American cultures she came into contact with had incorporated elements from other cultures. Parsons believed that the cultures with which she came into contact were not “pure.” In 1918, she speculated that “fifty percent of Zuni culture may be borrowed from White culture.”<sup>2</sup> She determined this even if her Zuñi informants were unaware of this or denied it. She wrote in 1916 of an exchange between herself and an informant, when her assessment of an informant’s statement as being “Surely, a Mexican belief” caused her informant to reply, “No, Zuni.”<sup>3</sup> She later argued that many folktales from Acoma, Laguna, and Zuñi were “probably of Spanish provenance,” though she provided no argument or evidence of it.<sup>4</sup>

Feminist ethnographers often linked the ideas of pristineness, authenticity, and “primitiveness” together. By seeking out what elements they identified as primitive in Southwestern Native American

cultures, they sought to identify authentic Native American cultural elements as well as presenting to Euro-American readers a positive view of primitive cultures. In this project, they were linked to a much broader movement among modernists and other ethnographers. In *Gone Primitive*, her fascinating examination of the construction of primitive identity within the emergence of modernist culture, literary scholar Marianna Togorvnick has identified the predominant conceptualization of “the primitive” that emerged in modernist culture in the late nineteenth and early twentieth centuries. She presents the primitive as “exist[ing] for us in a cherished series of dichotomies: by turns gentle, in tune with nature, paradisaical, ideal—or violent, in need of control; what we should emulate or, alternately, what we should fear; noble savages or cannibals.”<sup>5</sup> Togorvnick examines a generation of ethnographers, most prominently Bronislaw Malinowski but also including representatives of the American School like Kroeber and Mead, as well as artists, writers, and other social commentators. She argues that ethnographers constructed the primitive identities of the cultures they studied as ways of addressing largely unspoken fears within their own society. Togorvnick devotes a large portion of her examination of the construction of primitivism in Malinowski’s work to his own anxieties about his physical body—pale, waxy, flaccid, pimples, flabby—as compared to the physicality of his “primitive” Trobriand informants—ruddy, golden, lithe, smooth, and firm.<sup>6</sup> Even while he admired the primitive physical “superiority” of his informants, he also dwelled on their difference, calling them “niggers,” for example, and treating their privacy with a cavalier lack of respect. Thus, for Malinowski, primitive identity was both positive (beautiful) and negative (inferior). For later generations of ethnographers—especially cultural relativists and salvage ethnographers—the enumeration of the latter aspects of primitiveness were less likely to appear in their published texts. Rather, salvage ethnographies tended to focus on positive aspects of primitive identity, emphasizing instead ways in which Euro-American cultures could learn (or learn again) from “primitive” people healthier and happier ways of living.

Ethnographers were not the first to present Native American cultures in general as possessing secrets that had been lost to Euro-Americans. They merely continued a long tradition of writing about

the “noble savage” that reached back to the early Enlightenment. They built as well on the work of social reformers of the late nineteenth and early twentieth centuries who used Native American practices to juxtapose against Euro-American practices they considered unhealthy. One reformer who preceded ethnographers’ presentation of Native American cultures as containing important practices now abandoned by Euro-Americans was George Wharton James. James’s travels among the Native American populations of California led him to write a passionate jeremiad about public health called *What the White Race May Learn from the Indian*, published in 1908. For example, in this study James argued that Euro-American culture was endangered because it had forgotten the “primitive” practice of breathing correctly through the nose. James described ways in which “Indian mothers” inculcated the practice of nasal breathing in their children, sometimes even resorting to sealing their infants’ mouths with straps of leather or fabric to force correct breathing. In contrast, James wrote, “As one walks through the streets of every large city, he sees the dull and vacant eye, the inert face, of the mouth-breather; for, as every physician well knows, the mouth-breather suffers from lack of memory and a general dullness of the intellect.” These ill effects resulted, James argued, from allowing “disease germs” to pass to the lungs via the mouth, unimpeded by the “gluey mucus” of the nasal passages.<sup>7</sup> “Hence,” James exhorted his Euro-American readers, “emulate the Indian. Breathe through your nose; do not use it as an organ of speech. . . . [W]atch your children, and even if you have to bandage them up while they are asleep, as the Indians do, compel them to form early this useful and healthful habit of nasal breathing.”<sup>8</sup> James went on to discuss Native American women’s dress and hygienic habits, encouraging Euro-American women to eschew the corset and other unhealthful practices and return to the “primitive” and more healthful ways of Native Americans.

Even while they would have dismissed many of James’s arguments as unscientific, feminist ethnographers nonetheless built upon this same tradition of presenting Native American “primitivity” as something that would benefit Euro-American society. Feminist ethnographers, however, made their arguments specifically that primitive cultures often treated women with a greater degree of respect than did

so-called civilized cultures. Feminist ethnographers argued that women occupied a higher status within these cultures. This argument was coupled with the broader ethnographic assumption of the period that “primitive” cultures were more “natural”—more closely connected to nature and themselves purer—than “civilized” cultures. For the most part, Parsons, Underhill, and Reichard set out to examine the “primitive” identities of their informants’ cultures in order to present to Euro-American society case studies of societies that dealt more successfully with human individualism than did Euro-American society.

Ruth Benedict is an important exception to this agenda among feminist ethnographers. While she also applauded such “primitive” cultural values as openness to sexual diversity, she wrote specifically of the dangers inherent in seeking out primal human activities by looking to “primitive” cultures. Benedict resisted this agenda as romantic and counterproductive. She wrote in *Patterns of Culture*, “There is no justification for identifying some one contemporary primitive custom with the original type of human behaviour.”<sup>9</sup> Still, she did not emphasize the ways in which her informants had adapted to industrialization and the market economy. Her letters to Margaret Mead from the field indicate that Benedict’s difficulties in adapting to life in the field were eased by the fact that her Native American hosts could provide her with some of the creature comforts of home. While working in Peña Blanca, near Cochiti, in 1925, she wrote Mead: “the chief difficulty was that I was wholly unprepared to find flour unknown in the stores. But the keeper [of one of the local stores] unearthed three little cartons of Aunt Jemima pancake flour, and I am saved.”<sup>10</sup> However, she never mentioned such things in her ethnographic writing, omitting the presence of stores where ethnographers could buy Aunt Jemima products. Still, in her synthetic works, such as *Patterns of Culture*, Benedict rejected the idea that primitive cultures provided sketches of ideal societies or basal states among human cultures.

One way in which feminist ethnographers presented Southwestern Native American women as primitive was to draw connections between these women and their natural environments. In doing this, feminist ethnographers also deemphasized Native American women’s interactions with the market economy. By arguing that Native American women were not a part of the market economy of the Southwest,



ethnographers attempted to underscore their identification as primitive. Feminist ethnographic texts provide records of a dialogue about primitivism between Native American women informants and Anglo women ethnographers. Some of the richest dialogues of this sort appear between Native American women's artistic and craft expressions of the landscape and Anglo women ethnographers' depictions and constructions of those processes in text. Southwestern women's pottery provides perhaps the best concrete example of this synergy. Feminist ethnographers argued that to many Pueblo tribes the clay itself, from which women fashioned pots, was female, named "Clay Old Woman," "Clay Lady," or "Grandmother Clay."<sup>11</sup> The images and decorations that appeared on those pots also carried demarcations of gender. For example, Zuñi vessels made by women traditionally contained an image of water bird feathers also known as the "prayer stick design" on the lip of the pot; while Zuñi tradition precluded women from making the Zuñi prayer sticks that only men could use, women could paint the prayer stick design on the pots.<sup>12</sup> One Zuñi potter who painted these designs on her vessels told ethnographer Ruth Bunzel in 1924, "We like to paint the water birds because they live in the water, and so that the jar will never be empty."<sup>13</sup> For these Zuñi women, the use of natural imagery had little to do with the divisions between male and female power over or in the land, or with the potter's identity as primitive or modern; instead, it provided evidence of women's connections to nature and the interwoven condition of male and female relationships with the land and natural elements. Ethnographer Matilda Coxe Stevenson, however, argued that women's "borrowing" of nature images "restricted" to men indicated a struggle over resources between men and women, which Stevenson held up as an analogy to her own experiences as a woman in Western society. Like Stevenson, Elsie Clews Parsons also linked Native American women's interaction with the landscapes around them to gendered struggles over power. However, Parsons went further to also use these struggles to illustrate the primitivity of the cultures she studied. For Parsons, as for Benedict, Reichard, and Underhill, primitiveness was a positive value, associated with honesty, sustainability, and gender equity. In a sense, Parsons continued a time-honored tradition among European and Euro-American observers of Native Americans in presenting Indians as more closely

linked to nature; however, Parsons added to that tradition a sense of the femininity of nature as a positive attribute of the Native American worldview.

By focusing on Native American women's connections to nature in less patriarchal, more egalitarian cultures, especially, feminist ethnographers could argue that patriarchy had severed the innate and empowering ties between women and the land around them. For example, Elsie Clews Parsons examined Native American women's use of birth control to illustrate how women not dominated by patriarchy gained control over their fertility and thus their own bodies and lives. This in turn served to critique the patriarchal control that had criminalized women seeking birth control in her own culture. She argued that woman's nature was to seek fulfillment beyond the confines of domesticity and a burdensome motherhood, and linked emancipation of women to their ability to choose when to have children. Rejecting the Victorian conceptualization of women as dominated by their reproductive "duty"—in the memorable words of one doctor in the 1840s, "a uterus around which God had built a woman"—Parsons presented woman's nature as not innately constricted within domesticity.

Elsie Clews Parsons collected information on Pueblo women's primitive connection to nature in the course of collecting folkloric stories for her germinal study *Pueblo Indian Religion*.<sup>14</sup> She saw the value of collecting this information chiefly as a way of tracing connections and similarities among and between the folklore traditions of the various Pueblos, as she was interested in the general question of the diversification of the individual Pueblos. The majority of Parson's research into Pueblo women's land use and views of the environment consisted of recording birth taboos and remedies, collected from women informants, which Parsons used to argue that these women had kept up the "primitive" linkages between themselves and nature that "modern" women had lost. In doing so, Parsons obliterated evidence of what she saw as "unnatural" attitudes among Pueblo women—evidence that Pueblo women interacted with the market economy or viewed land and natural products commercially.

One example of Parson's selective presentation of Native American women's land use appears in her discussion of partum, or pregnancy, taboos.<sup>15</sup> Parsons took extensive testimony about Pueblo

women's land and plant use as a way to comment on their uses of "natural" birth control, fertility aids, and abortifacients. While informants told her of a variety of ways in which they interacted with their surroundings, Parsons only published information about what she determined to be "natural" interactions. Thus, she weeded out informants' references to a much more cosmopolitan Southwest than Parsons was ready to reveal. She cut out what she saw as cultural pollution, such as the incursions of industrialization into Native American environments. In this sense, Parsons's selective presentation of informants' stories differed little from those of male anthropologists or nonfeminist anthropologists.

However, Parsons's selective presentation of informant testimony also played a role in pushing forward her feminist agenda on woman's nature and the connections between reproduction on the one hand, and controlling reproduction on the other. For one thing, Parsons presented a sense of the femininity of nature as a positive attribute of the Native American worldview. In so doing, she constructed an identity for Native American peoples as more closely connected to nature and therefore more actively connected to their sexuality. In this way, too, Parsons argued that the more egalitarian gender practices of Pueblo people were also more "natural" than the patriarchy common in her own culture. And further, she used her depiction of Native American women's plant use as more natural to champion causes that she supported in her own culture, including the birth control movement. By connecting Native American women's "primitive" natures to the use of birth control and abortion, Parsons strove to argue that such practices were not vices of modernism but, instead, reclaimed traditions.

Parsons had been an ardent supporter of birth control advocate Margaret Sanger in New York. She had once circulated petitions among New York society women in support of birth control. She approached women chosen because they were mothers of at least two children and held socially prominent positions, and asked them to sign the petition, which would be published. Signers stated that they supported Sanger, that they believed that the information she set forth on birth control should indeed be heard, that they themselves had imparted such information, and that they themselves had practiced birth control. While the vast majority of women she approached felt

they could not sign it—usually because it would somehow damage their husband’s standing—nonetheless the experience was useful as a moment when women discussed birth control openly. Knowing that almost all of the women she had spoken to admitted privately that they used birth control emboldened Parsons to search out what birth control might be like in a “natural” society devoid of the Victorian mores that had prevented her counterparts from signing the birth control petition.<sup>16</sup>

Encountering natural forms of birth control during her fieldwork, she then set out to present her research in such a way that it would support her argument that in an egalitarian primitive culture, where the unnatural pressures of patriarchy and Victorianism did not trample it in the bud, birth control could be seen as a natural part of the reproductive cycle. To make this argument, she emphasized the connections between primitivity and Indian women’s birth control and childbearing practices.

Parsons selectively presented Native American women’s partum, or pregnancy, taboos, emphasizing “nature-lore” over intimate knowledge of the market economy.<sup>17</sup> Parsons collected women’s accounts of natural causes for birth difficulties, associating behaviors of the mother in the natural world with problems of child birth or of the fetus. By emphasizing this animist perspective, she underscored her presentation of Native American women as primitive. One such animist partum taboo warned women not to skin animals while pregnant lest they cut through the hide and create a corresponding mark on the child they carried.<sup>18</sup> Other taboos precluded a husband hunting while his wife was pregnant. In November of 1928, Parsons visited Isleta Pueblo, where an informant, Maria Chiwiwi, told her that her husband, a deer hunter, “went hunting when she was pregnant.” As a result, the “baby was born gasping. [The] husband had to go out & run as if chasing a deer & return & pass his hands over the baby, who began to breathe well.”<sup>19</sup>

Parsons wrote about partum taboos and treatments that focused on Native American activities in close proximity with what she deemed to be nature. She also emphasized ritualistic nature-lore ways of dealing with the consequences of breaking those taboos. In doing so, Parsons presented Native American women as closer to nature and

therefore positively more primitive than their Euro-American counterparts. Parsons later incorporated these stories verbatim into her studies of Pueblo mothers and daughters, providing them as examples of the ways in which Pueblo women used nature lore as a way of explaining biological process.<sup>20</sup>

However, she chose not to include an “unnatural” birth taboo that she had collected at Isleta Pueblo in December of 1927 from another informant, *The Sun Takes a Head*, that “pregnant women should not go to moving picture shows . . . if she goes baby will be twitchy, having no sense, moving quickly like film.”<sup>21</sup> Such an interaction with the market economy challenged Parsons’s description of partum taboos as evidence of primitivism. Moving pictures were not part of primitive culture, and were not natural. Further, an intimate knowledge of the kinds of images one would see when attending a moving picture indicated that *The Sun Takes a Head* had not only interacted with the market economy but had become a steady consumer of industrial culture. Thus *The Sun Takes a Head* indicated an example of the cultural hegemony of Hollywood films that Parsons identified as a pollution or destruction of authentic Indian (and primitive) culture. Coming from the immigrant city of New York, the assimilationist and hegemonic power of early film would not have been lost on Parsons. Progressive reformers, in fact, argued for moral reform of films in the 1910s and 1920s because films were one of the most important ways in which immigrants to America learned how to behave as Americans.<sup>22</sup> An Indian who attended the movies would have been, by the standards of most salvage ethnographers, a very “impure” cultural source. As a result, Parsons omitted this taboo story as not fitting what an authentic Isleta informant would say.

Parsons also omitted the story because it depicted an informant much more sophisticated about and involved in Euro-American culture and the market economy than she would have expected. And it should be noted that she never published another story, which an informant provided as a Taos cure for snakebite among other—herbal—remedies, in which the bite victim was advised to “stand in the river; send for the white doctor!”<sup>23</sup> By depicting Pueblo women as living in an environment that Parsons would have understood as “natural,” Parsons emphasized Pueblo women’s “primitiveness”

and romanticized the much more complex story of Native American women's views toward nature.

Similarly, Parsons collected ethnobotanical information about plants that assisted informants with "women's complaints"—menstrual cramps, childbirth and pregnancy, and post-partum illnesses. In the course of this research, however, several of her informants made it clear that they preferred to buy commercially produced remedies for such complaints at the local store. However, Parsons only published accounts of Native American women's use of "primitive" and natural remedies. One Hopi informant named Lucy told Parsons in June of 1920 of using a plant called "*palatspa*," which Hopi women would "boil, to stop blood flow after baby [and] also at monthly periods."<sup>24</sup> Lucy also told Parsons that she used "*Omapi*, cedar, boiled & drunk hot after birth."<sup>25</sup> In the same month, a Laguna informant told Parsons that "*yuushk'a wawa*, [a] root, collected by woman, stops flow of blood after childbirth." Lucinda Martinez of Taos Pueblo told Parsons in November of 1926 that she used the roots of a yellow-berried plant, "*Kwatapapiengumv*," to prevent conception as a tea at each menstrual period.<sup>26</sup> When Parsons drew on these interviews, she concluded that Indian women used nature to provide them with medicines and forms of birth control; however, she left out the fact that several informants, including Lucinda Martinez, had discussed with Parsons how women used products purchased from the local drugstore to preclude conception or induce abortion.<sup>27</sup>

This omission calls into question Parsons's presentation of Native American women's attitudes toward both plants and the market economy. More importantly, however, it also reveals Parsons's linkage of primitivism and nature lore to Pueblo women's attempts to control fertility. Parsons presented the argument that Pueblo women used plants to preclude conception as a reflection of the primitive belief in animism and the interconnectedness of natural elements in Native American women's lives. This underscored the primitivity of Native American women, as it drew out elements that ethnographers recognized as animism in their thinking about nature. However, it seems clear from women informants' admitted dependence on patent medicines that the key issue for them might not have been natural ritual but simply the avoidance of repeated and unwanted pregnancy.

Further, informants did not argue that they preferred natural remedies to those that they could buy in the store, nor did they refer to using store-bought remedies as anything out of the ordinary in their everyday lives.

In fact, informants revealed to Parsons that while they sometimes used traditional “natural” items for contraceptives and fertility aids, abortifacients usually came from the store. With the exception of the mention of *papiengumv* used exclusively as an abortifacient and never as a contraceptive, all other references to abortifacients in her notes mention that the medicines used came from the town store.<sup>28</sup> Had Parsons included this aspect of informant testimony she would have undercut her argument that abortion was not something facilitated with contact with non-Indians, something that Parsons presented as cultural pollution and therefore negative. Parsons also made notes for herself in her field notebook, which emphasized that Pueblo women did not view abortion as infanticide. In the course of identifying kinship connections between different families in the village, Lucinda Martinez of Taos Pueblo told her about a case of infanticide. The mother, after conceiving a child by her brother-in-law, had “tied a shoestring around baby’s neck & put it under [the] doorstep.” However, because “they found her bed bloody,” tribal authorities found the dead child and punished her. Parsons noted that this was the “first case known of infanticide,” and “no abortion case.”<sup>29</sup>

In essence, Parsons made a simple equation out of this. She argued that Native American women, as primitive, were closer to nature, and that they used natural birth control and abortion. From this she concluded that birth control and abortion were part of primitive culture and also therefore natural. She argued that abortion was not, among these “more natural” cultures, a form of infanticide. She removed testimony about abortifacients that Pueblo women purchased at stores because she did not want Euro-American readers to conclude that their use was something that Indians had learned from Euro-Americans; in fact, the removal of examples of cultural adulteration were commonly omitted from discussions of Native American cultures by Boasians hoping to salvage the authentic culture by itself. Through this logical progression, Parsons’s discussion of Pueblo women’s land and plant use became an opportunity for her to argue for more open attitudes about

birth control in her own culture. This further allowed her to link the European, "unhealthy" attitude against birth control to patriarchy.

Similarly, Gladys Reichard constructed Navajo primitivity in her ethnographic texts by emphasizing the connections between Navajo women and their natural environment. Reichard, like Parsons before her, focused on Native American conceptualizations of the environment as female. She stressed the ways in which the Navajo saw the land around them as their mother. She presented the Navajo as "making use of most of the facilities furnished by their Mother, the Earth, an animate vibrating personality who has their good at heart, who is lavish in color and atmosphere, and exacting, even cruel at times, about furnishing subsistence."<sup>30</sup> Reichard immediately followed this connection between the Navajo and their mother earth with her introduction of Dezba and her family, so that the analogies between the two relationships would not be lost. In case the reader might miss it, though, she pointed it out; mirroring Dezba surrounded by her offspring, she described a landscape in which "clumps of cedar grow close to pinyons as if they were sheltered children."<sup>31</sup> Throughout her study of the Navajo matron, Reichard returned to the connections between primitivity and Navajo identity with the land. As part of her longer discussion of Dezba's son John and his forays into white culture at a series of government schools, Reichard illustrated how learning a new way of interacting with the land was an ingredient in assimilation, and thus the loss of the positive qualities of primitive identity. Reichard wrote of John after his stay at a government school, "Although he had been taught it was improper to destroy plants without a ceremonial purpose, he reasoned that the plants which grew in a white man's garden were different, hence the Navajo rules did not apply." Thus, he stopped offering them prayers of thanks when he used them, severing his ceremonial tie to the earth just as he had severed his matrilineal tie to his mother by leaving her house. This small rebellion became a larger one, when it led John to want to abandon even the corn farming he contributed to his family's survival. "As time went on," Reichard wrote, "he learned too the many uses of strange plants, especially the edible ones, and he became ambitious to raise some of his own."<sup>32</sup> John rejected growing corn for growing white people's plants he had learned to grow at the government school. John, in Reichard's text,



became a representative of the dangers of losing one's primitive identity and yielding to Victorianism. In order to escape his positive primitive identity, he adopted a negative "civilized" identity, which led his family to see him as an object of ridicule and even scorn.<sup>33</sup> Further, John's use of "strange" plants and his ambitions regarding them marked his loss of authentic Navajo identity and his stepping away from his "primitive" identity. Reichard consistently presented this as a negative step, describing the "civilized" John as alternately "wild-eyed" and depressed; in contrast, Reichard commonly depicts other, unassimilated Navajo as calm, peaceful, and happy. For Reichard, the survival of Dezba's family depended on the maintenance of their authenticity and thus their primitivity. Those who "strayed"—like John—were characters who evoked pity and even scorn.

Feminist ethnographers were not "of a piece," though, in recognizing cultural change in the cultures they addressed. While Parsons carefully articulated cultural "borrowings" so she could excise them as inauthentic, Ruth Underhill, for example, rarely acknowledged their presence. This is especially evident in her "autobiographical" study of María Chona. Throughout the story of Chona's life, Underhill presented her as a representative of the "primitive" and pristine Tohono O'Odham, wearing loincloths, eating native desert foods, and clinging to animist religious beliefs. And yet María Chona presented herself as throwing away store-bought shoes because she had become too fat to wear them after eating store-bought lard and white flour, and she told of having her children baptized by a Catholic priest. Her own name—María Chona—came from her own Catholic baptism name of María Encarnación.

Native American informants did not live in isolation from other cultures. The Southwest, especially, served as a diverse cultural meeting ground.<sup>34</sup> As the stage for successive waves of conquest, the Southwest played host to Spanish conquistadors and priests, from the expeditions of Oñate in the 1590s to the missions of Father Eusebio Kino in the 1680s and 1690s. After the Spanish came Anglo miners and traders, the U.S. Army, and the townbuilders—men and women who met with, fought with, and often adapted to the native peoples they found there. Before the Spanish and before the Anglos, the region saw immense intercultural sharing between diverse tribes, with trade and warfare and

raiding forming a discourse between groups as different as the Apache, the Hopi, the Navajo, and the Pueblo tribes. Feminist ethnographers' informants were clearly conscious of this tradition of interchange, and there is no reason to believe that their knowledge ended when Anglos and ethnographers arrived on the scene.

María Chona, for one, understood how Tohono O'Odham ways had influenced at the very least the Apache. Telling about Tohono O'Odham women stolen by Apache during the autumn raids on Tohono O'Odham lands, Chona said that "those Papago women taught those 'Ops' to make baskets like us."<sup>35</sup> She was also conscious of the incursion of some Catholic religious beliefs into her own traditional Tohono O'Odham religiosity, especially as they affected the naming of her children and the burial of her father. So Chona did pay attention to the meetings and shared traditions of the diverse populations of the Southwest. When Underhill asked her how the Tohono O'Odham had come to live where they did, she did not express bitterness over the Anglo-American territorial encroachments that had taken away at least some Tohono O'Odham land. As they passed through a parcel of former Tohono O'Odham land, Underhill asked her, "'Your people lived there?' . . . Chona nodded. 'Us first. Then Spanish. Then Milgahn.' 'And they pushed you out?' Chona took that without anger. 'We moved. To be alone.'"<sup>36</sup> Thus Chona clearly illustrated that she was conscious of the cycles of conquest that had shaped the region.

Underhill differed from Reichard in that she did not connect Chona's authority as an elder woman to her perpetuation or protection of Tohono O'Odham traditions. Where Dezba appeared in Reichard's writing as a bulwark of Navajo tradition, Chona was a more ambiguous figure with regard to cultural adaptation. While Underhill presented Chona's memories of her earlier life as the "most authentic" elements of Tohono O'Odham life, Chona did not make such a sharp differentiation between her "primitive" and her "modern" lives. In Chona's "primitive" life, she wore a loincloth, practiced running with her family to evade Apache raids, ate desert rats and seeds, and spoke Tohono O'Odham. In her "modern" life, Chona wore "Western" dress, rode in Underhill's car, ate white flour and lard, and spoke Spanish and some English.<sup>37</sup> Underhill presented the "primitive" elements as authentically Tohono O'Odham, shading each of Chona's rememberings of such

elements with nostalgia. When Chona recalled eating seeds and woodpeckers, Underhill amplified her statement by encouraging Chona to say, “Good food, that used to be.”<sup>38</sup>

Underhill’s differing examinations of Chona’s relationships with her various husbands indicates further Underhill’s presentation of “primitive” cultural practice as authentically Tohono O’Odham. A large portion of the text of *Papago Woman* focused on Chona’s life as a child, a young woman, and as a new bride. It examined Chona’s “coming of age” as a woman—menstrual rituals, courtship, matchmaking, and preparation to become a bride. Her marriage to her first husband, a medicine man, warranted some extended discussion and included long explanations of her first nights with her husband and the ways in which their marriage proceeded before Chona left him.<sup>39</sup> However, Underhill’s emphases in the story of Chona’s second husband were fundamentally different. The discussion of the first marriage emphasized Chona’s sexual awakening, her performance of “traditional” Tohono O’Odham labor (weaving and food preparation), and her many attempts to produce offspring. The discussion of her second marriage, however, emphasized Chona’s (negative and inauthentic) adaptations to Euro-American culture.

Where her first husband had been a “medicine man” who had taken multiple wives and had lived with Chona in a “hut” at Where the Water Whirls Around, Chona’s second husband had greater experience with traditions other than those of the Tohono O’Odham. Chona’s second husband took her to his village, Where the Rock Stands Up. This place was very different from either Mesquite Root (Chona’s birthplace) or Where the Water Whirls Around. To begin with, Chona said, it was “nearer to where the white people live.” Chona also lived in an adobe rather than brush hut there, with a fireplace and “a box full of money.” Her husband raised horses to sell to Apaches and to settlers.<sup>40</sup> Underhill’s examination of Chona’s second marriage did not present any discussion of Chona’s sexual feelings or practices, despite the fact that Chona had several children with her second husband. Further, there was no discussion of the sorts of work that Chona did during this marriage. As her husband preferred buying flour in Tucson to depending on the gatherings and grindings that Chona had been used to preparing, Chona would not have been responsible for those

duties.<sup>41</sup> During this time, too, Chona adopted “Western dress,” purchased from stores in Tucson. Chona did not sew these clothes; in Tucson, her husband pointed to something in a store window: “‘Don’t you want that over there?’ It was a sewing machine. I said, ‘What would I do with it?’ So we left it and came home.”<sup>42</sup> So Chona did not sew. Chona may have continued to weave, but *Papago Woman* did not mention it.

In the course of this second marriage, Chona adjusted to using cash. She noted that when her second husband sold his horses to the Apache, “they paid him money, not food and clothes, as my first husband used to be paid.” Because her second husband had cash, the family came to depend more on purchased food than on forage and gatherings. As a result of the rich lard and potatoes that her husband bought in Tucson, Chona said, “I got fat. I could not stoop over to put on those new shoes. [My relatives asked] ‘What have you been feeding her?’”<sup>43</sup>

With her second husband, Chona made further adjustments to contact with Euro-Americans. In many ways, Chona’s later life was lived half in and half out of the Anglo world. With the two sons she had with her second husband, Chona and her husband spent the winter living in Tucson where they “worked for the white people.” For Chona, this marked a change from her family’s way of going into Mexico for the winter. “In Tucson,” María Chona remembered, “we had a shack with mattresses in it. We could go to the stores and buy chili and salt to eat with our food. And white flour that we did not have to grind.”<sup>44</sup>

In Underhill’s text, Chona expressed regret over these adaptations to Euro-American traditions. Chona seemed to have never been too comfortable in Tucson. “I have never liked white flour,” she recalled. “I feel sick and weak when I eat it.” Being in Tucson was also spiritually unsettling for Chona, who recalled that “I had strange dreams in Tucson.”<sup>45</sup> Further, Underhill emphasized that Chona did not love her second husband. Chona recalled, “I did not love that old man. I was not fond of him.”<sup>46</sup> Meanwhile, Underhill emphasized that Chona had loved her first, “primitive” husband with an uncharacteristic passion.<sup>47</sup>

After her first husband died calling for Chona, Chona believed that he haunted her for the rest of her life.<sup>48</sup> Further, Underhill presented Chona toward the end of her life as lost in the Anglo world. She

represented Chona as living in poverty in a shack in Tucson, alone, afraid, and silenced. Only after her return to her ancestral lands on the reservation did Chona regain her voice and assert her authority as an elder woman.

Underhill's enumerations of Chona's loss of Tohono O'Odhamness indicate some deeper levels of complexity than they reveal on their surface. At numerous points in her narrative, Chona indicated that she thought of herself as "modern," and that she took pride in her "modern" identity. She noted the many ways in which she was ahead of her time. When she had her first child, instead of following the traditional way of having the medicine man name the child, she remembered, "We were modern and I let a priest name him, Bastian."<sup>49</sup> When Chona's father died, the family gave him Catholic rites: "We did not bury him in a cave in the rocks as our people used to do. We had been up and down to Mexico, and had seen the priests and learned many things. We laid him down and dug a grave."<sup>50</sup>

It seems evident that Underhill had invested herself in presenting Chona as "primitive" by indicating that the intrusions of "modern" Euro-American practices into Chona's life were things that Chona regretted and rejected. Underhill indicated several times that she saw Chona as a representative of a "primitive" culture and as a "primitive woman." For example, Underhill discussed ways in which most "primitive" women saw the anthropologist as a threat and that this impeded the anthropologist's work. Underhill said, "So many of those primitive women think . . . either they're not so good as you are, they're not as powerful, or you represent a terrible power that they can't calculate on, so they better not confide in you."<sup>51</sup> In this sense, Underhill saw Chona as quite different, although no less primitive. Underhill recalled, "She wasn't at all afraid of me. Now, some primitive women are just afraid of somebody from another culture. She might have witchcraft or all sorts of awful things, but Chona was a person of such personal power, she felt she didn't have to be afraid of anybody."<sup>52</sup> It would have been possible for Underhill to have attributed Chona's refusal to allow Underhill to intimidate her to Chona's professed "modern" identity; instead, Underhill decided that Chona's lack of fear was simply the behavior of a *powerful* primitive woman.

According to Underhill, Chona's attitude about the telling of her

life was: "Well, the white people ought to know these things. Too bad they don't. They're stupid people; they ought to be told these things." Underhill did not see this as conceit, but as a defense mechanism. When you are a Papago woman, Underhill reasoned, "A pride like that is a very useful thing."<sup>53</sup> But it is important to note that Underhill's evaluation of the pride behind such statements also carried Underhill's commitment to viewing Chona as a primitive, and therefore as authentically Tohono O'Odham. Underhill felt that Chona was a simpler soul, clearly not as capable of understanding cultural relativity as well as Underhill, the trained anthropologist. So, Underhill thought, Chona saw *milgahn* differences from the Papago as "stupidity" rather than simple "difference." This idea came through again and again in *Papago Woman*. But Chona had extensive experience with "white" people—she lived for over ten years in Tucson, she worked for whites, she lived with children who attended white schools in Phoenix. It is significant that Underhill consistently downplayed this interaction in *Papago Woman*, even while she documented it in interviews and in her own personal writings. When Ruth Underhill first met María Chona in Tucson in 1931, she noted that in her house "the most important element of furniture was the sunshine."<sup>54</sup> Underhill did not note, however, that the one-room house was Chona's own home in Tucson, and had been for over a decade. She did not note that Chona and her husband had purchased the house with cash from her husband's horse trading with whites.

That feminist ethnographers presented their informants in their published texts as more "primitive" than they in fact were is not all that surprising. In fact, presenting them as such ensured that their research would be considered part of the project of salvage ethnography. They could not have admitted in print the degree to which they had arrived too late to salvage what ethnographers tended at the time to define narrowly as authentic. This alone, however, did not cause them to construct Southwestern Native American women as primitive. More importantly, constructing Native American women as primitive was a part of a broader modernist agenda of searching for lost traditions among people who had not been corrupted by Victorianism. Defined broadly, modernists embraced primitivism as a way to rebel against Victorian ideals and to challenge with them Victorian scientific thought, including theories of cultural evolution.

As a part of this movement, feminist ethnographers often complemented the more general pattern of constructing primitive identity with examining primitive *womanhood*. However, not all feminist ethnographers did so in exactly the same way. Ruth Benedict, for one, rejected the idea of finding a basal primitive identity. For Elsie Clews Parsons, examinations of ways in which Pueblo women used natural forms of birth control or abortifacients became an important way for her to challenge the Victorian idea that women's control over their reproduction was unhealthy and unnatural. For Gladys Reichard, constructing Navajo "primitive" identity with the landscape allowed her to argue that the nonpatriarchal practices of the Navajo and the power of matrons like Dezba could better sustain Navajo culture against Euro-American incursion. For Ruth Underhill, constructing the primitiveness of Tohono O'Odham culture allowed her to argue that Chona's power in a patriarchal society decreased as she came more into contact with Victorian ideals. Thus, feminist ethnographers used the construction of primitive identity to further their varying feminist concerns. Further, the ways in which ethnographers presented informant primitiveness illustrated the cultural and ideological processing that took place between the informants' testimony and published ethnographic texts.

## Strands of Knowledge

The strands of knowledge in the Southwest always remained tangled.  
—Esther Schiff Goldfrank (1978)<sup>1</sup>

The feminist anthropologists addressed in this study represent many different strands of knowledge. As the first generations of women scholars to emerge as professionally trained anthropologists, they marked a transition of the discipline from amateur avocation to profession. They represented the emergence in the social sciences of a feminist critique of patriarchy that grew out of First Wave feminism. Although they shared their focus on feminism as a critique of patriarchy, they varied quite a bit in terms of personal styles and research concerns. Ruth Benedict focused chiefly on broad comparisons of psychological aspects across cultures. Much of her research in more general terms argued against prejudice, especially racial and gender prejudice. Elsie Clews Parsons focused her research on reconstructing the lives of women as mothers and daughters and on folklore and religious tradition. Gladys Reichard's research focused mostly on religion and linguistics, but her ethnographies and life-history work examined Navajo women's lives, with weaving traditions at their center. Ruth Underhill's work emphasized the Tohono O'odham, focusing on both gender roles and ritual poetics.

The feminism of the four ethnographers examined in this study grew out of intellectual trends and political forces that swirled around



them in the first half of the twentieth century. These included modernism, the concept of the "New Woman," and shifting paradigms of sexual identity. For these women ethnographers, contests over the definition of gender and sexuality consumed their interest intellectually as well as politically. Feminist ethnographers especially shared a common commitment to feminism and to researching the roles of women in various cultures. Their feminism focused on providing a critique of patriarchy, examining the ways in which patriarchal control had thwarted the development of individual women and contrasting this with the development of women's roles in more egalitarian and matrilineal cultures. Partly because of their First Wave feminist agenda of challenging assumptions of women's inferiority, and partly because of their knowledge of and interest in the occurrences and situations of gender equity, they laid the groundwork for defining gender as socially constructed. They certainly saw gender as a cultural construct that also changed through time. They assumed that Southwestern Native American societies allowed for a broader range of sexual expression and behaviors than was common in their own society. Finally, they assumed that naturalness provided answers for many of the dilemmas that plagued modern industrial America.

As a rule, these women set out to prove that gender identities resulted from cultural, rather than physiological, definitions.<sup>2</sup> Further, research about gender became an important way to assert their own legitimacy as anthropologists and scholars. According to Carol A. B. Warren, during the 1920s and 1930s, gender was "part of the structural grounds upon which negotiation took place."<sup>3</sup> Thus, by examining the social construction of gender in Southwestern Native American communities, women anthropologists could also examine the gender constructs that limited their career options and opportunities.

In addition, feminist researchers recognized that many Southwestern tribes treated men and women more equally than did their own society. This made it easier, many believed, for women to conduct research there than in patriarchal cultures. As Ruth Bunzel recalled, "Zuni is a woman's society. The women have a great deal of power and influence, so it's a good place for women to work."<sup>4</sup> Among the Pueblos and the Navajo, women anthropologists found societies that seemed to them to be more egalitarian regarding gender than was their own. As Florence

Hawley Ellis remembered, "When the Pueblo Indians think of you as a woman, I don't think they think of you as a lower creature the way our men tend to do." In fact, the relatively higher status afforded women in many Southwestern Native American societies transferred as well to women researchers, according to Ellis. "These Pueblo women are given an equality that is then passed onto the rest of us when we come into the picture."<sup>5</sup>

Of course, Southwestern Native American cultures had differing views of the place of women. The Apache and the Tohono O'Odham were patriarchal societies. Those who worked among these tribes—for example, Ruth Benedict among several Apache tribes, and Ruth Underhill among the Tohono O'Odham—did not record the special challenges that working in a patriarchal culture may have posed for women anthropologists. In part, women anthropologists may not have seen this aspect of their fieldwork as striking, for they themselves lived in a patriarchal culture; as anthropologists, they were much more likely to comment on fascinating differences and surprising similarities. Ruth Underhill did comment on such surprising similarities between her own experiences as a woman in Euro-American culture and Chona's among the Tohono O'Odham. She did so when she recounted ways in which her informant, María Chona, asserted herself in Tohono O'Odham culture and expressed pride in being a woman. On one occasion, Underhill commented, "That delightful attitude I should have been glad to take home with me."<sup>6</sup>

Women anthropologists may have also had a second reason for not discussing the difficulties of doing research among patriarchal cultures. In essence, women anthropologists were fighting for the legitimacy of their research, and fighting against chivalric arguments against the propriety of women doing fieldwork. The several reactions that followed the murder of a female anthropology doctoral student from Columbia University on the Whiteriver Apache reservation in Arizona in 1931 reveal why they might have felt it risky to discuss this topic in public. In the summer of 1931, Henrietta Schmerler was found murdered; an Apache man, one of her informants, eventually received a life sentence for murdering her. In the aftermath of the murder and throughout the sensational trial, the public exerted considerable pressure on the commissioner of Indian affairs to prevent it from happening again by

forbidding women to do fieldwork in the Southwest. Many commentators were swift to blame the murder victim for her own death; after all, she had gone alone into Apache lands and had resisted the authorities' attempts to control her movements. In response, Matthew Stirling, director of the Bureau of American Ethnology of the Smithsonian Institution, defended women's right to do fieldwork. He argued, "There is no real reason why a white woman would be in greater danger among Indians than among her own people, provided she has sufficient knowledge of Indian ways. It would seem inadvisable, however, for a young woman to live all alone in a remote region in any circumstances."<sup>7</sup> Franz Boas himself lamented in a letter to Ruth Benedict that the murder called into question his responsibility for his female students. He asked Benedict, "How shall we now dare to send a young girl out like this?"<sup>8</sup> Women anthropologists, realizing the risks involved, avoided directly referring to the difficulties of research in patriarchal cultures.

In addition to those who argued that women did not belong in the field, feminist ethnographers faced the same difficulties male anthropologists faced, most importantly informants' reticence or cultures of secrecy. Many questions went unanswered because they violated codes of secrecy or because informants claimed that they were not familiar with such practices. Once they found ways around such roadblocks, however, feminist ethnographers also approached their research with preconceptions. These preconceptions met with Native American informants of many different cultures that did not necessarily wish to "live up" to the ethnographers' expectations. As a result, when informants provided answers that were beyond what feminist ethnographers could accommodate given their expectations and their mindset, ethnographers sometimes grappled with, sometimes shaded, and sometimes erased the contradictions.

Because of these erasures, much of what feminist ethnographers published reflects their own worldview, rather than the informants' worldview. As such, they constitute fascinating ethnographers' autobiographies, especially when read alongside correspondence, journals, and other more traditionally autobiographical documents. As stories of ethnographers' concerns, they illuminate many arresting stories. They tell about feminist anthropologists' struggles to define themselves as women and to examine their own sexual identities. They trace the

often-contradictory emergence of feminist thought among women scientists in the early twentieth century. They illustrate feminist scholars' struggle with arguments about women's difference from men and women's equality to men. They track the uneven emergence of modernism in early anthropology, as well as the tensions of the "nature/nurture" debate in ethnological thought.

At the same time, however, critical readings of ethnographic texts reveal them to be less reliable primary documents of Native American life and values. This does not erase their value as documents for ethnohistory. Instead, it requires that scholars using such texts as primary sources historicize them, placing them in context as products of a specific community of scholars who shared to varying degrees a specific and historically situated mindset. Feminist ethnographers wrote the stories of Native American women to preserve and "salvage" their traditions, but they also wrote those stories with an agenda. Their agenda as feminists focused on providing a critique of patriarchy that proved several suppositions. First, they set out to argue that patriarchy was not a "natural" social structure, but instead resulted from a series of cultural values that devalued women and placed men in positions of authority with power over them. Second, they sought to argue that cultures which valued egalitarian relations between the sexes were made more stable and cohesive by those egalitarian practices and traditions. Third, they set out to assert that "primitive" cultures—as they judged Southwestern Native American cultures to be—provided greater individual freedom than did Euro-American culture, especially with regard to variations in gender roles, and gender and sexual identities.



# Notes

## Introduction

1. Benedict, "Folklore," 291.
2. Lemons, *The Woman Citizen*. In making this distinction, he builds on the work of William O'Neill, in "Feminism as a Radical Ideology."
3. Cott, *The Grounding of Modern Feminism*.
4. Lane, *To "Herland" and Beyond*.
5. Lerner, "Women's Rights and American Feminism," 236–37.
6. Stocking, *Victorian Anthropology*; Stocking, ed., *Observers Observed*; and Stocking, *The Ethnographer's Magic*; Kuklick, *The Savage Within*; Clifford and Marcus, eds., *Writing Culture*; and Clifford, *The Predicament of Culture*.
7. Stocking, "The Ethnographic Sensibility of the 1920s," in *The Ethnographer's Magic*, 187–237. Other discussions of this transformation include: May, *The End of American Innocence*; Lears, *No Place of Grace*; Leuchtenberg, *The Perils of Prosperity*; Nash, *The Nervous Generation*; Baritz, "The Culture of the Twenties"; and Singal, ed., *Modernist Culture in America*.
8. Bieder, *Science Encounters the Indian*; Hinsley, *Savages and Scientists*.
9. Basso, "History of Ethnological Research," 14–21.
10. Redfield, *Tepoztlán*; and Lewis, *Life in a Mexican Village*; Mead, *Coming of Age in Samoa*; and Freeman, *Margaret Mead and Samoa*; Freeman, "On Franz Boas and the Samoan Researches of Margaret Mead"; Freeman, "'There's Tricks i' th' World.'"
11. Albers, "From Illusion to Illumination"; and Medicine, "The Anthropologist as the Indian's Image Maker."
12. Clarke, *Sex in Education*, esp. 37; and Maudsley, "Sex in Mind and Education." To place these works in context, see Russett, *Sexual Science*.

13. For an overview of this impetus within feminism, see Rosenberg, "In Search of Woman's Nature." For an example of how these struggles played out in the lives of one community of women and one strain of American thought, see Pittenger, "Evolution, 'Woman's Nature' and American Feminist Socialism."
14. Bataille and Sands, *American Indian Women*.
15. *Ibid.*, p. 3.
16. *Ibid.*, p. 4.
17. In 1992, Lamphere edited an issue of *Frontiers: A Journal of Women Studies* focusing on the Southwest, which included essays about women anthropologists in the Southwest by both Babcock and herself; see *Frontiers* 12, no. 3 (1992). In addition, see Parezo, *Hidden Scholars*.
18. See Babcock's essay, "Elsie Clews Parsons and the Pueblo Construction of Gender," introducing her edition of *Pueblo Mothers and Children*. Babcock also produced, with Museum of Arizona Curator of Ethnology Nancy Parezo, *Daughters of the Desert*, an illustrated catalog of an exhibit at the University of Arizona.
19. Lamphere, "Women, Anthropology, Tourism, and the Southwest"; and Lamphere, "Gladys Reichard among the Navajo."
20. Scott, "Gender"; see also Ortner and Whitehead, eds., *Sexual Meanings*.
21. Russett, *Sexual Science*. See also Rosenberg, *Beyond Separate Spheres*; and Fitzpatrick, *Endless Crusade*.
22. For a discussion of the significance of conquest in the history of the West, see: Limerick, *The Legacy of Conquest*; and Casteñeda, "Women of Color and the Rewriting of Western History."
23. Spicer, *Cycles of Conquest*.
24. For the gendering of conquest, see: Morrissey, "Engendering the West." On female moral authority, see: Pascoe, *Relations of Rescue*.
25. Two overviews that illustrate the fecundity of recent research in the history of sexuality are: D'Emilio and Freedman, *Intimate Matters*; and Duberman, Vicinus, and Chauncey, eds., *Hidden from History*.
26. Foucault, *The History of Sexuality*.
27. Epstein and Straub, eds., *Body Guards*; and Garber, *Vested Interests*.
28. Lacquer, *Making Sex*.
29. Todorov, *The Conquest of America*.
30. Smith, *The View from Officers' Row*; an interesting example of this can be found in Martha Summerhayes's *Vanished Arizona*, in which she describes her Cocopah servant Charley, whose "gee-string" costume "showed the supple muscles of his clean-cut thighs," as "tall, and well-made, with clean-cut limbs and features, fine smooth copper-colored skin, handsome face. . . . This was my Charley, my half-tame Cocopah" (156, 150).
31. Hutton, *Phil Sheridan and His Army*, esp. 80–81, 144, 194.
32. Williams, *The Spirit and the Flesh*; Will Roscoe, *The Zuñi Man-Woman*.
33. Gutiérrez, *When Jesus Came, the Corn Mothers Went Away*.

## Chapter 1

1. Mead, "Apprenticeship under Boas," 43.
2. Basso, "History of Ethnological Research," 14.
3. Goldfrank, *Notes on an Undirected Life*, 42.
4. Bennett, "The Interpretation of Pueblo Culture," 361.
5. Hinsley, *Savages and Scientists*, as quoted in Parezo, "Conclusion," 359.
6. Kuhn, *The Structure of Scientific Revolutions*.
7. Crane, *Invisible Colleges*, 99.
8. Boas's opposition to the cultural evolutionary ideas of Tylor and Morgan appears in Boas, "The Methods of Ethnology" and "The Aims of Ethnography," 281 and 637, respectively.
9. Franz Boas, 1858–1942, trained an entire generation of the finest anthropologists, including, beside Underhill and Ruth Benedict, Ruth Bunzel, Melville Herskovits, Zora Neale Hurston, Alfred Kroeber, Robert Lowie, Margaret Mead, Edward Sapir, Leslie Spier, and John Swanton. A fine biography of Boas is Hyatt's *Franz Boas, Social Activist*. Boas's papers are held by the American Philosophical Society, Philadelphia.
10. Kuklick, in *The Savage Within*, discusses the development of British anthropology during this period, and recounts a very different story from the development of the "American School" associated with Boas and his students.
11. Boas, *The Mind of Primitive Man*, 42.
12. Boas, "A Journey in Cumberland Sound," 271.
13. Mead, "Apprenticeship under Boas," 30.
14. *Ibid*, 43.
15. *Ibid*.
16. Oral interview with Mrs. Tom Johnson, Fort Rupert, British Columbia, by Ronald R. Rohner, June 17, 1964; in Ruth Murray Underhill Collection, F. H. Douglas Memorial Library, Denver Art Museum (hereafter, DAM-Underhill), Box 2, Folder 5, 1.
17. DAM-Underhill, Box 2, Folder 5, 4.
18. Marcus and Fischer, *Anthropology as Cultural Critique*, 129.
19. Boas, *Anthropology and Modern Life*, x.
20. Boas, "An Anthropologist's Credo," 201.
21. Marcus and Fischer, *Anthropology as Cultural Critique*, 130.
22. Underhill, *Papago Woman*, ix.
23. Smith, "Boas' 'Natural History' Approach to Field Material," 54.
24. Mead, "Apprenticeship under Boas," 32.
25. *Ibid.*, 32; see also Mead, *An Anthropologist at Work*, part 3.
26. Mead, "Apprenticeship under Boas," 32.
27. Barnouw, "Ruth Benedict," 252.
28. Quoted in Harris, *The Rise of Anthropological Theory*, 281.
29. Sapir, "Do We Need a Superorganic?"



30. Malinowski, *Sex and Repression in Savage Society*.
31. Kardiner, *The Individual and His Society*, and Linton, *The Cultural Background of Personality*, were an outcome of these seminars.
32. An example of a classic of this type of study is DuBois's *The People of Alor*.
33. Spier, "Havasupai Ethnography"; and Spier, *Yuman Tribes of the Gila River*.
34. Parsons, "Isleta, New Mexico."
35. Forde, "Ethnography of the Yuma Indians"; and Gifford, "The Cocopa."
36. Kroeber, ed., "Walapai Ethnography."
37. See especially Parsons's notes for the late 1910s in the Elsie Clews Parsons Papers, American Philosophical Society (hereafter, APS-Parsons), 572/P25.1.
38. Kroeber, "Handbook of Indians of California"; and Kroeber, "Cultural and Natural Areas of Native North America."
39. Spier, "Problems Arising from the Cultural Position of the Havasupai."
40. Beals, "The Comparative Ethnology of Northern Mexico before 1750."
41. Beals, "Northern Mexico and the Southwest."
42. Strong, "An Analysis of Southwestern Society"; and Haurly, "The Problem of Contacts between the Southwestern United States and Mexico."
43. Steward, "Ecological Aspects of Southwestern Society."
44. Goldfrank, "Irrigation Agriculture and Navaho Community Leadership."
45. Parsons, "Spanish Elements in the Kachina Cult of the Pueblos"; and Parsons and Beals, "The Sacred Clowns of the Pueblo and Mayo-Yaqui Indians."
46. Opler, "The Kinship Systems of the Southern Athapaskan-Speaking Tribes."
47. Spicer, *Pascua*; Titiev, "Old Oraibi"; Eggan, *Social Organization of the Western Pueblos*.
48. Underhill, *Social Organization of the Papago Indians*.
49. Opler, *An Apache Life-Way*; Kluckhohn and Leighton, *The Navaho*.
50. Parsons, *Pueblo Indian Religion*.
51. Underhill, *Ceremonial Patterns in the Greater Southwest*.
52. Reichard, *Navajo Religion*.
53. Benedict, "Zuni Mythology"; Kluckhohn, "Navaho Witchcraft"; Kluckhohn and Leighton, *The Navaho*.
54. Kroeber, "Preface" to *The Anthropology of Franz Boas*, vi.
55. Haeberlin, "The Idea of Fertilization in the Culture of the Pueblo Indians."
56. Benedict, "Psychological Types in the Cultures of the Southwest"; Benedict, "Configurations of Culture in North America"; and Benedict, *Patterns of Culture*.

57. Kluckhohn, "Patterning as Exemplified in Navaho Culture"; Eggan, "The General Problem of Hopi Adjustment."
58. Goldfrank, "Socialization, Personality, and the Structure of Pueblo Society"; Thompson, "Logico-Aesthetic Integration in Hopi Culture."
59. Simmons, ed., *Sun Chief*.
60. Leighton and Leighton, "Gregório"; Aberle, "The Psychosocial Analysis of a Hopi Life-History."
61. Benedict, "Zuni Mythology."
62. Goldfrank, "The Impact of Situation and Personality on Four Hopi Emergence Myths."
63. Eggan, "The Significance of Dreams for Anthropological Research."
64. Kluckhohn, "Navaho Witchcraft."
65. Dennis, *The Hopi Child*; Kluckhohn and Rosenzweig, "Two Navaho Children over a Five-Year Period." Another example of this sort of study by a Boasian is Goldfrank, "Socialization, Personality, and the Structure of Pueblo Society."
66. Devereux, "Institutionalized Homosexuality of the Mohave Indians"; Devereux and Loeb, "Some Notes on Apache Criminality"; Devereux, "The Function of Alcohol in Mohave Society."
67. Hill, "Navaho Warfare"; and Hill, "The Agricultural and Hunting Methods of the Navaho Indians"; Kluckhohn and Wyman, "An Introduction to Navaho Chant Practice."
68. Underhill, *Social Organization of the Papago Indians*; and Underhill, *Papago Indian Religion*.
69. Spicer, *Pascua*; Spicer, "Potam"; Bennett and Zingg, *The Tarahumara*.
70. Kroeber, "Handbook of Indians of California"; Spier, "Havasupai Ethnography"; Spier, *Yuman Tribes of the Gila River*; Forde, "Ethnography of the Yuma Indians"; Gifford, "The Southwestern Yavapai"; and Gifford, "Northeastern and Western Yavapai."
71. Rosenberg, *Beyond Separate Spheres*, esp. 28-53.
72. Boas to Berthold Laufer, July 23, 1920, in Franz Boas Papers, American Philosophical Society, Philadelphia (hereafter, APS-Boas).
73. Cott, *Grounding of Modern Feminism*.
74. Debo, *And Still the Waters Run*.
75. Leckie, *Angie Debo*, 66.
76. Mead, "Introduction" to *The Golden Age of American Anthropology*, 5.
77. Ruth Bunzel, interviewed by Jennifer Fox, July 2, 1985. Videotapes in Wenner-Gren Foundation, New York City.
78. Lowie, *A History of Ethnological Theory*, 134.
79. It is important also to note that all of these scholars left behind large bodies of work and collections of their papers for scholars to use. The one exception to this rule is Gladys Reichard, whose personal papers are not as complete as the rest; however, she left behind a core of autobiographical writings that begin to make up for the lack of extensive

- personal correspondence files, and much of her correspondence appears in the collections of Benedict, Underhill, Mead, Parsons, and Franz Boas.
80. Stevenson is a figure about whom there has been considerable controversy but insufficient scholarship. McFeely's *Zuni and the American Imagination* discusses her work and life in some detail, but the authoritative study of Stevenson's work remains Parezo's "Matilda Coxe Stevenson: Pioneer Ethnologist."
  81. Boas's daughter Franziska Boas recalled of the choice for senior professor in 1972, "It should have been Benedict." When Franziska Boas's male interviewer responded, "Yes, but she was a woman," Boas replied, "They didn't know it before?" See Franziska Boas, *Reminiscences*, 1972, 77 pp. typescript (produced by the Oral History Research Office, Columbia University), conducted by John Cole, in August 1972, in Sandisfield, Massachusetts. In APS-Boas, B/B61re, 66.
  82. Benedict, "Women and Anthropology."
  83. Bunzel, quoted in Parezo, "Anthropology: The Welcoming Science," 24.
  84. Rossiter, *Women Scientists in America*, 272.
  85. *Ibid.*, 39.

## Chapter 2

1. For an overview of Parsons's life, see Hare, *A Woman's Quest for Science*; Zumwalt, *Wealth and Rebellion*; and, especially, Deacon, *Elsie Clews Parsons*. For a discussion of Parsons in her social and cultural milieu, and her fieldwork in general, see Rosenberg, *Beyond Separate Spheres*, especially 147–77; Babcock and Parezo, "The Leading Edge"; and Deacon, "The Republic of the Spirit." Parsons's papers are held by the American Philosophical Society and include, in addition to copious personal papers, extensive field notes, upon many of which she never based published works (APS-Parsons).
2. Boyer, "Parsons, Elsie Clews," 20; and Hare, *A Woman's Quest for Science*, 23–29.
3. Rosenberg, *Beyond Separate Spheres*, 153.
4. Boas, "Elsie Clews Parsons," *Science*, 89.
5. Boas, "Elsie Clews Parsons" *Scientific Monthly*, 481.
6. Kroeber, "Elsie Clews Parsons" *American Anthropologist*, 252.
7. Hare, *A Woman's Quest for Science*, 107–21; see also Herbert Parsons (Elsie's son) to Major Herbert Parsons, Feb. 3, 1918, in France with the 5th Division of the American Expeditionary Force, APS-Parsons, Series VI—Peter Hare Papers, Box 1.
8. Kroeber, "Elsie Clews Parsons," 252.
9. *Ibid.*, 253.
10. Hare, *A Woman's Quest for Science*, 44–45.
11. Boyer, "Parsons, Elsie Clews," 21; Rosenberg, *Beyond Separate Spheres*, 168–69; Stocking, "Ideas and Institutions in American Anthropology," 33.

12. Schwarz, *Radical Feminists of Heterodoxy*.
13. Other members of Heterodoxy, founded in 1912 by Marie Jenny Howe, a Unitarian minister, included: Ruth Hale, a journalist; Henrietta Rodman, a teacher; Leta Stetter Hollingworth, a psychologist; Doris Stevens, a suffragist; Rheta Childe Dorr, a journalist; and Rose Pastor Stokes, a socialist trade unionist.
14. Hare, *A Woman's Quest for Science*, 14; Rosenberg, *Beyond Separate Spheres*, 161–62.
15. Hare, *A Woman's Quest for Science*, 7.
16. Quoted in *ibid.*, 102.
17. Parsons, *The Family*, 351–52.
18. Sanger, *Margaret Sanger*, 189.
19. Bourne, "A Modern Mind," 239.
20. Spier, "Elsie Clews Parsons," 244.
21. Parsons, "In the Southwest," quoted in Babcock and Parezo, *Daughters of the Desert*, 15.
22. Spier, "Elsie Clews Parsons," 246.
23. *Ibid.*, 250.
24. Parsons quoted in Rosenberg, *Beyond Separate Spheres*, 166.
25. Spier, "Elsie Clews Parsons," 246.
26. Lamphere, "Feminist Anthropology," 525.
27. Southwest Society aid went to Ruth Benedict, Franz Boas, Ruth Bunzel, Esther Goldfrank, Berard Haile, Dorothy Keur, Morris Opler, Gladys Reichard, Ruth Underhill, Charles Wagley, and Leslie White.
28. Weltfish is quoted in Mintz, "Ruth Benedict," 165.
29. Alfred Kroeber to Gladys Reichard, May 26, 1942, in Department of Anthropology files, Kroeber Correspondence Files, Bancroft Library, University of California, Berkeley (hereafter Bancroft-Kroeber).
30. Parsons, "Notes on Ceremonialism at Laguna," 87.
31. Parsons, "Mothers and Children at Laguna"; Parsons, "Mothers and Children at Zuni, New Mexico"; Parsons, "Hopi Mothers and Children"; and Parsons, "Tewa Mothers and Children."
32. Boas and Parsons also later traveled together in the Southwest, in the summer of 1920 and the fall of 1921.
33. Parsons, "A Hopi Ceremonial."
34. Parsons, "Hopi Mothers and Children"; Parsons, "Getting Married on First Mesa, Arizona."
35. Parsons, "Hopi Tales by Alexander MacGregor"; Parsons, *The Hopi Journals of Alexander M. Stephen*.
36. Parsons, *The Pueblo of Jemez*.
37. Parsons, *The Social Organization of the Tewa in New Mexico*, 9.
38. For an account of this relationship, see Arthur Huff Fauset to Peter Hare, August 1970, in APS-Parsons, Series VI—Peter Hare Papers, Box 1.
39. Pandey, "Anthropologists at Zuni."

40. Lewis called Parsons "Lady Mine" in a 3 December 1919 letter from Zuni, New Mexico; Lewis called Parsons "Dear Lady" in a June 2, 1932 letter from Zuni, New Mexico; APS-Parsons.
41. Hare, *A Woman's Quest for Science*, 133.
42. Parsons, "In the Southwest—The Governor of Zuni," 5–6, in APS-Parsons, Series III—Manuscripts, Box 3.
43. *Ibid.*, 6.
44. Parsons, "Notes on San Felipe and Santo Domingo," 485.
45. Parsons, "Spanish Tales from Laguna and Zuni," 47 n.1.
46. Parsons, Notebook Six: "Taos Nov. 1926 I," in APS-Parsons, Box 11 (Taos Notebooks 1922–1933).
47. Parsons, *Taos Pueblo*.
48. Ralph Beals, Department of Anthropology, UCLA, to Peter Hare, Department of Philosophy, SUNY-Buffalo, December 19, 1978, APS-Parsons, Series VI—Peter Hare Papers, Box 1.
49. Hare, *A Woman's Quest for Science*, 147. It is important to note that in repeating this handed-down depiction of Stevenson as a nonscientist who got results because she got her informants drunk, Ralph Beals relegated Stevenson to the status of an unprincipled "hobbyist" who lacked the ability to undertake serious scientific work.
50. Ellis, "Across Some Decades," 302.
51. Elsie Clews Parsons to Mabel Dodge Luhan, January 4, 1939, in APS-Parsons, Correspondence, Box 1, A-L, "Luhan, Mabel Dodge."
52. Tony Mirabal to Elsie Clews Parsons, dictated to Mabel Dodge Luhan, January 3, 1939, in APS-Parsons, Correspondence, Box 1, A-L, "Luhan, Mabel Dodge."
53. Elsie Clews Parsons to Mabel Dodge Luhan, January 4, 1939, in APS-Parsons, Correspondence, Box 1, A-L, "Luhan, Mabel Dodge."
54. Tony Mirabal to Elsie Clews Parsons, dictated to Mabel Dodge Luhan, January 3, 1939, in APS-Parsons, Correspondence, Box 1, A-L, "Luhan, Mabel Dodge."
55. Parsons, "Notes on Isleta, Santa Ana, and Acoma," 56.
56. *Ibid.*
57. Parsons, "Notes on San Felipe and Santo Domingo," 485.
58. Parsons, "Further Notes on Isleta," 149.
59. Parsons, *The Social Organization of the Tewa in New Mexico*, 7–9.
60. *Ibid.*
61. Parsons, "Nativity Myth at Laguna and Zuñi," 258 n. 4.
62. Parsons, "A Few Zuni Death Beliefs and Practices," 250.
63. Parsons, "Pueblo-Indian Folk-tales, Probably of Spanish Provenance."

64. Parsons, Notes for a lecture on "Getting Information from Pueblo Indians," n.d., in APS-Parsons, Series III—Manuscripts, Box 3.
65. Ralph Beals, Department of Anthropology, UCLA, to Peter Hare, Department of Philosophy, SUNY-Buffalo, December 19, 1978, APS-Parsons, Series VI—Peter Hare Papers, Box 1.
66. Elsie (Lissa) Parsons Kennedy, interviewed November 11, 1962, by Louis M. Starrano and Allan Nevins, at Lenox, Massachusetts, original in Columbia University Oral History Project, Columbia University. In APS-Parsons, Series VI—Peter Hare Papers, Box 1.
67. Ibid.
68. Ralph Beals, Department of Anthropology, UCLA, to Peter Hare, Department of Philosophy, SUNY-Buffalo, December 19, 1978, APS-Parsons, Series VI—Peter Hare Papers, Box 1.
69. Ibid.
70. Ibid.
71. Elsie (Lissa) Parsons Kennedy, interviewed November 11, 1962, by Louis M. Starrano and Allan Nevins, at Lenox, Massachusetts, original in Columbia University Oral History Project, Columbia University. In APS-Parsons, Series VI—Peter Hare Papers, Box 1.
72. Elsie Clews Parsons to Herbert Parsons, September 20, 1905, in APS-Parsons, Correspondence.
73. Reichard, "Elsie Clews Parsons" *Journal of American Folklore*, 48.
74. Ralph Beals, Department of Anthropology, UCLA, to Peter Hare, Department of Philosophy, SUNY-Buffalo, December 19, 1978, APS-Parsons, Series VI—Peter Hare Papers, Box 1.
75. Kroeber wrote to Parsons in 1916: "Dear Propagandist: You write provocative books that are distinctly good and very clever unethnological articles on ethnological subjects." See Hare, *A Woman's Quest for Science*, 137. Babcock discusses the more general dismissal of Parsons's work by male colleagues in "Taking Liberties," 396. To be fair, however, it is important to note that Franz Boas expressed unreserved respect for Parsons's work; however, he never ensured that Parsons secured a professorship of either anthropology or sociology.
76. Babcock, "Taking Liberties," 396.
77. Parsons, "Review of *Zuni Mythology* by Ruth Benedict and *Zuni Texts* by Ruth Bunzel," 108.
78. Ruth Benedict to Margaret Mead, New York, September 21, 1928, in Margaret Mead Papers, American Philosophical Society, B/B428.mx (hereafter, APS-Mead).
79. Ruth Benedict to Margaret Mead, New York, December 29, 1928. In APS-Mead.

## Chapter 3

1. For an overview of Benedict's life, see Mead, *Ruth Benedict*; Modell, *Ruth Benedict*; Caffrey, *Ruth Benedict*; Mintz, "Ruth Benedict." For an overview of Benedict's work, see Mead, *An Anthropologist at Work*. Benedict's copious papers are held by the Vassar College Library in Poughkeepsie, NY. However, because much of her ethnological writing built upon the field research of others, including Franz Boas, field notes do not account for a large portion of her papers. Further, as Margaret Mead was Benedict's literary executor, much of Benedict's correspondence is archived in Margaret Mead's papers at the American Philosophical Society in Philadelphia, Pennsylvania (APS-Mead).
2. Babcock and Parezo, "Review of *Ruth Benedict* by Margaret M. Caffrey."
3. See also Mintz, "Ruth Benedict"; Modell, "Ruth Benedict, Anthropologist," in *Toward a Science of Man*, 199–202. Benedict adopted three pseudonyms throughout her life. The earliest, chosen while she was married to Stanley Benedict, was Edgar Stanhope, a play on Stanley's name, her hopes for the marriage, and Edgar Allen Poe. Later, still married to Stanley, she adopted Ruth Stanhope. As her marriage to Stanley Benedict began to crumble, she adopted Anne Singleton as a pseudonym under which to publish her poems.
4. Parezo, "Anthropology: The Welcoming Science," 14.
5. *Ibid.*, 15.
6. As quoted in Roger Sanjek, "The American Anthropological Association Resolution on the Employment of Women," 848.
7. The appointment of Ralph Linton, additionally, marked an attempt to free the Columbia Anthropology Department from the shadow of Franz Boas. Linton had attempted to enroll at Columbia after World War I, had gone to Boas's office in his army uniform, and been made unwelcome. Linton had then enrolled at Harvard and received his Ph.D. there. Thus, Linton represented a move away from Boas's brand of social activism, focused on pacifism, especially. See Linton and Wagley, *Ralph Linton*, 14. According to his daughter Franziska Boas, Boas saw the appointment of Linton over Benedict as a renunciation of Boasian tradition at Columbia University.
8. See Sanjek, "The American Anthropological Association Resolution on the Employment of Women," 848. Although Sanjek does not provide statistics on the rates of promotion among males of Benedict's stature, it is clear that Benedict was promoted at a much slower rate than the majority of comparable male academics. Further, her record of publication, teaching, and public service were exemplary, and would not have been a bar to her advancement.
9. Modell, "Ruth Fulton Benedict," in *Women Anthropologists*, 1; see also Modell, *Ruth Benedict*.

10. Modell, "Ruth Fulton Benedict," 1.
11. Benedict discussed the discovery of her deafness and its more general impacts on her life and childhood in Mead, ed., *An Anthropologist at Work*, 105.
12. See Modell, *Ruth Benedict*, 39.
13. Mead, ed., *An Anthropologist at Work*, 136.
14. *Ibid.*, 140.
15. Ruth Benedict, Journal, not dated, but probably late 1910s, Ruth Fulton Benedict Papers, Special Collections, Vassar College Library, Poughkeepsie, NY (hereafter, Vassar-Benedict).
16. See her mentions of "blue devils" and "devils" in Mead, ed., *An Anthropologist at Work*, 100, 102, and 105; Benedict also discusses her blue devils throughout her undated journal, including the journal most likely to have been kept in 1914–15. See Ruth Benedict Journal, no date, Vassar-Benedict.
17. Ruth Benedict Journal, entry not dated, Vassar-Benedict. While there is no date on the journal entries, she made the entries in the same journal notebook in which she kept her 1912 journal entries made when she was in Pasadena, and at the beginning of the entries not dated she writes, "I have barely written here for a year or more." For that reason, and because of the mentions she makes of her new marriage to Stanley Benedict, she most probably made the entries in late 1914 or 1915.
18. According to Judith Modell, Margaret Mead had told her that Benedict's blocked fallopian tubes were the cause of her inability to conceive, and that Stanley Benedict had vetoed surgery as being far too risky. Modell, *Ruth Benedict*, 97.
19. Benedict wrote a poem to her mother during her courtship with Stanley that contained some of the self-imagery that Benedict feared. She wrote:

I see the taut preciseness of her tucked-in bed.  
 And I saw—I saw my own pity in her eyes.  
 And now I knew that I too show first gray streaks of hair,  
 That I too taste white collars,  
 That I too punctuate the day by going out to meals.  
 I did not know before.

It is significant of Benedict's struggle over the meaning of the connection between her mother's chaste widowhood and her own "taste [for] white collars" that she had first used in place of "pity" the words "indifferent" and then "carnal," both of which are crossed out on the manuscript. See Benedict, "Awakening," in Vassar-Benedict.

20. Ruth Benedict, Journal, July 26, 1930, Vassar-Benedict.
21. Journal entry for March 22, 1930, Vassar-Benedict.



22. Barnouw, "Ruth Benedict," 505.
23. Mead, ed., *An Anthropologist at Work*, 132.
24. *Ibid.*, 142.
25. *Ibid.*, 141.
26. Six drafts of the Wollstonecraft essay and the undated "preface" to the biographical essays, Vassar-Benedict.
27. For the intellectual employment of Mary Wollstonecraft's life story by other early twentieth-century feminist writers, see Todd, "The Biographies of Mary Wollstonecraft," 725.
28. Second Draft of "Mary Wollstonecraft," Vassar-Benedict.
29. First and Third Drafts of "Mary Wollstonecraft," Vassar-Benedict.
30. Sixth Draft of "Mary Wollstonecraft," Vassar-Benedict.
31. Draft of "Preface," in Vassar-Benedict.
32. First draft of "Mary Wollstonecraft," in Vassar-Benedict.
33. Speech to the American Association of University Women, June 1946, Vassar-Benedict.
34. For Benedict's views of Goldenweiser, see her draft of an obituary for him, 1940, Vassar-Benedict.
35. Mead, *Anthropologist at Work*, 179.
36. *Ibid.*, 61–62.
37. Benedict, course notes for Columbia courses, no date, Vassar-Benedict.
38. Weltfish, quoted in Mintz, "Ruth Benedict," 165.
39. Benedict discussed her fears of being alone during this time in her correspondence with Edward Sapir, the head of the Canadian Geological Survey and a former student of Boas who had begun to correspond with Benedict in 1922. Benedict shared her poetry with Sapir, and Sapir turned to Benedict throughout depressions caused by his first wife's treatment for mental illness and his inability to find a job in a U.S. university. Much to Benedict's delight, Sapir left his children with her when he brought his wife to New York for treatment. Sapir later accepted a position at the University of Chicago and then at Yale, where he founded the Department of Anthropology. Their correspondence was published in Mead, ed., *An Anthropologist at Work*.
40. Mintz, "Ruth Benedict," 142.
41. Ruth Benedict to Margaret Mead, October 16, 1932, APS-Mead.
42. Fortune, *The Sorcerers of Dobu*.
43. An overview of the impact of Benedict's ideas on Apollonian and Dionysian cultures appears in Barnouw, "Ruth Benedict: Apollonian and Dionysian."
44. Benedict, *Patterns of Culture*, 75.
45. *Ibid.*, 75.
46. *Ibid.*, 74.
47. *Ibid.*, 203.
48. *Ibid.*, 203–4.

49. Ruth Benedict to Margaret Mead, from Santa Fe, August 25, 1925, in APS-Mead.
50. Ruth Benedict to Margaret Mead, from Peña Blanca, August 29, 1925, in APS-Mead.
51. Ruth Benedict to Margaret Mead, from Cochiti Pueblo, September 5, 1925, in APS-Mead.
52. Ruth Fulton Benedict Journal, not dated, in Vassar-Benedict; she refers to her nine-hour transcription sessions in letters to Mead, including Ruth Benedict to Margaret Mead, from Cochiti, September 16, 1925, in APS-Mead.
53. Ruth Benedict to Margaret Mead, from Cochiti Pueblo, September 8, 1925, in APS-Mead.
54. Ruth Bunzel to Franz Boas, from Cochiti, July 5, 1926, in APS-Boas.
55. Mead, *Anthropologist at Work*, 292.
56. *Ibid.*, 303.
57. Ruth Benedict to Margaret Mead, September 8, 1925, in APS-Mead.
58. Parsons, *Pueblo Indian Religion*, 1:65.
59. Ruth Underhill to Margaret Mead, from Zuñi, August 15, 1925, reprinted in Mead, *Anthropologist at Work*, 292.
60. Ruth Benedict to Margaret Mead, Cochiti, September 16, 1925, in APS-Mead.
61. Jaime de Ángulo, an anthropology student with a Zuñi father, quoted Benedict back to her in a horrified letter to Benedict from Berkeley, California, written on May 19, 1925. Vassar-Benedict.
62. Mead, *Anthropologist at Work*, 296, 299.
63. Ruth Benedict to Margaret Mead, from Cochiti, September 8, 1925, APS-Mead.
64. *Ibid.*
65. Ruth Benedict to Franz Boas, 1932, APS-Boas.
66. Ruth Benedict to Margaret Mead, from Cochiti Pueblo, September 5, 1925, in APS-Mead.
67. Ruth Benedict to Margaret Mead, from Cochiti Pueblo, September 8, 1925, in APS-Mead.
68. Jaime de Ángulo to Ruth Benedict, May 19, 1925, in Vassar-Benedict.
69. Barnouw, "Ruth Benedict," 241.

## Chapter 4

1. For an overview of Reichard's work, see Smith, "Gladys Armada [*sic*] Reichard"; Lyon, "Gladys Reichard at the Frontiers of Navajo Culture"; Lamphere, "Gladys Reichard among the Navajo"; and Babcock and Parezo, *Daughters of the Desert*, 46–51. Her papers are held at the Museum of Northern Arizona in Flagstaff, although her field notes are not included among her papers. Most of her correspondence is to be found in the Franz Boas Papers at the American Philosophical Society

- (APS-Boas) or in other correspondents' papers, rather than in her collection at the Museum of Northern Arizona. Reichard's work is intensely personal and autobiographical, and she is especially present in "I, Personally"; *Spider Woman*; and *Navajo Shepherd and Weaver*.
2. Franziska Boas, *Reminiscences*, 1972, 45; 77 pp. typescript (produced by the Oral History Research Office, Columbia University), conducted by John Cole, in August 1972, in Sandisfield, Massachusetts. In APS-Boas.
  3. Edward Sapir to Franz Boas, May 12, 1927, APS-Boas.
  4. Quoted in Lamphere, "Gladys Reichard among the Navajo," in Parezo, *Hidden Scholars*, 177.
  5. Ruth Fulton Benedict journal entry for February 13, 1923, Vassar-Benedict.
  6. Edward Sapir to Ruth Benedict, March 16, 1931, Vassar-Benedict.
  7. Frank R. Lillie, on behalf of the National Research Council, to Elsie Clews Parsons, May 23, 1924, in Vassar-Benedict.
  8. Reichard, "The Hogan School," manuscript in Gladys Reichard Papers, Museum of Northern Arizona, Flagstaff (hereafter, MNA-Reichard).
  9. Griffen, "Ruth Underhill," 357.
  10. Reichard, "Elsie Clews Parsons"; and Elsie Clews Parsons to Reichard, August 16, 1923, in MNA-Reichard (29-33).
  11. Gladys Reichard to Elsie Clews Parsons, September 4, 1923, in APS-Parsons.
  12. Reichard discusses driving Red Point (Miguelito) and his family around during her stay there in *Spider Woman*.
  13. Gladys Reichard to Elsie Clews Parsons, September 24, 1924, in APS-Parsons.
  14. Clyde Kluckhohn to Gladys Reichard, November 12, 1943, MNA-Reichard.
  15. Lamphere, "Gladys Reichard among the Navajo," in Parezo, *Hidden Scholars*, 163.
  16. *Ibid.*, 164.
  17. *Ibid.*, 165.
  18. Gladys Reichard to Elsie Clews Parsons, July 9, 1932, in APS-Parsons.
  19. de Laguna, "Gladys Reichard: Appreciation and Appraisal," Barnard College memorial booklet, 1955, 11, in MNA-Reichard.
  20. Reichard gives information about her field experiences, and the chronology, in two unpublished manuscripts: "Another Look at the Navajo," (29-37), and "Some Materials for Navajo Study," (29-12), MNA-Reichard.
  21. Gordon, "Among Women," 136.
  22. Reichard, *Dezba*, 73.
  23. *Ibid.*, 69.
  24. *Ibid.*, 70.
  25. *Ibid.*

26. Ibid., 73.
27. Ibid.
28. Ibid., 75.
29. Draft of essay later published in *The Atlantic* (July 13, 1949), "I Personally. . . ." in MNA-Reichard (29–18). See also Elsie Clews Parsons Correspondence Files, APS-Parsons, and Franz Boas Correspondence Files, APS-Boas.
30. See also Reichard's other writings on religion: "Human Nature as Conceived by the Navajo Indians" (1943); "Distinctive Features of Navajo Religion" (1945); "The Navajo and Christianity" (1949); *Prayer, the Compulsive Word* (1944); and *Navajo Religion* (1950).
31. Lyon, "Gladys Reichard at the Frontiers of Navajo Culture," 138.
32. Ibid., 138.
33. Ibid., 139.
34. Ibid. It is important to note, however, that Kluckhohn had a reputation of encouraging woman anthropologists other than Reichard.
35. For examples of these men's reviews of her work, see Father Berard [Haile], Review of *Social Life* (1932); Hoiijer, Review of *Prayer* (1947). For Kluckhohn's letter to Reichard regarding Bittany, see Kluckhohn to Reichard, November 12, 1943, MNA-Reichard, 29–5–9.
36. Lyon, "Gladys Reichard at the Frontiers of Navajo Culture," 139.
37. Wyman to Reichard, n.d. (maybe 1947), MNA-Reichard, 29–2–23.
38. Mead, *Anthropologist at Work*, 95; original in APS-Mead.
39. Lyon, "Gladys Reichard at the Frontiers of Navajo Culture," 139; Lyon based this conclusion on correspondence with anthropologist Fred Eggan (November 1, 1987).
40. Lamphere, "Gladys Reichard among the Navajo," in Parezo, *Hidden Scholars*, 159.
41. Alfred Kroeber to Edward Sapir, 1924, letter 355, in Golla, ed., *The Sapir-Kroeber Correspondence*, 410.
42. Ruth Bunzel would also make a valuable person to include in this community of anthropologists but her papers are not available to researchers.
43. Personal correspondence from Ruth Bunzel to Margaret Mead, 1959, quoted in Mead, "Apprenticeship under Boas," 33–35.
44. Ruth Bunzel, Interview with Jennifer Fox, 1985, Original in Wenner-Gren Foundation, New York.
45. Mead, "Apprenticeship under Boas," 34.
46. Ibid.
47. Ibid., 33.
48. Personal correspondence from Ruth Bunzel to Margaret Mead, 1959, quoted in Mead, "Apprenticeship under Boas," 34.
49. Ruth Bunzel, interviewed by Nancy Parezo, as quoted in Parezo, "Anthropology: The Welcoming Science," 24.
50. Rossiter, *Women Scientists in America*, 39.

51. Mead, "Apprenticeship under Boas," 30.
52. Gladys Reichard to Elsie Clews Parsons, August 25, 1929, APS-Parsons.
53. Gladys Reichard to Elsie Clews Parsons, March 17, 1931, APS-Parsons.
54. Gladys Reichard, "Another Look at the Navajo," unpublished manuscript, ca. 1950, MNA-Reichard.
55. Reichard, *Navajo Shepherd and Weaver*.
56. Ruth Bunzel, "Gladys A. Reichard: A Tribute," 1955, Memorial service at Barnard College; transcript in MNA-Reichard.
57. Reichard, "I, Personally," 1949, draft later published in revised form in *The Atlantic* (13 July 1949), in MNA-Reichard.
58. Kate Peck Kent, Interview with Jennifer Fox, 1985, Videotape in Wenner-Gren Foundation, New York City.
59. Leacock, "Gladys Amanda Reichard (1893-1955)," 303.
60. Underhill Interview by Joyce Herold, Tape 1, 1-5, Ruth Murray Underhill Papers, Denver Museum of Science and Nature Archives, Denver, Colorado (hereafter DMNS-Underhill).
61. Ruth Underhill interview with Joyce Griffen, November 1981, quoted in Griffen, "Ruth Underhill," 355.
62. Underhill, September 14, 1980, in Oral History Tapes, Tape 17, 11, DMNS-Underhill.
63. Underhill interview with Joyce Griffen, November 1981, quoted in Griffen, "Ruth Underhill," 356.
64. Underhill, *Papago Woman*, ix.
65. Sochen addresses women's society in Greenwich Village in *The New Woman*.
66. Underhill, *Papago Woman*, ix.
67. Several of her columns from 1919 are in DMNS-Underhill, Reference File, Box 9.
68. Underhill interview with Joyce Griffen, November 1981, quoted in Griffen, "Ruth Underhill," 356.
69. Interview with Richard Conn, Curator, Denver Art Museum, June 27, 1991.
70. In part because of her association with Ruth Benedict and Margaret Mead, who are assumed to have been lovers (see below), and in part because of her extended single status, it has been intimated that Underhill was either bisexual, lesbian, or asexual. However, all sources point to Underhill as a heterosexual, independent woman who enjoyed singlehood and felt comfortable with it. For this reason, I have included the anecdote about her relationship with Gilfallan. Interview with Richard Conn, Curator, Denver Art Museum, June 27, 1991.
71. Underhill interview with Joyce Griffen, November 1981, quoted in Griffen, "Ruth Underhill," 356.
72. See the photos in DMNS-Underhill, which she captioned "The Crawford Unwedding Trip," Box 17.

73. Underhill, *Papago Woman*, ix.
74. Hyatt, *Franz Boas, Social Activist*, xii, 49–51; also see Mead, “Apprenticeship under Boas.”
75. Interview with Ruth Underhill by Joyce Herold, May 12, 1980, DMNS-Underhill, Tape 10, 26.
76. Underhill, *Papago Woman*, x.
77. Ruth Underhill Interview with Joyce Herold, May 12, 1980, DMNS-Underhill, Tape 10, 26.
78. *Ibid.*, 26; and Underhill, *Papago Woman*, 32. Underhill’s father also gave her an annual cash gift that helped cover her expenses.
79. Denver Museum of Natural History (now Denver Museum of Nature and Science), *Ruth Murray Underhill: Friend of the Desert People*, videotape (1985).
80. Mead, *Blackberry Winter*, 113.
81. Ruth Underhill interview with Joyce Herold, DMNS-Underhill, Tape 10, 23.
82. Ruth Underhill interview with Joyce Herold, February 21, 1980, DMNS-Underhill, Tape 6, 41–42.
83. *Ibid.*, 41.
84. *Ibid.*, 44.
85. Underhill, *Papago Woman*, 34; Underhill might well have meant “bossy.”
86. Ruth Underhill interview with Joyce Herold, February 21, 1980, DMNS-Underhill, Tape 6, 43.
87. Griffen, “Ruth Underhill,” 357.
88. Kelly, “Anthropology and Anthropologists in the Indian New Deal.”
89. Griffen, “Ruth Underhill,” 358.
90. Parsons, “Review of *Singing for Power* by Ruth Underhill,” 483.
91. In the later years of her own career, Underhill prepared an extensive annotated bibliography of Parsons’s writings. See Underhill’s Elsie Clews Parsons bibliography in Ruth Murray Underhill Collection, Penrose Library Special Collections, University of Denver (hereafter DU-Underhill), Box 2, Folder 1.
92. Underhill, *Papago Woman*, 91.
93. Ruth Underhill interview with Joyce Herold, May 12, 1980, DMNS-Underhill, Tape 10, 25.
94. *Ibid.*, 24.
95. Ruth Underhill interview with Joyce Herold, February 21, 1980, DMNS-Underhill, Tape 6, 44.
96. Underhill quoted in Griffen, “Ruth Underhill,” 357.
97. Ruth Underhill interview with Joyce Herold, February 21, 1980, DMNS-Underhill, Tape 6, 42.
98. Underhill, *Papago Woman*, 3.
99. *Ibid.*, 7.

100. *Ibid.*, 13.
101. See Tinsdale's assertions of this in general terms in "Women on the Periphery of the Ivory Tower," 330–32.
102. "Papago Tribe Honors Ruth Murray Underhill," 3.
103. Papago Tribe of Arizona, *Tohono O'Odham*.
104. Underhill Correspondence file, DMNS-Underhill, Box 7.
105. Julie Pierson, "To Ruth Underhill," 1979, in DMNS-Underhill.
106. "Papago Tribe Honors Ruth Murray Underhill," 3.
107. Underhill examined this Papago narrative style in several studies of the tribe, especially in her *Singing for Power* (1938), and her *Social Organization of the Papago Indians* (1939). The style is evident as well in Saxton, *O'othham Hoho'ok A'agitha*.
108. Underhill interview by Joyce Herold, September 14, 1980, in DMNS-Underhill, Tape 17, 12.
109. *Ibid.*, 12.
110. Underhill, *Papago Woman*, 27.

## Chapter 5

1. Underhill, *Papago Woman*, 34.
2. See, for example, Parsons's unpublished manuscript, "In the Southwest—The Governor of Zuni," in APS-Parsons, Series III—Manuscripts, Box 3; and the section wherein María Chona tells the story of the making of saguaro wine and the festival following in *Papago Woman*, 17–27.
3. Rosenberg, *Beyond Separate Spheres*, 172–77.
4. Correspondence quoted in Hare, *A Woman's Quest for Science*, 90.
5. APS-Parsons, Series III—Manuscripts, "Journal of a Feminist, 1913–14."
6. Babcock, "Taking Liberties," 396.
7. Correspondence quoted in Hare, *A Woman's Quest for Science*, 139.
8. *Ibid.*, 137.
9. Parsons, *Old Fashioned Woman*, 134.
10. Reichard, *Dezba*, 87.
11. *Ibid.*, 86–87.
12. *Ibid.*, 9.
13. *Ibid.*, 4.
14. *Ibid.*, 5.
15. Underhill, *Papago Woman*.
16. *Ibid.*, 91.
17. *Ibid.*, 92.
18. *Ibid.*
19. Ruth Underhill interview with Joyce Herold, February 21, 1980, DMNS-Underhill, Tape 6, 45.
20. Underhill, *Papago Woman*, 56.
21. *Ibid.*, 59.

22. Ibid., 34.
23. Ibid.
24. Ibid., 15–16.
25. Griffen, “Ruth Underhill,” 359.
26. Underhill, *Papago Woman*, 52.
27. Ibid., 51–52. During her second marriage, Chona believed that she had almost been driven mad by the spirit of a poisoned rabbit; see *ibid.*, 79–80.
28. Ibid., 52.
29. This “operation” was not a medical procedure in the European sense. From the European perspective, the medicine man used “sleight of hand” to “suck” something out of Chona’s body. But from the Papago perspective, and from Chona’s, the medicine man really did remove the crystals that grew in Chona’s body. To refer to this as simply a “trick” would be unfairly to downplay the Papago faith in the “miracle” performed by the medicine man. Many Papago medical procedures defy explanation in non-Papago terms, just as the Freudian psychology so familiar to Europeans would defy description in Papago. Underhill did not define the nature of the operation, either.
30. Gordon, *Heroes of Their Own Lives*, 22–23.
31. Scharf, *To Work and to Wed*, 85, 107.
32. Blackwelder, *Women of the Depression*, 124–25.
33. Wandersee, *Women’s Work and Family Values*, 98–99.
34. Blackwelder, *Women of the Depression*, 125.
35. Pigeon, *Employed Women under the N.R.A. Codes*, (1935), 140–41.
36. Scharf, *To Work and to Wed*, 65–85.
37. Blackwelder, *Women of the Depression*, 125.
38. Ware, *Holding Their Own*, 27.
39. Oppenheimer, *The Female Labor Force in the United States*, 44–45, 53.
40. Ware, *Holding Their Own*, 27.
41. Chafe, *The American Woman*, 108.
42. Shallcross, *Should Married Women Work?*, 17. Sociologist Mary Anderson also commented on the economic pressures experienced by women during the Great Depression in her *Woman at Work* (1949).
43. For an overview of Depression-era film, see Bergman, *We’re in The Money*, esp. 48–61. More recent studies of Depression-era film include Roffman and Purdy, *The Hollywood Social Problem Film*.

## Chapter 6

1. Parsons, Zuñi Notebook (Journal no. 2)—1915—Assimilation, Owl, Sunlore, Lightning, Reptiles, Time Keeping, Calendars; in APS-Parsons, Series IV—Research Notes and Notebooks, Box 10 (Taos, Tesuque, Tewa, Tiwa, Zuñi).



2. For excellent compilations of the literature on cross-gender traditions in Native American cultures, see: Jacobs, "Berdache"; Callender and Kochems, "The North American Berdache"; Roscoe, "Bibliography of Berdache"; and Jacobs, Thomas, and Lang, eds., *Two-Spirit People*. It is important to remember that, as Elizabeth Spelman has noted, "the creation of 'homosexual' as a category of person as opposed to a category of action dates apparently from the late nineteenth century"; see her *Inessential Woman*, 149–50; see also Heller, Sosna, and Wellerby, eds., *Reconstructing Individualism*, 222–36. Because of this search for the individuals who make up the category rather than for the activities that define the individuals, anthropologists, like other social scientists of the period, focused almost exclusively on the identity of the man-woman, rather than the lived experiences of the man-woman.
3. Silko, "Yellow Woman and a Beauty of the Spirit," 67.
4. Roscoe, "Bibliography of Berdache," 158, 163.
5. Gutiérrez, *When Jesus Came, the Corn Mothers Went Away*.
6. Roscoe, "Bibliography of Berdache," 86 n. 2; Legg, "The Berdache and Theories of Sexual Inversion"; Allegre, "Le Berdache, est-il un modèle pour nous?"; and Williams, *The Spirit and the Flesh*, esp. 204–8.
7. Linton, *The Study of Man*, 480; Whitehead, "The Bow and the Burden Strap"; and Seward, *Sex and the Social Order*, 112–24.
8. Anderson, *A Recognition of Being*, 89. See also Lang, "Various Kinds of Two Spirit People," in Jacobs, Thomas, and Lang, *Two-Spirit People*, 103.
9. Parsons, "Journal of a Feminist, 1913–14," in APS-Parsons, Series III—Manuscripts.
10. Ruth Underhill interview with Joyce Griffen, quoted in Griffen, "Ruth Underhill," 355.
11. It is important to note that María Chona uniformly referred to Shining Evening as "she," and Margaret Lewis consistently used "she" for man-women and "he" for women-men. Ruth Underhill, in contrast to Chona, used "he" to refer to Shining Evening, while Parsons, in contrast to Lewis, used "he" for morphologically male man-women and "she" for morphologically female women-men. Both Benedict and Reichard used "he" for man-women and "she" for women-men, but because of the way their field notes have (not) been preserved, it is not possible to determine the pronouns that their informants used for each. In contrast, Matilda Coxe Stevenson adopted We'Wha's use of "she" to refer to herself. I choose to use María Chona's, Margaret Lewis's, and We'Wha's determination of the proper pronoun, and thus use "she" when referring to Shining Evening, We'Wha, and other morphologically male man-women, and "he" when referring to morphologically female women-men, like the Zuñi Nancy.
12. The importance of motherhood as differentiating female gender among First Wave feminists is a significant issue. For example, when Heterodoxy member and feminist writer Charlotte Perkins Gilman

wrote her feminist utopian novel, *Herland* (1915), she argued that what fundamentally linked women was "Motherhood." Creating an all-female world governed by feminist ideals, Gilman argued that women did not exist merely as the opposites of males; in an all-female world, such oppositions could no longer define female gender. Instead, Gilman illustrated, Motherhood—which the female inhabitants of Herland had learned to achieve without the sexual or biological contributions of males—was the one unique and defining characteristic of female gender. Parsons participated in Heterodoxy with Gilman, and reflected similar convictions about motherhood as the determinant of female gender in her ethnographic writing.

13. Rosenberg, *Beyond Separate Spheres*, 39.
14. Davis, *Factors in the Sex Life of Twenty-Two Hundred Women* (1920), and Davis, "Why They Failed to Marry" (1928); and Dickinson, *A Thousand Marriages* (1931). See also Rosenberg, *Beyond Separate Spheres*, 200–203.
15. See Wile, ed., *The Social Life of the Unmarried Adult* (1934); and Sahli, "Smashing" (1979). For an extended discussion of public cultural reactions to homosexuality, see Chauncey, *Gay New York*, especially 99–102, where he discusses public reactions and tensions over public displays of gay identity in the late 1910s and early 1920s. Chauncey provides as well some of the dynamics of change that took place in public acceptance of openly homosexual behaviors, arguing that while there was some openness in the middle to late 1920s, with the coming of the Great Depression there was a general "exclusion of homosexuality from the public sphere in the 1930s," 331–54, esp. 331.
16. Parsons, *The Family* (1906); and *The Old-Fashioned Woman* (1913).
17. Elsie (Lissa) Parsons Kennedy, interviewed November 11, 1962, by Louis M. Starr and Allan Nevins, at Lenox, Massachusetts—original at Columbia University Oral History Project, 16.
18. See Parsons's notes from an interview with Mrs. Chavez of Isleta Pueblo, about man-women, in APS-Parsons, Box 10 (Isleta Tales 1925–1927; 8 Notebooks) Notebook Four: "I Isleta Dec. 20 1927."
19. Interview with Margaret Lewis in Zuñi Notebook (Journal no. 1)—August 15, 16, 1915, in APS-Parsons, Series IV—Research Notes and Notebooks, Box 10 (Taos, Tesuque, Tewa, Tiwa, Zuñi).
20. See, for example, her interviews with informants George (Hano) and Lucy (Hopi), in APS-Parsons, Series IV—Research Notes and Notebooks, Box 2 (Hano, Havasupai, Hopi); Hopi Notebook no. 4—June 10, 1920—General, Sticks, Calendar, Morning Star.
21. For the discussion of syphilis, see her interview with the Taos informant L. Mantano, Arroyo Seco, NM, in 1931–1932, in APS-Parsons, Box 8 (Taos Notebooks); for discussion of conjugal continence, see her interview with the Zuñi informant Niña, in Zuñi Notebook (Journal no. 1)—August 15,

- 16, 1915, in APS-Parsons, Series IV—Research Notes and Notebooks, Box 10 (Taos, Tesuque, Tewa, Tiwa, Zuñi); for discussion of sexual crimes, see her interview with Margaret Lewis, in Zuñi Notebook (Journal no. 1)—August 15, 16, 1915, in APS-Parsons, Series IV—Research Notes and Notebooks, Box 10 (Taos, Tesuque, Tewa, Tiwa, Zuñi).
22. Benedict, *Patterns of Culture*, 262.
  23. *Ibid.*, 264.
  24. *Ibid.*, 265.
  25. *Ibid.*, 263.
  26. McFeely, *Zuni and the American Imagination*, 63.
  27. Benedict, *Patterns of Culture*, 263.
  28. *Ibid.*, 264.
  29. Bateson, *With a Daughter's Eye*, 115–27. Caffrey, in *Ruth Benedict*, for example, posits a sexual relationship between the two women and cites as her sources literary analyses of the women's letters and poetry and Bateson's argument.
  30. Smith-Rosenberg, *Disorderly Conduct*. See also Rupp, "Imagine My Surprise"; and Faderman, *Surpassing the Love of Men*.
  31. See her correspondence with Sapir in Vassar-Benedict, and as published in Mead, *Anthropologist at Work*.
  32. Benedict, "Crime against Nature," typewritten manuscript of a speech given before the New York Academy of Medicine, April 11, 1939, in Vassar-Benedict. In this speech, Benedict fell into line with Dickinson's 1931 book, *A Thousand Marriages*.
  33. Reichard, *Social Life of the Navajo Indians*, 150.
  34. Reichard, *Navaho Religion*, 141.
  35. *Ibid.*, 140.
  36. *Ibid.*, 141.
  37. For Reichard's discussion of Lassos-a-Warrior, see the chapter "Authority," in *Dezba*, 108–18.
  38. In fact, the word in Navajo commonly used to denote a man-woman, *nadle*, means "crosses over."
  39. Reichard, *Dezba*, 114.
  40. *Ibid.*, 115.
  41. *Ibid.*, 116–17.
  42. *Ibid.*, 117.
  43. Ellis, *Studies in the Psychology of Sex*; von Krafft-Ebing's *Psychopathia Sexualis*. See her notes on both in DMNS-Underhill.
  44. Underhill, *Papago Woman*, 68.
  45. For Chona's discussion of Shining Evening, see *ibid.*, 64, 67–68, 74.
  46. Parsons, Zuñi Notebook (Journal no. 1)—August 15, 16, 1915, Informants Niña, Margaret Lewis, 32, in APS-Parsons, Series IV—Research Notes and Notebooks, Box 10 (Taos, Tesuque, Tewa, Tiwa, Zuni).
  47. Parsons, Zuñi Notebook (Topics no. 1)—1915—Four, Pregnancy and

- Childrearing, Sex Distinction, in APS-Parsons, Series IV—Research Notes and Notebooks, Box 10 (Taos, Tesuque, Tewa, Tiwa, Zuni).
48. Reichard, *Social Life of the Navajo Indians*, 150.
  49. Tschopik, "Navaho Pottery Making," 45.
  50. Hill, "The Status of the Hermaphrodite and Transvestite in Navaho Culture," 273.
  51. Roscoe, *Zuni Man-Woman*, 126.
  52. Frank H. Cushing, "Nominal and Numerical Census of the Gentes of the Ashiwi or Zuni Indians" (1881), MS 3915, National Anthropological Archives, Smithsonian Institution, Washington, DC.
  53. See, for example, photos of We'Wha weaving with waist loom and blanket looms, both at Zuñi and Washington, DC, in National Anthropological Archives, Smithsonian Institution.
  54. *National Tribune* (Washington, DC), May 20, 1886.
  55. Parsons, Zuñi Notebook no. 1—June 20, 1920—General Notes, Flora Zuni Informant, in APS-Parsons, Series IV—Research Notes and Notebooks, Box 12 (Zuñi).
  56. Underhill, *Papago Woman*, 64, 67.
  57. *Ibid.*, 64.
  58. *Ibid.*, 64, 68.
  59. *Ibid.*, 68.
  60. Parsons, Notebook Four: "I Isleta Dec. 20 1927," Informant: Mrs. Chavez, in APS-Parsons, Series IV—Research Notes and Notebooks, Box 10 (Isleta Tales 1925–1927; 8 Notebooks).
  61. Parsons, "The Zuñi La'mana" (1916), 27.
  62. The name appears in Zuñi Notebook (Journal no. 2)—1915—Assimilation, Owl, Sunlore, Lightning, Reptiles, Time Keeping, Calendars, APS-Parsons, Series IV—Research Notes and Notebooks, Box 10 (Taos, Tesuque, Tewa, Tiwa, Zuni); however, the story appears in Zuñi Notebook (Journal no. 1)—August 15, 16, 1915, Informant Margaret Lewis, 46, APS-Parsons, Series IV—Research Notes and Notebooks, Box 10 (Taos, Tesuque, Tewa, Tiwa, Zuni).
  63. Underhill, *Papago Woman*, 74.
  64. Hill, *Navaho Humor*, 15.
  65. Peter Blaine, Sr., interviewed by Michael S. Adams, 1981, Oral History Collection, Arizona History Society Library Archives, Peter Blaine, Sr., 1902–, AV0362 (1980), 97.
  66. Underhill, *The Autobiography of a Papago Woman* (1936), 6.
  67. Peter Blaine, Sr., Arizona History Society Library Archives, 177.
  68. Underhill, *Social Organization of the Papago Indians*, 186.
  69. Parsons, Zuñi Notebook (Journal no. 2)—1915—Assimilation, Owl, Sunlore, Lightning, Reptiles, Time Keeping, Calendars, in APS-Parsons, Series IV—Research Notes and Notebooks, Box 10 (Taos, Tesuque, Tewa, Tiwa, Zuni).

70. Stevenson, "The Zuñi Indians" (1904), 37.
71. The argument about the equivalence or lack thereof between man-women and women-men is a complex one. See for example, Whitehead, in "The Bow and the Burden Strap," who argues that there is no female equivalent to the morphologically male man-woman (90–91). Blackwood, however, argues for the inclusion of female women-men in the category on equal footing; see her "Sexuality and Gender in Certain North American Tribes." In fact, most references to female "cross-gender" activity within Southwestern tribes appear in studies of the Apache; this may reflect anthropologists' conflation of Apache life patterns with those of the Plains tribes known for the inclusion of female women-men. See, for example: Gifford, "Culture Element Distributions 12: Apache-Pueblo," (1940), 66, 163; Opler, "Anthropological and Cross-Cultural Aspects of Homosexuality," (1965), 111; Opler, *Culture and Social Psychiatry* (1967), 252–53; and Opler, *An Apache Life-Way*, 79–80, 415–16.
72. Parsons, Zuñi Notebook (Journal no. 1)—August 15, 16, 1915, Informants Niña, Margaret Lewis, in APS-Parsons, Series IV—Research Notes and Notebooks, Box 10 (Taos, Tesuque, Tewa, Tiwa, Zuni).
73. Parsons, *Notes on Zuñi* (1917), 246.
74. Parsons, "The Zuñi La'mana," 23.
75. *Ibid.*, 27.
76. Reichard, *Navaho Religion*, 141.
77. For Reichard's discussion of Lassos-a-Warrior, see the chapter "Authority," in *Dezba*, 108–18.
78. *Ibid.*, 108.
79. *Ibid.*, 112.
80. *Ibid.*, 116–19; also, see the more detailed discussion of this relationship between gender and cultural resistance in Chapter 5 of this study.
81. Parsons, "Zuñi La'mana," 28.
82. Spelman, *Inessential Woman*, 149–50; see also Heller, Sosna, and Wellerby, eds., *Reconstructing Individualism*, 222–36.
83. Underhill, *Social Organization of the Papago Indians*, 186.
84. Parsons, "Zuñi La'mana," 26.
85. Benedict, *Patterns of Culture*, 264.
86. Reichard, *Navaho Religion*, 140.
87. Parsons, "Zuñi La'mana," 26. In Zuni tradition, the creation stories include the creation of a man-woman.
88. Underhill, *Social Organization of the Papago Indians*, 187.
89. Faithorn, "Gender Bias and Sex Bias," 276.
90. *Ibid.*
91. Stevenson, "Zuñi Indians," 119.
92. Parsons, "In the Southwest—Pedro Baca and I go Visiting," in APS-Parsons, Series III—Manuscripts, Box 3.

## Chapter 7

1. Frances Densmore's fieldwork among the Tohono O'Odham resulted in her publications "Poems from Desert Indians" (1926); *The American Indians and Their Music* (1926); and *Papago Music* (1929). Among those who were in the field working on the Tohono O'Odham at the same time as Underhill were several other anthropology graduate students, including Helen Lenore Moore of the University of Southern California, and Stanley Harding Boggs, Marie Louise Gunst, and John F. Wilkinson of the University of Arizona. See Moore, "The Papago Indians of Arizona and Sonora" (1925); Boggs, "A Survey of the Papago People" (1936); Gunst, "Ceremonials of the Papago and Pima Indians" (1930); and Wilkinson, "The Papago Indians and Their Education" (1935). The folklorist J. Alden Mason was also working among the Tohono O'Odham before and while Underhill was, and published "The Papago Migration Legend" (1921) and "The Papago Harvest Festival" (1920), as well as his later *The Language of the Papago of Arizona* (1950) based on research conducted before and during Underhill's tenure on the reservation.
2. Parsons, "Nativity Myth at Laguna and Zuñi", 258 n. 4.
3. Parsons, "A Few Zuni Death Beliefs and Practices", 250.
4. Parsons, "Pueblo-Indian Folk-tales, Probably of Spanish Provenance".
5. Togorvnick, *Gone Primitive*, 3.
6. *Ibid.*, 232-34.
7. James, *What the White Race May Learn from the Indian* (1908), 39.
8. *Ibid.*, 41.
9. Benedict, *Patterns of Culture*, 18.
10. Ruth Benedict to Margaret Mead, from Cochiti, September 5, 1925, in Mead, *Anthropologist at Work*, 300.
11. Stevenson, "A Chapter of Zuni Mythology" (1894), 318-19. Hill and Lange argue that clay is the flesh of a woman, in their *Ethnography of Santa Clara Pueblo New Mexico* (1982), 84, as does Cushing in *Zuni Breadstuff* (1920), 310-16; and Parsons argues that clay is female for those at Zuni, Cochiti, Laguna, and Isleta in *Pueblo Indian Religion*, vol. 1 (1939), table 2. Parezo, Hays, and Slivac provide several Native American women potters' perspectives on the femaleness of clay in "The Mind's Road," in *The Desert Is No Lady* (1987), 170-71.
12. Bunzel, *The Pueblo Potter* (1929), 72; also, Hardin quotes Matilda Coxé Stevenson's field notes from the National Anthropological Archives confirming the taboo against women painting prayer sticks in her *Gifts of Mother Earth*, 32.
13. Bunzel, *Pueblo Potter*, 94-96.
14. See Parsons, *Pueblo Indian Religion*.
15. See especially Parsons's *Social Organization of the Tewa in New Mexico*.

16. Parsons, "Wives and Birth Control" (1916), 187.
17. See especially Parsons's *The Social Organization of the Tewa in New Mexico* (1929).
18. See, for example, Parsons, (Taos Notebook), "1931-1932 Taos I," Informant L. Mantano, Arroyo Seco, NM, December 12, 1931, in APS-Parsons, Box 8.
19. Parsons, Notebook Three: "Isleta November 28 1926," Informant Maria Chiwiwi, in APS-Parsons, Box 10 (Isleta Tales 1925-1927; 8 Notebooks).
20. Parsons, "Hopi Mothers and Children" (1921); "Mothers and Children at Laguna" (1919); and "Mothers and Children at Zuñi" (1919).
21. Parsons, Notebook Four: "I Isleta Dec. 20 1927," Informant The Sun Takes a Head, in APS-Parsons, Box 10, (Isleta Tales 1925-1927; 8 Notebooks).
22. See May's study of moral reform in the Hollywood film industry during and following the Progressive Era, *Screening Out the Past*.
23. Parsons, Notebook Two: "Taos 1924 II," Informant Lorenzo Martinez, November 26, 1924, in APS-Parsons, Box 11 (Taos Notebooks 1922-1933; 8 Notebooks).
24. Parsons, Hopi Notebook no. 3, June 9, 1920, "Ethnobotany," Informants George and Lucy, in APS-Parsons, Series IV—Research Notes and Notebooks, Box 2 (Hano, Havasupai, Hopi).
25. Ibid. This would have been the Western Red Cedar (*Thuja plicata*).
26. Parsons, Notebook Six: "Taos Nov. 1926 I," Informant Lucinda Martinez, November 8, 1926, in APS-Parsons, Box 11 (Taos Notebooks 1922-1933; 8 Notebooks).
27. Parsons, Notebook Six: "Taos Nov. 1926 I," Informant Lucinda Martinez, November 3, 1926, in APS-Parsons, Box 11 (Taos Notebooks 1922-1933; 8 Notebooks).
28. The Taos informant who told Parsons of *papiengumv* was L. Mantano, Arroyo Seco, NM, during her fieldwork at Taos in 1931 or 1932. See APS-Parsons, Box 8 (Taos Notebook). *Papiengumv* seems to have been White Oak (*Quercus velutina*), a species of beech found in northern New Mexico. The leaves and bark of White Oak have a high tannin content, and hence one of its common names is "Tanner's Bark." Tannin is useful for stopping hemorrhaging, which explains its application for excessive menstrual flow. It also has a strong astringent quality, which means that it dehydrates cells. As such, it could have been an abortifacient in that it could dehydrate or even cut off the placental flow to an early-term embryo or, entering the embryo, cause the cells to malfunction to such a degree that spontaneous abortion took place.
29. Parsons, Notebook Six: "Taos Nov. 1926 I," Informant Lucinda Martinez, interviewed November 4, 1926, at Taos Pueblo, in APS-Parsons, Box 11 (Taos Notebooks 1922-1933).

30. Reichard, *Dezba*, xv.
31. *Ibid.*, xviii.
32. *Ibid.*, 119.
33. *Ibid.*, 121.
34. For an overview of the nature of these meetings, see Meinig, *Southwest*, and Spicer, *Cycles of Conquest*.
35. Underhill, *Papago Woman*, 8.
36. *Ibid.*, 7.
37. For example, Chona used the term “Vassindone” to indicate Washington (Underhill, *Papago Woman*, 3), and she did speak some English, although Chona commonly gave her testimony in either Spanish or Tohono O’Odham. Underhill employed two Tohono O’Odham translators, first Lapai, María Chona’s nephew, and later Ella Lopez Antone, a teenaged student home on summer vacation from the Phoenix Indian School. More information regarding Underhill and Ella Lopez Antone is in Bataille and Sands, *American Indian Women*, 55–56.
38. Underhill, *Papago Woman*, 39.
39. Ruth Underhill interview with Joyce Herold, February 21, 1980, DMNS-Underhill, Tape 6, 45; also, Underhill, *Papago Woman*, 76.
40. Underhill, *Papago Woman*, 77.
41. *Ibid.*, 78.
42. *Ibid.*, 77.
43. *Ibid.*, 78.
44. *Ibid.*, 79.
45. *Ibid.*
46. *Ibid.*, p. 78.
47. Ruth Underhill interview with Joyce Herold, February 21, 1980, DMNS-Underhill, Tape 6, 45.
48. Underhill, *Papago Woman*, 78.
49. *Ibid.*, 67.
50. *Ibid.*, 73.
51. Ruth Underhill interview with Joyce Herold, February 21, 1980, DMNS-Underhill, Tape 6, 41–42.
52. *Ibid.*, 41.
53. *Ibid.*, 44.
54. Underhill, *Papago Woman*, 3; the account of the meeting between Underhill and Chona appears in Underhill’s autobiographical section in *Papago Woman*, 3–7.

## Chapter 8

1. Goldfrank, *Notes on an Undirected Life*, 42.
2. Rosenberg, *Beyond Separate Spheres*, esp. 54–83.
3. Warren, *Gender Issues in Field Research*, 9.



4. Ruth Bunzel, Interview (with Jennifer Fox) for Daughters of the Desert Oral History Project (July 2, 1985, New York City, NY); Videotape Originals in Wenner-Gren Foundation for Anthropological Research Archives, New York City.
5. Florence Hawley Ellis, Interview (with Jennifer Fox) for Daughters of the Desert Oral History Project (August 4–5, 1985, Santa Fe, New Mexico); Videotape Originals in Wenner-Gren Foundation for Anthropological Research Archives, New York City.
6. Underhill, *Papago Woman*, 92.
7. Matthew Stirling, quoted in "Women Students among Indians" (1931), 184–85.
8. Letter to Ruth Benedict, quoted in Mead, *Anthropologist at Work*, 410.

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# Index

- Aberle, David, 37  
Aberle, Sophie D., 62, 110  
abortifacients, 174  
acculturation, 35  
Albers, Patricia, 8  
ałoi (Weaver) (Navajo), 153  
American Anthropological Association, 45, 55  
Àngulo, Jaime de, 85  
anthropology. *See also*  
ethnographers; American School, 25–28; biological determinism, 127–28, 143–44; configurational studies, 32–33; cultural evolution, 24–29, 33, 52–53; cultural relativism, 21–24, 143–44, 165; culture, 31–37, 79–80; essentialist arguments, 127–28; European School, 25–28; field work, 24, 28–30; folklore work, 45, 58; Functionalists, 26, 35–36; history of, 7–9; idiographic methodology, 31; and modernism, 1–2; participant-observer method, 99–102, 109; and psychology, 31–33, 37; in the Southwest, 21, 38–39; women professionals, 14–15, 41–48, 72, 100–101, 183  
Antonia, Maria (Navajo), 94, 101, 120–21  
Apache Indians, 36, 78, 177, 185  
Apollonian culture, 79–80  
assimilation, 94–96, 175–76  
Babcock, Barbara A., 14–15  
Barnard University, Department of Social Sciences, 46, 51, 88–89  
basic personality, 32  
Basso, Keith H., 8  
Bataille, Gretchen, 11  
Beals, Ralph, 34, 68  
Benedict, Ruth Fulton: her biographical work, 75–76; and Boas, 107; her career path, 45–46, 72–77, 183; her depression, 74, 81–82; her family, 73; her feminism, 71, 75, 77, 84; her field work, 78, 84; and grants, 90; her informants, 82–84; and man-woman role, 145–49; her marriage, 74–75, 77; and Mead, 80–81, 148–49; and Parsons, 69–70, 77; and primitivism, 167; her publications, 36, 78–79; and Reichard, 88–89; and Sapir, 148; her sexuality, 5–6, 148–49; her students, 85–86; and Underhill, 112

- Bennett, Wendell C., 38  
 berdache, 139, 159. *See also*  
   man-woman  
 Bieder, Robert E., 8  
 biological determinism, 127–28,  
   143–44  
 birth control, 53, 168–75  
 Blue Eyes Manuscript (Goddard),  
   93–94  
 Boas, Franz: and Benedict, 107; and  
   cultural relativism, 24–29; his  
   education, 26–27; and field work,  
   24, 28–30; and functionalist  
   theory, 35–36; and psychology,  
   31–33, 37; and Reichard, 87–89,  
   98; his students, 29, 33, 35, 41–43,  
   47, 185  
 Bunzel, Ruth, 42, 46, 99–101  
 Bureau of Indian Affairs, 47, 91,  
   109–10  
 Caddo Indians, 57  
 Cahita Indians, 58  
 childrearing practices, 38  
 Chiwiwi, Maria (Isleta Pueblo), 171  
 Chona, María (Papago): internal  
   contradictions, 7, 126–32; and  
   male authority, 111–12; and  
   man-woman role, 155–57;  
   and primitivism, 176–81; as  
   protofeminist, 120–21, 131–32;  
   and Shining Evening, 152–54;  
   and Underhill, 107–9, 114–18  
 Clews, Henry, 49  
 Cochiti Pueblo, 78, 82, 83  
 Cocopa Indians, 33  
 Collier, John, 110–11  
 Columbia University Anthropology  
   Department: and Benedict, 72,  
   86, 88–89; and Boas, 22, 23–24;  
   graduate program, 29; its female  
   students, 39–40, 45–46; and  
   Parsons, 51; and Underhill, 106  
 configurational studies, 32–33  
 conquest in the West: and Chona  
   (Papago), 177–81; and Dezba  
   (Navajo), 94–96, 175–76; and  
   man-woman role, 150–52, 158;  
   and primitivism, 163–64, 176–77;  
   and sexuality, 17–19  
 Cott, Nancy, 3  
 Crawford, Charles, 105  
 Crawford, James, 62  
 cross-gender traditions. *See* man-  
   woman  
 Crow Wing (Hopi-Tewa), 56  
 cultural evolution, 24–29, 33, 52–53  
 cultural relativism, 21–24, 143–44,  
   165  
 culture, 31–37, 79–80  
 Davis, Katherine Bement, 144  
 Debo, Angie, 41–42  
 Dennis, Wayne, 38  
 Densmore, Frances, 47  
 Devereaux, George, 38  
*Dezba, Woman of the Desert*  
   (Reichard), 94–96, 150–52,  
   175–76. *See also* Antonia, Maria  
   (Navajo)  
 Dickinson, Robert Latou, 144  
 Diffusionists, 26, 33  
 Dionysian culture, 79–80  
 divorce, 77, 105, 111–12, 128  
 Dobu Islanders, 79  
 dreams, 37–38  
 Eggan, Dorothy, 37  
 Eggan, Fred, 36  
 Ellis, Havelock, 152  
 environment, 35, 168–75  
 essentialist arguments, 127–28  
 ethnographers. *See also*  
   anthropology; women;  
   autobiographical research,  
   13–14; competition, 15;  
   configurational studies, 32–33;  
   and essentialist arguments, 127–  
   28; as feminists, 40–43, 183–84;  
   and man-woman role, 5–6, 139,  
   160–62; and patriarchy, 2–3, 4,  
   9; and psychology, 31–33, 37;  
   published omissions, 7, 10–11,  
   117–18, 157–59, 172–75; and  
   race, 29–30; salvage ethnography,  
   22, 28, 42–43, 165

- Evening, Shining (Papago), 152-57
- executive women, 4, 108-9, 120, 132-37. *See also* gender identity
- The Family* (Parsons), 51-53
- Fauset, Arthur Huff, 58
- feminism. *See also* women; and Benedict, 71, 75, 77, 84; and biological determinism, 143-44; changes in, 40-43, 183-84; defining, 2-3; and essentialist arguments, 127-28; and gender identity, 5, 122-23, 141-42, 184-86; Heterodoxy, 51; and Parsons, 122-23; and Reichard, 123-26; and Underhill, 105, 111-12, 126-32
- folklorists, 45, 58
- Forde, C. Daryll, 33
- Freeman, Derek, 8
- Frobenius, Leo, 26
- Fulton, Bertrice Shattuck, 73
- Fulton, Frederick, 73
- Functionalists, 26, 35-36
- gender identity. *See also* man-woman; and biological determinism, 127-28, 143-44; executive women, 4, 108-9, 120, 132-37; and feminism, 5, 122-23, 141-42, 184-86; fluidity of, 140-41; gender roles vs. experience, 132; and Navajo Indians, 123-26; and Papago Indians, 126-32; and power, 111-12, 126-27, 134-36; and production/labor, 142, 153-55; and resources, 168-70; social construction of, 16, 135-37
- Gifford, Edward W., 33
- Goddard, Pliny Earle, 91, 93-94
- Goldenweiser, Alexander, 76-77
- Goldfrank, Esther, 37
- Great Depression, 132-34
- Gutiérrez, Ramón, 19
- Habbell, Roman, 94
- Haerberlin, H. K., 37
- Haile, Father Bernard, 92-93, 97
- Havasupai Indians, 33
- hermaphrodite, 158, 159. *See also* man-woman
- Heterodoxy, 51
- Hinsley, Curtis M., 8
- Hogan School, 109-10
- Hoijer, Henry, 97
- homosexuality, 144-45. *See also* man-woman
- Hopi Indians, 36, 37, 56-57, 173
- Horney, Karen, 149
- idiographic methodology, 31
- informants, Native American. *See also* anthropology; Chona, María (Papago); approaching, 58-65; and Benedict, 82-84; characteristics of, 119-20; Hopi, 173; Hopi-Tewa, 56; internal contradictions, 7, 10-11, 126-32, 141, 186-87; Isleta Pueblo, 171; and man-woman role, 146-47, 149-50, 154; published omissions, 7, 10-11, 117-18, 157-59, 172-75; translations, 161-62; types of writings, 6-7; and Underhill, 108, 112-15, 160; Zuñi, 94, 96, 120-21, 149-50, 153, 155-59
- inversion, 152, 159. *See also* man-woman
- Isleta Pueblo, 171, 172
- James, George Wharton, 166
- Jemez Pueblo, 57, 61
- Kardiner, Abram, 32
- katsotse, 158
- kinship, 36
- Kiowa Indians, 57
- Klah, Hostiin (Navajo), 94, 149-50
- Kluckhohn, Clyde, 36, 38, 92-93, 97
- Kóthlana, 158
- Krafft-Ebing, Richard von, 152
- Kroeber, Alfred L.: his career path, 45; on Parsons, 41, 56, 123; his students, 33; his studies, 34, 36
- Kwakiutl culture, 79

- Laguna Pueblo, 56  
 Lamphere, Louise, 14-15  
 Lassos-a-Warrior, 150-52, 158  
 Leighton, Dorothea, 36, 37  
 Lemons, J. Stanley, 3  
 lesbianism, 149. *See also* man-  
 woman  
 Lewis, Margaret (Zuñi), 120-21, 153,  
 155-59  
 Lewis, Oscar, 8  
 Linton, Ralph, 32, 46  
 Lowie, Robert, 36, 45  
 Lucero, Gov. Guadalupe (Taos  
 Pueblo), 63-64  
 Lucy (Hopi), 173  
 Luhan, Mabel Dodge, 58, 61, 62-64  
 Luhan, Tony (Taos Pueblo), 61,  
 62-64  
 MacGregor, Alexander, 57  
 Malinowski, Bronislaw, 26, 32, 165  
 man-woman. *See also* gender  
 identity; and Benedict, 145-49;  
 description, 4-5, 139-41, 162; and  
 feminist ethnographers, 5-6, 139,  
 160-62; and informants, 146-47,  
 149-50, 154; and Parsons, 145,  
 157-58, 159-60; and production/  
 labor, 142, 153-55; and Reichard,  
 149-52, 158; and sexuality,  
 5-6, 16-19, 144-45, 149; and  
 Underhill, 152-54, 159-60  
 market economy, 167-68, 170-74,  
 177-81  
 marriage: and Benedict, 74-75, 77;  
 and Chona (Papago), 178-79;  
 and the Great Depression,  
 133-34; and Parsons, 50-51; and  
 Underhill, 105  
 Martinez, Lucinda (Taos Pueblo),  
 60-61, 174  
 matriarchy, 79-80. *See also*  
 patriarchy  
 McBain, Howard Lee, 72  
 Mead, Margaret: on anthropology,  
 42; and Benedict, 80-81, 148-49;  
 on Boas, 30-31; on Bunzel, 99-  
 100; and psychology, 31  
 Medicine, Beatrice, 8  
 Mekeel, Scudder, 110  
 Mescalero Apache, 78  
 Miguelito (Navajo), 94, 96  
 Mirabal, Tony (Taos Pueblo), 62-64  
 modernism, 1-2, 21-24, 143-44,  
 181-82. *See also* primitivism  
 Morgan, Lewis Henry, 25-28  
 nadle, 152, 154, 158. *See also* man-  
 woman  
 nadle tco (Ugly Berdache) (Navajo),  
 153  
 Nancy (Zuñi), 158  
 National Research Council  
 fellowships, 90  
 Navajo Indians: and gender identity,  
 123-26; informants, 94, 96,  
 101, 120-21, 149-50; and man-  
 woman role, 149-52, 158;  
 and primitivism, 175-76; and  
 Reichard, 89-90, 92-94; studies  
 of, 36-38  
 New School for Social Research,  
 53-54, 55  
 nicknames, 155-56  
 Niña (Zuñi), 153  
 nomothetic methodology, 31  
 Opler, Morris, 36  
 Pai Indians, 38  
 Papago (Tohono O'odham) Indians.  
*See also* Chona, María (Papago);  
 and the Bureau of Indian Affairs,  
 110; and gender identity, 126-32;  
 and man-woman role, 152-56;  
 and primitivism, 177-81; studies  
 of, 36, 38; and Underhill, 116  
*Papago Woman* (Underhill), 107-9  
 Parsons, Elsie Clews: and Benedict,  
 69-70; and Bunzel, 100; her  
 career path, 44-45, 55, 68-70,  
 183; her education, 49-50; and  
 ethnology, 54; her family, 49,  
 65-67; her feminism, 122-23;  
 her field work, 55-58, 65-67,  
 164; and functionalist theory,

- 36; and gender identity, 5; and individualism, 52; Kroeber's criticism of, 41; and man-woman role, 145, 157-60; her marriage, 50-51; and Native American informants, 58-65; her personality, 68; and primitivism, 164, 168-75; on psychology, 69-70; her publications, 33, 51-53, 56-58; and Reichard, 91-92, 101; her students, 55-56; and Underhill, 112
- participant-observer method, 99-102, 109
- patriarchy. *See also* feminism; and Benedict, 79-80; and birth control, 168-75; and cultural evolution, 27; and feminist ethnographers, 2-3, 4, 9; matriarchy, 79-80; and power, 111-12, 126-27, 134-36; and Reichard, 94-96; and Underhill, 112
- Patterns of Culture* (Benedict), 78-79
- personality, 31-33, 79-80
- Peyote Boys (Taos Pueblo), 62-64
- Pima Indians, 57, 78
- political structures, 107
- pregnancy/partum taboos, 171-75
- primitivism: and authenticity, 164-65, 176; and Benedict, 167; and civilization, 9-10, 52-53, 167; and executive women, 122, 137; and James, 166; and nature, 168-75; and Parsons, 164, 168-75; and Reichard, 175-76; and sexuality, 144-45; and Underhill, 108, 114-15, 176-81
- psychology, 31-33, 37
- Pueblo Indians. *See also* Zuñi Pueblo; and Benedict, 79; Cochiti Pueblo, 78, 82, 83; Isleta Pueblo, 171, 172; Jemez Pueblo, 57, 61; Laguna Pueblo, 56; San Felipe Pueblo, 59-60; studies of, 33, 35, 36; Taos Pueblo, 58, 60-64, 174; Tewa Pueblos, 57
- race, 24-29, 84-85
- Radcliffe-Brown, A. R., 26, 35-36
- Red Point (Navajo). *See* Miguelito (Navajo)
- Reichard, Gladys: and Benedict, 88-89; her career path, 46-47, 183; her family and education, 87; her feminism, 123-26; her field work, 91, 98-99, 164; and Goddard, 91, 93-94; and grants, 89-91; Hogan School, 109-10; and independence, 6; and man-woman role, 149-52, 158; and Navajo weaving, 101-2; and Parsons, 101; and primitivism, 175-76; her publications, 36, 94-97; her students, 102-3; and Underhill, 109
- Reichard, Minerva Ann (Jordan), 87
- Reichard, Noah W., 87
- religion, 36-37
- Rivers, W. H. R., 26
- Roscoe, Will, 18-19
- Rosenzweig, Janine, 38
- salvage ethnography, 22, 28, 42-43, 165
- Sands, Kathleen Mullen, 11
- San Felipe Pueblo, 59-60
- Sanger, Margaret, 53
- San Ildefonso Pueblo, 57
- San Juan Pueblo, 57
- Santa Clara Pueblo, 57
- Sapir, Edward, 31, 88-89, 97, 98, 148
- Schmerler, Henrietta, 185-86
- scientific racism, 25-28
- sexuality, 5-6, 16-19, 144-45, 149.  
*See also* gender identity; man-woman
- Simmons, Leo, 37
- skirt string, 156
- social organization, 107
- Southwest: attraction to  
anthropologists, 21, 38-39; and conquest, 17-19, 94-96, 150-52, 175-81; geographical definition, 34; its cultural diversity, 176-77; and primitivism, 163



- Southwest Society, 55, 80  
 Spicer, Edward, 36, 38  
 Spier, Leslie, 34  
 Stevenson, Matilda Coxe, 44, 61, 145, 161, 168  
 Steward, Julian, 35  
 Strong, William D., 35  
 suffragism, 40, 52  
 The Sun Takes a Head (Isleta Pueblo), 172  
 Swanton, John, 26–27
- Taos Pueblo, 58, 60–64, 174  
 Tarahumara Indians, 38  
 testimonies, Native American, 7, 10–11, 126–32, 141. *See also* informants, Native American  
 Tewa Pueblos, 57  
 Titiev, Mischa, 36  
 Tłá'h (Navajo), 149–50  
 Togorvnick, Marianna, 165  
 Tohono O'Odham (Papago Indians). *See also* Chona, María (Papago); and the Bureau of Indian Affairs, 110; and gender identity, 126–32; and man-woman role, 152–56, 160; and primitivism, 177–81; studies of, 36, 38; and Underhill, 116  
 transvestitism, 160–62. *See also* man-woman  
 Tsaladize (Zuñi), 155–56  
 Tumaka, Nick (Zuñi Pueblo), 82–84  
 two-spirits, 139. *See also* man-woman  
 Tylor, Edward B., 25–28
- Underhill, Abram Sutton, 103  
 Underhill, Anna Murray, 103  
 Underhill, Ruth: and Benedict, 112; her career path, 47, 103, 104–5, 109–11, 183; her family, 103–4; her feminism, 105, 111–12, 126–32; her field work, 106, 164; and functionalist theory, 36; her informants, 107–9, 112–15, 160; and man-woman role, 152–54, 159–60; her marriage, 105; and Parsons, 112; and primitivism, 108, 114–15, 176–81; her publications, 38, 107–9, 111, 115–17; and Reichard, 109  
 University of California at Berkeley, 86  
 University of Chicago, 35–36, 39, 86  
 University of Denver, 47, 111  
 University of Pennsylvania, 58
- Wesuji (Pohuati), 60  
 We'Wha' (Zuñi), 146, 147, 154–55  
 Williams, Walter, 18–19  
 Wiyot Indians, 89  
 Wollstonecraft, Mary, 75–76  
 women. *See also* ethnographers; feminism; abortifacients, 174; and assimilation, 94–96; and careers, 39–40, 41–48, 72, 100–102, 183; emancipation, 3–4, 40, 169; during the Great Depression, 132–34; pregnancy/partum taboos, 171–75  
 women-men, 158. *See also* man-woman  
 Worthington, Lucy Madison, 49  
 Wyman, Leland, 97–98
- Yaqui Indians, 36, 38  
 Yellowpine (Hopi-Tewa), 56  
 Yuma Indians, 33, 38
- Zingg, R. M., 38  
 Zunie, Catalina (Zuñi), 101  
 Zuni, Flora (Zuñi), 155  
 Zuñi Pueblo: and Benedict, 78, 79–80, 82–84; and Bunzel, 99–101; and Lewis, 120–21, 153, 155–59; and man-woman role, 146–47, 153–58; and Parsons, 56, 58–60; pottery, 168; studies of, 36, 37; and Tumaka, 82–84



Elsie Clews Parsons

**In the first decades of the twentieth century, the work produced** by women anthropologists dominated scholarship about the Native American Southwest. Against the backdrop of a rapidly changing American culture, early anthropologists sought examples of cultures capable of coping successfully with diversity and complexity.

Ethnographers believed that they had found such cultures in the Native American Southwest, and turned to these

cultures to make sense of their own. For women anthropologists especially, living in a society where women's roles and identities were hotly contested, Southwestern Indian cultures provided examples of more open possibilities for women.



Ruth Benedict

In *Scientists and Storytellers*, Catherine Lavender examines the work of a community of Columbia University-trained ethnographers—Elsie

Clews Parsons, Ruth Benedict, Gladys Reichard, and Ruth Underhill—who represent four generations of feminist scholarship about the region. In their analysis of Indian gender, sexuality, and supposed

“primitiveness,” these anthropologists created a feminist ethnography that emphasized women's roles in Southwestern Indian cultures. In doing so, they provided examples of Indian women who functioned as leaders in

their communities, as economic forces in their own right, as negotiators of cross-gendered identities, and as matriarchs in matrilineal societies—examples they intended as models for American feminism.



Ruth Underhill



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