INSCRIPTIONS

Papers in Honor of
Richard and Nathalie Woodbury

Edited by:
Regge N. Wiseman
Thomas C. O’Laughlin
and Cordelia T. Snow

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THE ARCHAEOLOGICAL
SOCIETY OF NEW MEXICO: 31

-2005-
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THE ARCHAEOLOGICAL SOCIETY OF NEW MEXICO: 31

PREFACE

The Archaeological Society of New Mexico has many kinds of members, most of whom are residents of the state. We have also had the good fortune to attract loyal members who live elsewhere and who bring us a wider view of what our field is.

The Woodburys, Richard and Nathalie, are long term members who through the years have given us enhanced links to institutions such as the University of Massachusetts, the Smithsonian and the American Anthropological Society. During their careers they have, individually or together, worked at Awatovi, published on Hawikuh, and excavated at El Morro. Their dedication to thorough and careful research, to following the evidence even when contrary to commonly accepted beliefs, and to the history of archaeology have been major contributions. Always generous with help and encouragement for students and younger colleagues, their freely offered friendship is even more valued. We have all benefited, some personally, some less directly through their writings. We are fortunate to have the opportunity to recognize such a deserving pair.
With the publication of this volume, the Archaeological Society of New Mexico honors two remarkable 20th-century anthropologists: Nathalie and Richard Woodbury. Trained as generalists and working from institutional bases in Arizona, New Mexico, Kentucky, New York City, Washington D.C., and western Massachusetts, they have carried out primarily archaeological research in the Southwest and Mesoamerica; promoted interest in the history of anthropology; provided extensive editorial, professional, and administrative service; and mentored, encouraged, and supported countless young colleagues. The extent and significance of their influence on the development of anthropology in this country has already been recognized by many individual honors and awards. In addition, they have received joint recognition from the Society for American Archaeology (1988), the Arizona Archaeological and Historical Society (1994), and the Archaeological Conservancy (2004).

I am pleased to have this opportunity to provide a summary of their many activities and to offer some comments on their lasting impact on our profession. Although the Woodburys have collaborated on many projects, worked together in various research efforts, and labored jointly on behalf of many shared goals, they emphatically reject the idea that they are a team (N. Woodbury 1985: 47). Differences in personality, work habits, specific interests, and professional style, they say, have prevented them from being a team, even though they can and do “work together.” Nevertheless, we, their friends and colleagues, observe that they have had a powerful joint impact on us and on our professional world. We can defer to their self-identification as a non-team, but we insist that, as a pair of creatively interacting individuals, they have done much to deserve the honor that this volume represents.
My own treasured association with them began in the summer of 1947 when I was an undergraduate student from Tufts University at the University of Arizona Archaeological Field School at Point of Pines and they were single and separately employed members of the staff. It was there in that same year that I met another staff member, Molly Kendall, who would become my wife the following year in the same month that Nat and Dick were married. In the next few paragraphs I summarize their separate youthful accomplishments prior to their becoming that creative non-team.

RICHARD BENJAMIN WOODBURY

Dick Woodbury was born on May 16, 1917 in West Lafayette, Indiana, the second child after older sister Barbara Anne, with younger brother Charles Benjamin yet to come. His horticulturist father, Charles Goodrich Woodbury [1884-1971], was Director of the Agricultural Experiment Station at Purdue University. His mother, Marian Benjamin Woodbury [1882-1973], was trained as an architect at Cornell University, although she never practiced professionally. When Dick was three years old, his father accepted a position on the scientific staff of the National Canners Association in Washington, D.C. During World War II he worked for the U.S. Department of Agriculture. While Dick was in Western High School in Washington, he worked as a volunteer for Neil Judd of the U.S. National Museum, which bolstered his growing interest in archaeology and the Southwest.

In 1934 he went to Oberlin College, but found little there to expand those interests. On the advice of Dean Mildred McCaffie (Horton), he transferred to Harvard College after two years at Oberlin. Three members of the Harvard faculty played important roles in his career development. JO Brew reinforced his interest in the Southwest and invited him to spend the summers of 1938 and 1939 at the Peabody Museum excavation of Awatovi on the Hopi Reservation in northern Arizona (R. Woodbury 1990). Clyde Kluckhohn honed his critical skills in stimulating undergraduate tutorials (R. Woodbury 1961a). Alfred Tozzer's lecture style did not inspire Dick and Tozzer's belief that an independent income was essential if one aspired to be an archaeologist further alienated him (Dincauze 1993: 8).

After earning a B.S. degree cum laude in Anthropology in 1939, Dick moved to Columbia University in search of a more stimulating intellectual environment. During that 1939-40 year at Columbia, he found Duncan Strong to be a source of such stimulation. It was also at this time that he first became acquainted with fellow graduate student Nathalie Sampson. In the summer of 1940 he broadened his archaeological experience by assisting Gordon Willey, another graduate student...
friend, in an archaeological survey of northwestern Florida.

Despite his satisfaction with the Columbia scene, he returned to Harvard University in the fall of 1940 to take advantage of a scholarship for Downer descendants, for which he qualified through his father's ancestors. He completed formal graduate work in December 1941 and received an MA from Harvard the following spring. From January 1942 to October 1945 he was a weather observer in the Army Air Force, serving in New Mexico, Australia, and New Guinea. Following his military service he returned to Harvard where he began work on the analysis of the stone tools from Awatovi, which was to be his doctoral dissertation. During 1946-47 he greatly enjoyed being a teaching assistant at Harvard for Carleton Coon.

At the invitation of Emil Haury at the University of Arizona, he spent the summer of 1947 as a Dig Foreman at Point of Pines. Haury had also invited Alfred and Madeleine Kidder to visit Point of Pines that year. Kidder shifted Dick's focus to Mesoamerica for a time by offering him a position as archaeologist on the United Fruit Company project to excavate and restore the Highland Maya site of Zaculeu in Guatemala.

NATHALIE FERRIS SAMPSON

Nat was born the youngest of three sisters on January 25, 1918 in Humboldt, Arizona, a mining community in Yavapai County named after the German naturalist Alexander von Humboldt. Her engineer father, Frank Herbert Sampson [1869-1944] went there in 1912 to build a smelter and then did some ranching. Nat was named after her mother Nathalie Ferris Sampson [1880-1968]. She is often asked: “How could a quintessential New Yorker like you be born in a remote Arizona mining camp?” The ever practical Nat always responds: “Because that is where my mother was.” When Nat was eight years old the family moved to Chappaqua in the northern suburbs of New York City where her mother's family had deep historical roots. Her father was employed by the engineering firm of Stone and Webster. Because of a bout with polio, Nat was home schooled until she entered the sixth grade. When she graduated from Horace Greeley High School in 1935, an aunt offered to help her go to Vassar College (following older sisters Barbara and Margaret Oakely), or to Smith College, neither of which appealed to Nat. Instead, she used a New York State Regents Scholarship to go (that is, commute) to Barnard College where she developed a lasting relationship with Gladys Reichard whose research among the Navajo reinforced Nat's strong and early interests in the Southwest. She attended the University of New Mexico Field School at Jemez with Paul Reiter in the summer of 1938 and at Chaco Canyon with Florence Hawley Ellis the...
following summer. Between the semesters of her senior year at Barnard, she lost the sight of her right eye in an automobile accident, a loss made more serious because she had earlier suffered some damage to her left eye in a playground accident.

She graduated from Barnard in 1939 and began graduate study in anthropology at Columbia University, where she had taken some courses during her senior year at Barnard. It was during that first year at Columbia that she became acquainted with Richard Woodbury. In the summer of 1940 she took part in an ethnolinguistic field study of Comanche color terminology with George Herzog under the auspices of the Laboratory of Anthropology in Santa Fe. At that time Columbia was emerging from the Boas era and there were two faculty groups, the Boas followers and another group moving in newer directions. Nat was identified with the Boas-Benedict-Reichard group, but because of her broad and eclectic interests, ranging from linguistics and folklore to archaeology and ethnology, she also interacted with the newer group, especially Duncan Strong and Ralph Linton. She often found herself in between the two groups, but because of her practical sense and her people skills she became a kind of broker working with them both. Under the influence of Marian Smith, she collected Muslim folk tales from Bengali seamen in New York for a dissertation that she never finished. While in graduate school she held a number of fill-in, temporary, and short-term teaching positions at Hunter College, New York University, and Brooklyn College. In 1945-46 she was an assistant professor at Eastern New Mexico College (now University) in Portales, where she learned first hand the problems of dealing with creationists.

A teaching position for a generalist opened up at the University of Arizona the following year. While the Columbia faculty dithered over which male student to recommend, Nathalie wrote directly to Emil Haury at Arizona, whom she had met when he lectured at the Chaco Field School in 1939. She got the position and spent 1946-47 in Tucson. Ned Spicer offered her a summer opportunity to study the impact of cotton-picking machines on Pima and Papago (O’Odham) laborers, but she opted for Haury’s offer of the position of Laboratory Assistant at the archaeological field school at Point of Pines. He mentioned to her that Dick Woodbury would also be there, but Nat insists it was a cool summer in the pines over a hot one in the desert that caused her to accept Haury’s offer. The Haurys picked up Dick at the railroad station in Tucson and drove both Nat and Dick up to Point of Pines. Nat noted that Dick had changed the bushy haircut that he had sported as a graduate student at Columbia that she had found unattractive.

FROM CAMBRIDGE TO AMHERST VIA THE REST OF THE COUNTRY

In his Foreword to Haury’s (1989) history of the Arizona field school, Dick Woodbury wrote the following paean to life at Point of Pines: “Everyone fortunate enough to have been at Point of Pines, whether as a student or a staff member, has many pleasant memories of the collegiality, intellectual stimulation, fine food, and beautiful natural setting among the pines” (R. Woodbury 1989: xi). He could also have added that it was a place that provided wonderful opportunities for social activities of the romantic kind. Nat and Dick and Molly and I took happy advantage of those opportunities and it became apparent to everyone at Point of Pines in 1947 (with the probable exception of Dick and me) that weddings were in the making. In fact, Nat and Molly had a chat at the end of the summer while sitting on a log in the pine forest that resulted in a modest wager as to who would get married first. Dick went to Guatemala to work at Zaculeu and back to Cambridge where he finished writing his disserta-
tion on the stone tools from Awatovi at the end of the summer of 1948. He and Nat were married on September 18, 1948. Molly and I spent 1948 at Point of Pines and were married on September 9. Several weeks later, Molly received from Nat a ten dollar bill with a note that said: “Best bet I ever lost.” However, Nat does complain that because of the timing of the wedding she missed the first year in Guatemala (N. Woodbury 1985: 23). The newly wedded Woodburys went to Guatemala for the second season at Zaculeu and returned to Cambridge to write up their part of the report. Dick was awarded a doctorate in anthropology from Harvard University in the spring of 1949.

With the completion of the Zaculeu report, Dick was ready for more permanent employment. He chose a position at the University of Kentucky over one at the U.S. National Museum because he was not ready to return to Washington D.C. In January 1950 he became Associate Professor of Anthropology and Curator of the Museum of Anthropology at Kentucky. He and Nat stayed there for two and-a-half years before responding to an invitation from Duncan Strong in 1952 for Dick to return to Columbia University as a tenured Associate Professor in the Graduate School. Nat obtained a position as Lecturer in Anthropology (and later Acting Chair and Assistant Dean of Students) with Gladys Reichard at Barnard College. In 1958 Emil Haury offered Dick a research position as an Associate Professor of Anthropology in the newly established Interdisciplinary Arid Lands Program at the University of Arizona. Dick accepted and Nat was appointed a Research Associate in the Arizona State Museum.

In 1963 Dick returned to Washington D.C. to accept a position in the U.S. National Museum where he had been a high school volunteer, succumbing to what Dena Dincauze (1993: 10) called “one of the grand inevitabilities of his life.” He was Curator of Archeology and Anthropology and later Chairman of the Office of Anthropology. Nat was made a Research Associate of the Smithsonian Institution. Despite the interesting challenges of the Smithsonian, Dick found that he missed the intellectual stimulation of university teaching. In 1969 he accepted the responsibility of guiding the development of a new Department of Anthropology that had just gained independence from a joint department with sociology at the University of Massachusetts at Amherst. When Dick arrived in Amherst to take up his new responsibilities, he discovered that all three of the administrators who had so vigorously wooed him and hired him were no longer in office or still at the university. He wondered if he even had a job and called the experience “a funny thing happened on the way to U. Mass.” Fortunately, universities are remarkably stable institutions, so once Dick had met the new Chair of Sociology, Dean of Arts and Sciences, and Provost, rapid progress was made.

Dick served as Chairman of the new Department of Anthropology from 1969 to 1973, continuing as Professor of Anthropology until his retirement in 1981. Throughout the years, Nat and Dick maintained the fascination with the Southwest that had nurtured their early interests in anthropology. They purchased land in Santa Fe in anticipation of moving to New Mexico upon retirement. Instead, they expanded their home in Shutesbury to include guest quarters and remained in Massachusetts to provide gracious hospitality to the many friends and colleagues who visit them.

**RESEARCH**

Although Dick and Nat have wide ranging interests, their research has been focused primarily on the Southwest. The main exceptions have been survey and excavation in northwest Florida in 1940 (Willey and R. Woodbury 1942), excavation and restoration of Zaculeu in Guatemala (Trik and Woodbury 1953, with a chapter on the history of Zaculeu by Nat), and excavation with William S.
Webb of the University of Kentucky in 1950-51 of an Adena burial mound at Dover, Kentucky. Their Southwestern research emphasized stone artifacts, Zuni prehistory, and prehistoric adaptations to arid environments.

Dick's landmark monograph on the stone tools from the Hopi site of Awatovi in northern Arizona (R. Woodbury 1954) was the first extensive study of Anasazi artifacts since A. V. Kidder's (1932) pioneering work at Pecos. It is interesting to note that Dick had undertaken an analysis of stone artifacts from site BC 50-51 in Chaco Canyon excavated by his tutor Clyde Kluckhohn while he was an undergraduate at Harvard (R. Woodbury 1939).

The Zuni research was stimulated by Duncan Strong who urged Dick to develop an excavation project in the Southwest when he joined the faculty at Columbia. He and Nat dug at Atsinna, an ancestral Zuni site on top of El Morro National Monument in 1953-55 with Columbia graduate students and Zuni workmen (R. and N. Woodbury 1956; R. Woodbury 1956). An unanticipated extension of the Zuni research was the collaboration from 1961 to 1964 with Watson Smith on the preparation for publication of all surviving data from Frederick Hodge's excavations at the Zuni site of Hawikuh (Smith, R. Woodbury, and N. Woodbury 1966). Dick continued an interest in land use in the Zuni valley over the following decade (R. Woodbury 1979).

The study of aboriginal water use and agriculture in arid environments worldwide owes much to Dick's insightful and fundamental research that began when he joined the Interdisciplinary Arid Lands Program at the University of Arizona in 1959. He has published more than 20 papers and monographs that greatly advance our understanding of the human adaptation to the extensive arid regions of the world (R. Woodbury 1959, 1963, 1970). This work had its origins in the Southwest with excavation of Hohokam irrigation canals (R. Woodbury 1960) and analysis of prehistoric agricultural practices at Point of Pines (R. Woodbury 1961b). He carried out similar research in the Tehuacan Valley in Mexico (Woodbury and Neely 1972) and in the best tradition of comparative anthropology he extended the scope of this research to include non-industrial water management systems in other areas, including North Africa, the eastern Mediterranean, and South Asia.

In 1961-62 Dick and Nat worked as a team on a study of Papago (O'Odham) land use for the Bureau of Ethnic Research (now Bureau of Applied Research in Anthropology) at the University of Arizona. Dick did most of the field work and Nat most of the interviewing and compiling of archival data. While on the reservation she was housed with the family of the agricultural administrator who was a Comanche. Because of her work among the Comanche in 1940, she knew some of his relatives, which helped in developing a good working relationship with him. Nat and Dick published this work jointly (R. and N. Woodbury 1964), but the fact that they co-authored only six publications goes a long way toward proving that although Nat and Dick certainly did "work together," they did not work as a team in most cases. Dick has been the more prolific writer with more than 80 publications and more than 100 book reviews (see his bibliography in Krass, Thomas, and Cole 1993: 15-19).

THE HISTORY OF THE DISCIPLINE

Both Nat and Dick have promoted an increased interest in the history of anthropology with an almost missionary zeal. Dick wrote the definitive biography of A. V. Kidder (R. Woodbury 1973), a fascinating history of the Pecos Conference that is almost a history of Southwestern archaeology (R. Woodbury 1993a), and valuable obituaries of J. O. Brew, Alden C. Hayes, Clyde K. M. Kluckhohn, Richard S. MacNeish, and Watson Smith (R.
Woodbury 1990, 1999, 1961a, 2002, 1993b). Nathalie provided necrology for the Anthropology Newsletter from 1978 to 1991, editing and writing countless obituaries and death notices. In 1984 she began a column of historical commentary, "Past Is Present," that appeared in almost every Newsletter through 1996. She covered a wide range of topics in sparkling essays that contributed directly to a broader appreciation for the history of anthropology. She and Dick celebrated the role of wealthy women in Southwestern anthropology (N. and R. Woodbury 1988) and provided an extremely useful review of the complex history of the Bureau of American Ethnology (R. and N. Woodbury 1999). When major journals started to eliminate obituaries, both of them vigorously protested and mounted a spirited defense of the value of obituaries as basic historical documents. They have also provided historical context for several monographs and books in their carefully crafted Forewords (for example, N. and R. Woodbury 1993). Through their many seminal historical contributions they have stimulated renewed interest in the history of the discipline and have inspired a new generation of historically minded colleagues.

GOOD CITIZENS

"The first requisite of a good citizen," said Teddy Roosevelt, "is that he shall be able and willing to pull his weight." By this standard the Woodburys are no less than super citizens, for they have given an incredible amount of editorial, professional, and administrative service to their profession. Their editorial work alone is truly impressive. Nat got her first editorial experience when she was a graduate student at Columbia assisting Ralph Linton when he was Editor of the American Anthropologist and Marian Smith when she was an assistant to Doug Byers, Editor of American Antiquity. Later in her career she was offered the editorship of both of these journals and the Journal of American Folklore. Although Nat has plenty of editorial competence and the willingness to take on the enormous work load involved, she modestly declined all three offers. She believed that the major journals needed someone with high visibility in the profession in terms of research and publication. She considered herself to be a generalist and a pragmatist more interested in identifying and meeting challenges than in participating directly in the traditional research arena (Fox 1993: 298, 307; N. Woodbury 1985). She served as Associate Editor of the American Anthropologist (1973-75), Assistant Editor of American Antiquity (1954-58), Associate Editor for Notes and News (1958-62), Associate Editor for Obituaries (1985-87), and Associate Editor of Abstracts in New World Archaeology (1960-61). She took on the editorship of the Anthropology Newsletter and the Guide to Anthropology Departments when those two basic publications of the American Anthropological Association were still in their formative stage (1968-74).

Dick was Editor of American Antiquity, including six Memoirs of the Society for American Archaeology (1954-58), founder with former student Charles Brush and Editor of Abstracts in New World Archaeology, and Editor of the American Anthropologist (1975-78). Although the Abstracts only lasted for two volumes, the practice of publishing abstracts with journal articles, beginning with American Antiquity in 1957, was widely adopted. Today commercial publishers have revived the idea of compilations of abstracts. Dick's editorial talents also benefited the Archaeological Institute of America (Review Editor for the New World, American Journal of Archaeology 1957-72, Editorial Advisory Board Archaeology 1958-67), American Association for the Advancement of Science (founding Editor of the Arid Lands Research Newsletter 1959-60), American Ethnological Society, and the School of American Research. Both he and Nat were involved in the editing of Societies Around the World, a multi-cultural text book conceived by colleagues at the University of Kentucky (1952-53).
Professional and public organizations depend on volunteer participation to carry out their responsibilities. Dick and Nat Woodbury have done more than their share of such work. Dick has served the Society for American Archaeology (Treasurer 1953-54, Editor 1954-58, President 1958-59, Chair of Finance Committee 1987-89), the American Anthropological Association (Executive Board 1963-66, Editor 1975-78), American Association for the Advancement of Science (Council 1961-63, Committee on Arid Lands 1969-74), Archaeological Institute of America (Executive Council 1965-66), National Research Council (Division of Anthropology and Psychology, Executive Committee 1955-57), National Science Foundation (Anthropology Panel 1970-73), Committee for the Recovery of Archaeological Remains (Smithsonian Institution liaison 1965-69), Human Relations Area Files (Executive Committee of Board of Directors 1968-70), Museum of Northern Arizona (Board of Trustees 1983-90), New York Academy of Science (Secretary of Anthropology Section 1954-55), International Union of Anthropological and Ethnological Sciences (Commission on Documentation 1978-85), Archaeological Conservancy (Founding Member, Board of Directors 1979-84), and Connecticut Historical Commission (Consultant on Archaeology 1970-72). Dick applied to his administrative roles at the U.S. National Museum (Office of Anthropology, Acting Head 1965, Chair 1966) and the University of Massachusetts at Amherst (Founding Chair, Department of Anthropology 1969-73, Acting Dean of Graduate School and Associate Provost 1973-74) the same patient, even handed, gracious, and organized approach to controversies and disputes that made him such an effective member of the many organizations he served.

Nat found many opportunities to apply her common sense and practical approach to problem solving in the several organizations she served in her self-identified role as “generalist and association administrator”: American Anthropological Association (Editor of the Anthropology Newsletter and the Guide to Departments 1968-74, Secretary 1970-75, Executive Board 1975-78), Society for American Archaeology (Treasurer 1965-69), American Ethnological Association (Councilor 1956-59, Secretary 1960-66, Treasurer 1978-80), and the Anthropological Society of Washington (Board Member 1964-67). Nat’s early graduate student experience of mediating between faculty groups at Columbia served her well during her many years of service to these organizations during a period of major changes and considerable controversy in the discipline.

Dick and Nat have also been good citizens in their community, the Town of Shutesbury in the Connecticut River valley in western Massachusetts where they settled when Dick accepted the position at the University of Massachusetts at Amherst. Dick served on the Advisory Council of Valley Health Plan (1978-81) and as a member of its Board of Directors (1981-84). He was elected to the School Committee of the Town of Shutesbury (1979-82, Chair 1980-82) and to the Board of Assessors (1982-85). The Woodburys have long been interested in dogs and have given much of their time to the Dakin Animal Shelter in Shutesbury. Dick was Chairman of the Finance Committee of the Friends of Amherst Stray Animals from 1983 to 1985 and has served as a Trustee of its Shelter Trust Fund since 1992. He has been Secretary of the Shutesbury Historical Commission since 1999. Nat was appointed Finance Officer of the Town of Shutesbury (1976-79) and elected Auditor (1979-82) and Selectman (1983-85). She brought to the business of running a small New England town the same enthusiasm, determination, hard work, and common sense that she had used in working for anthropological organizations, and she thoroughly enjoyed it. On Celebrate Shutebury Day in September 2004, the Town officially recognized both Nat and Dick for their many years of dedicated public service with a plaque stating that they “...have significantly improved the quality of life in Shutesbury.”
HONORS

In 1978 the American Anthropological Association recognized Nat as a super citizen in her profession by giving her its Distinguished Service Award for her "creative and caring leadership over more than two decades in many roles, ... an unsurpassed contribution which has set the standard of service for all who follow." The Society for American Archaeology, in addition to the Distinguished Service Award given to Nat and Dick together in 1980, honored Nat with a Presidential Recognition Award in 1990. The Department of Anthropology at the University of Massachusetts at Amherst established an annual Nathalie F. S. Woodbury Award to honor a student who has made outstanding contributions to the department's activities, an especially appropriate award for it honors two of Nat's special interests: service and junior professionals.

In 1989 Dick received the most prestigious honor available to an Americanist archaeologist, the Alfred Vincent Kidder Award for Eminence in the Field of American Archaeology. In making the award the American Anthropological Association cited him as "an exemplary leader in the analysis and interpretation of archaeological resources." The Department of Anthropology at the University of Massachusetts at Amherst established the Woodbury Scholarship in 1981 to provide travel support for students giving a paper for the first time at a national meeting and in 1993 published a volume of essays in his honor (Krass, Thomas, and Cole 1993). He and Nat both received the 50th Anniversary Award of the Society for American Archaeology in 1985 and the Cummings Award of the Arizona Archaeological and Historical Society in 1994. In 2004 the Archaeological Conservancy (of which Dick was a founding board member) also honored Dick and Nat jointly by naming one of its permanent archaeological preserves in New Mexico, an ancestral Zuni cluster of 15 room blocks built between A.D. 1210 and 1276, "The Woodbury Community."

Throughout their long and productive careers, Nat and Dick have treasured above all else their relationships with friends, colleagues, and students. They have especially enjoyed working with aspiring young professionals who invigorate and advance the discipline of anthropology. They have developed and maintained an amazing network of correspondents who provide them with information about openings, opportunities, problems, and challenges. Nat and Dick broker that information to the many young people that they nurture and mentor "after the fashion that was so successful for both of them in their formative years" (Dincauze 1993: 9). The countless members of that multitude of grateful recipients of their concern and friendship thank the Archaeological Society of New Mexico for publishing this volume in honor of those creatively interacting Woodburys.

ACKNOWLEDGMENTS

I thank the Woodburys for supplying many important details, Dena Dincauze (1993) for her thoughtful biographical tribute to Dick, and Carol Gifford, Alexander Lindsay, and Molly Thompson for their help.
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Woodbury, Richard B. and Nathalie E. S. Woodbury


It feels like once upon a time: the Eisenhower Fifties, when college girls might be co-eds, wearing crinoline petticoats to fluff out their skirts, angora sweaters over stiff bras shaped like rocket nose-cones. Back then, Dick Woodbury taught archaeology at Columbia University in Manhattan, and Nat taught across Broadway at Barnard College. Two girls met as Barnard freshmen, eager to study anthropology and archaeology. We—Dena Ferran and Alice Beck—found that Barnard had one anthropology professor, Gladys Reichard, and that she would not permit freshmen in her courses, because anthropology, she said, demanded more mature minds. A year later, we were admitted to the privilege. Our anthropology classes were small, and what we were to learn was Reichard's take on Boas' anthropology. Nat Woodbury assisted (I never really knew what title Barnard gave her), our sounding-board and refuge as we tried to understand Reichard’s eccentricities. Then, unexpectedly, Dr. Reichard died during the summer after our sophomore year. Now, to fulfill the requirements for an anthropology major, we were forced to take most of our courses across the street at Columbia. (The best of two worlds then, a first-rank anthropology faculty and the protection of a firmly feminist women's college.) Thus, we took not only lecture but also a seminar in archaeology from Dick Woodbury, and became close to Nat, the anthropologist at Barnard when Reichard did not return.

Two experiences with the Woodburys highlight my memories of college. Dick's seminar on American archaeology, my senior year, clarified how an archaeologist works as a scientist. Nat prepared the comprehensive examination in our major for us anthropology seniors; it was brilliant, asking us not to parrot back facts or canned theories but to give sources for answering questions. That is how a scholar works!

At the time I took his courses, Dick was editor of American Antiquity. I remember assisting once in preparing artifact photographs for an article, using the skill I honed in kindergarten to cut out photos and paste them on a clean board. Over my head, as it were, Dick was guiding the field with his selection of papers and editing advice. This was the period of postwar movement into greater discussion of theory, as distinguished from methodology. In 1955, the Carnegie Corporation provided funds for four seminars to advance discussion on four themes: culture contacts, cultural stability, cultural isolation, and implications of community patterning. Most of the seminar reports (Wauchope 1956) offered classifications of types of societies, such as “Simple Nuclear Centered,” or situations, such as “Site-unit Intrusions,” although the seminar on the American Southwest as “A Problem in Cultural Isolation” brought in substantial data and concluded that Southwest societies were very far from being isolated. Simmering under the “Cultural Stability” discussion, from a seminar including Albert Spaulding, was the issue of quantification and statistics exploded in the recent sharp debate between James Ford (1954a, 1954b) and Spaulding (1953a, 1953b). The Woodburys oversaw the editing and publication of the seminars as a Memoir of
the Society for American Archaeology, but in their roles of American Antiquity editor and editorial assistant, did not participate in them.

As I recall those two academic years, 1954-55 and 1955-56, I realize Dick's wisdom in keeping our student noses to the grindstone of empirical data. In Michigan, Spaulding's conviction that statistics would reveal everything, Leslie White's vulgar-Marxist evolutionism, and James Griffin's passion for fine-tuned ceramic typologies were exciting grad students who would soon carry a New Archaeology into the Southwest. Meanwhile, Dick's low-key, kind teaching, extended into his editing of American Antiquity, kept us focused on what was in the archaeological record, what was not in that record, and why, and how that record reflected the ecologies of past societies. This attention would soon lead Dick to move to his major project on arid lands.

Another shift occurring in the late 1950s engaged Nat Woodbury. Both American archaeology and American anthropology were growing, fueled by large salvage projects, United States postwar supervision of overseas territories, and the enormous expansion of universities inundated by veterans taking advantage of the G.I. Bill. Both the Society for American Archaeology and the American Anthropological Association were managed by members as minor part-time obligations, the societies' business files kept in their offices alongside their own records. Crises loomed, in the case of the American Anthropological Association precipitated by a mentally unbalanced clerical employee who hid dues checks rather than processing them. Nat Woodbury analyzed the two societies' needs and proffered plans for professional management. Somehow the societies might have muddled through without Nat, but she was—from her business card said—the Facilitator.

Reflecting back on those two years at Barnard/Columbia, I realize that we learned more than the Woodburys ostensibly taught: we saw a marriage of true mutual respect, and as the years moved by, that creative scholars can find many varied ways of doing interesting and worthwhile work. My classmate Dena Ferran Dincauze followed Dick Woodbury's path to be editor of American Antiquity and then president of the Society for American Archaeology. That sense of civic responsibility to our profession modeled by the Woodburys led me, also, to accept offices in our societies. We learned from the Woodburys that to practice archaeology entails commitment to data and commitment to the broad range of colleagues in our discipline. Both arenas of practice call for creative, innovative, open-minded, observant, careful, self-less, unstinting work. Modest, without fanfare, both Dick and Nat very significantly enriched the discipline and profession of archaeology.

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Navajo clans can be correlated with archeological data in various ways. It is first necessary to describe what Navajo clans are, for popular views of clans do not fit the Navajo reality.

As clans exist among the Navajos, they are exogamous social units based on real or adoptive kinship through descent in the female line from one or more common ancestresses. Although captives in early times are said to have founded clans, usage in 1800s led to incorporation of captives or their descendants into the clans of their captors or owners (Brugge 1985:143; Hill 1936:16; Matthews 1994:146,158).

There are over sixty clans which are organized into nine clan groups. Each clan group is, or once was, exogamous. While children are born into their mother’s clan, they are “born for” the father’s clan and must not marry members of either. Marriages to grandfather’s clans are usually, but not always, forbidden (Franciscan Fathers 1910:428-31; Kluckhohn and Leighton 1962:111-14; Reichard 1928:12-26).

When Navajos introduce themselves to others, they do so by identifying their own clan, father’s clan and both grandfathers’ clans. When speaking of their own or others’ clans, some may use well known clans in a group to substitute for small, little-known clans.

Regulation of marriage is just a small part of clan functions, the extension of incest categories being but one aspect of the kinship established by clan membership. Clan relationship helps ensure the mutual trust that being kin implies and carries with it the obligations culturally prescribed for the various named kin, both genetic and through marriage. Clan ties can influence political connections, choices of activities, rights to hospitality, participation in task groups, the granting of permission to use a water source, and the making of gifts and loans, among other matters. They are especially significant in linking people in different communities across Navajo country, thereby forging a unity that melds the Navajo people into one nation (see especially, Williams 1970:37-39; Kluckhohn and Leighton 1962:111-13).

Ideally all clans are equal, but in the past some clans were thought of as “slave” clans due to origin by capture (Franciscan Fathers 1910:427-31; Reichard 1928:15-16). There are stereotypic beliefs concerning clan characteristics, some harmless, but some pejorative. Jokes about clans are not easily translated, but they are often not unlike in tenor ethnic jokes in mainstream American society. Like many stereotypes, the good or bad aspects may be called upon according to circumstances.

Native clans sometimes arose from local groups and thus have names derived from places, activities or characteristics of their progenitors. Others can cite mythic texts to account for their existence. Many clans are of foreign origin, however. The rules of exogamy helped ensure their integration through intermarriage, and some have
mythologized their origins in order to claim a more native status.

There are two types of old Navajo clans. First are those of local origin, some claiming descent from people who survived the age of the monsters or who assert some supernatural event for their creation. Second are the Western Water Clans, said to have been created by Changing Woman, a major Navajo deity, at her home on an island in the Pacific Ocean. Different versions of this story count four to six clans. Four clans are mentioned in most accounts, Todich'ii'nii, Hasht'ilishnii, Kiyaa'aanii and To'ahahí, at least three of which appear in all recorded clan texts.

These two categories have been uncomfortably reconciled in the origin stories, but they seem to indicate a very early, prehistoric merging of two peoples. Whether the union was of two Athabaskan-speaking groups is not clear, nor is it certain just where this took place, but it is usually described as happening on the San Juan River. One text describes the purposeful combining of two languages or dialects (Matthews 1994:135-59).

The more recent incorporation of non Navajos is generally more straightforward. Their origin stories often retain sufficient detail to be matched with historic data. Two forms of incorporation appear, one of willing inclusion, the other of the taking of captives.

There are several time levels for clan origins that must have had different effects on Navajo culture. Two caveats must be kept in mind. It is difficult to discern which cultural traits were introduced by immigrants and which gained through outside contacts. Secondly, as Jeffrey Dean (1996:27) has described, "Such behaviors need not be overtly expressed...they can be latent in the knowledge system...to be activated as situations require."

The earliest periods would be in pre-contact times and may involve cultural acquisitions while still living in the subarctic regions, in the course of migration, and initial adaptations to a southwestern environment. Hypotheses regarding cultural changes during these times are still highly conjectural. Equally speculative must be ideas about the period of Spanish exploration, when Mexican Indians and an occasional priest remained behind among the Pueblo Indians. That any of the former joined the Navajos seems unlikely, but not impossible.

Following this, from the time of settlement of the New Mexican colony in 1598 to the Pueblo Revolt of 1680, Pueblo people frequently fled the conquest and then the missions to find refuge in neighboring tribes. That the Navajos hosted their share cannot be doubted (Brugge 1969:200). The archeological evidence of Puebloan presence among the Navajos may be the appearance in some Navajo sites dating between 1620 and 1680 of Gobernador Polychrome, a painted Navajo ceramic type that bears a close resemblance to the matt paint wares of some of the pueblos (Reed and Reed 1996:94-108). There are hints of other Puebloan, Mexican Indian and Hispanic cultural complexes being acquired before the Revolt. Governor Cuervo y Valdes in 1706 wrote that sheep husbandry and weaving in wool were "nothing new" among the Navajos (Hackett 1937:381-82). Successful sheep raising implies the presence of people who knew this skill (Brugge 1995). That they had also adopted much of the Spanish equestrian tradition well before 1650 is almost certain. Finally, the similarity of at least two layout patterns of Navajo sandpaintings to depictions in Mesoamerican codices suggests quite early Mexican Indian contact, if not during the 1500s, not likely later than 1680.2

The Pueblo Revolt can be firmly identified with the capture of two Spanish girls, Agustina de Peralta and Juana Almassán. In the 1740s Franciscan missionaries visited a Navajo community that they called Pueblo Españoles, a name that they attributed to the two girls. There is nothing to indicate that these women were still
alive, but their descendants were (Reeve 1959:16-17, n.17). One Navajo tradition relates that two Spanish girl babies were captured by a Navajo of the Nósda’idné’é or Ute People Clan. The girls were raised at Tó’aheedlí, the junction of the Pine and San Juan rivers. One of the girls became the progenitor of the Nakaaidné’é or Mexican People Clan. The other girl founded the Tó’aheedlíinii Clan (Brugge 1966).

There is no detailed itinerary for the priests’ visits, but some Navajo sites dating from the 1740s and early 1750s in the Largo-Gobernador region have indications of strong Hispanic influence. Frances Canyon Ruin, from which the latest tree-ring date is 1745, Three Corn Ruin with a similarly late date and Old Fort dating as late as 1753 (Towner 2003:52, 69, 109, 177) have yielded many artifacts of Spanish origin (Carlson 1965:21-28, 94), some very likely gifts of the missionaries.

The Mexican People and Ute People clans, as a result of their names, retained definite memory of their origins. The Tó’aheedlíinii, however, lacking a name specifying ethnicity, has acquired a story of supernatural origin that has been widely accepted (Mitchell 1978:188).

With the Spanish Reconquest in the 1690s, many Pueblo people again fled to the Navajos. Some returned to their natal societies, but others remained and women from diverse pueblos were frequently reported in Navajo country in subsequent years (Brugge 1979:109-19; 1985:42-43; Hendricks and Wilson 1996:20, 45, 90-92). It must be assumed that they had significant cultural influence, but it is difficult to specify what it was in view of the refugees who had preceded them. Having once been Christian, they may well have introduced some Christian concepts. The welcome given the missionaries in the 1740s may in part have been due to their influence, as perhaps was resort to Pueblo and Hispanic architecture when small fortresses were built to defend from Ute raids.

The next major addition is attributed to the destruction of the Hopi pueblo of Awatovi in 1700. Navajo clan traditions mention several subclans of the Táchii'nii Clan that have Navajo names that are translations of Hopi clan names. Most prominent of these is the Tobacco People Táchii'nii, many of whom now live in the Jeddito region near the ruins of Awatovi (Brugge 1994:9). Another, the Ye'iidine'é Táchii'nii, best translated as the Kachina People Táchii'nii, also retain knowledge of their Awatovi ancestry (William Beaver, personal communication 2004). Navajo tradition cites Hopi refugees introducing peach trees to Canyon de Chelly (Jett 1974:369). Other Hopis fled to Navajo country later in the 1700s and the peach orchards may have begun with either event (Thomas 1932:109-10).

The final period of clan formation with incorporation of outsiders comes during the last Indian wars in the late 1800s. Both the Chiricahua Apache and Mescalero Apache clans are said to have their beginnings in these years. Two curing ceremonies, Chiricahua Windway and Sucking-way, have been traced to Apache origins, as has the diagnostic procedure called Hand Trembling (Haile 1950; Wyman 1962:214-16). By integrating immigrants into the tribe, clans helped the Navajos survive and grow into one of the three largest tribes north of Mexico. The Cherokees and the Chickasaws, the other two, are also tribes that absorbed members from many sources, at the same time adapting many elements of Old World cultures while still independent peoples.

Tracing the culture history of the Navajos in tandem with clan origins requires a base line for first contact with Old World peoples. There is no identifiable reference to Navajos in the records of the first major Spanish expedition into what is now the Southwest in 1540. In 1583 the Espejo expedition encountered people certainly Navajos near Mount Taylor. They planted crops, but also
brought products of the hunt to trade at Acoma Pueblo (Hammond and Rey 1966:181, 200-01). A quarter century later, Oñate, founder of the New Mexico colony, placed the elderly survivors of his attack on Acoma with these same people (Hammond and Rey 1953:478). Not until the 1620s were observations of Navajo ways made that led to written descriptions set down the following decade. It has been customary to rely on the two descriptions by Fray Alonso de Benavides for inferences of the Navajos at first contact (Hodge, Hammond and Rey 1945; Forrestal and Lynch 1954).

We still know remarkably little about Navajo ways during these early years. One example of a question of considerable interest is the beginnings of weaving by the Navajos. Their weaving technology is native Southwestern, closest to that of their Puebloan neighbors. Accounts from the 1700s mention weaving in both cotton and wool. It is entirely possible that weaving long preceded the introduction of sheep, perhaps even well before the arrival of Europeans in the Southwest.

There is much yet to learn before we can feel that the Navajo past is understood in such depth that we can be confident as to the time of arrival of their Athabaskan ancestors in the Southwest, the route or routes by which they came or how they adapted to their new home. We cannot even be sure that the original nucleus of the tribe spoke an Athabaskan language. Perhaps there was a linguistic change when a local group accepted immigrants from the north.4

It is clear that the clan stories relate to history, and even to prehistory, but that they hide mysteries, some purposefully, some the result of the wear and tear of telling over generations. They sometimes blur the passage of decades and centuries. Significant detail is often lost in translation into English.

They also give meaning to life in the present. It will require more than mere listening to probe the mists of time: hard work, ingenuity, learning to think outside our own cultural parameters, and perhaps a measure of luck. The stories can inspire hypotheses and theories almost endlessly, but to choose among the possibilities to focus on the most likely, undeterred by preconceptions, cannot be hurried. In the interim, we should not ignore any apparent insights, whatever their source. Recording all the variants, especially from clan members, needs to receive high priority, as does the search for independent data.

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I must thank Rena G. Martin for a copy of her Master of Arts thesis which added significantly to my data base as well as expanded greatly my appreciation of the importance of clan traditions for Navajo families and William Beaver for his comments on the Navajo clans descended from survivors of the destruction of Awatovi Pueblo. I have profited from conversations with the late Joe Ben Wheat regarding the origins of Navajo weaving. Previous versions of this paper were presented at The Navajo Studies Conference in 2001 and at the 2003 New Mexico Archaeological Society annual meeting.
ENDNOTES

1 The total number of clans that have ever existed is not known, for some clans are now extinct. Some clan names may be counted as separate, as subclans, or as alternate names.

2 Most distinctive are Navajo radial drypainting layouts with deities in the cardinal directions and plants in the NE, SE, SW and NW, sometimes with birds perched on top. Arrays of deities in ranks seem reminiscent of some codex depictions as well (See especially Seler 1902:104).

3 Other subclans sharing this origin are the Tansy Mustard People Táchii'nii, Deer People Táchii'nii, Rabbit People Táchii'nii and Sun People Táchii'nii (Brugge 1994:9).

4 Tradition recorded by Matthews (1994:141-3) in the 1890s describes “the old Navajo” as “a poor language” which was transformed when two chiefs united it with the language of a new clan, the Tábąąha, which had better words and so the new language came to be more like that of the immigrants. The people of the Tábąąha “were good hunters, skilled in making weapons and beautiful buckskin shirts” and who also ate ducks and fish, a description that might well fit immigrants from the Subarctic.

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My object in making a collection was to save as much as possible before it would be destroyed, & if not, before it could be sold to private individuals without a history & by whom in many cases it would only be used for decorative purposes in their home.

(J.W. Simmons to A.V. Kidder, letter, 28 March 1931, Arizona State Museum Archives, University of Arizona, Tucson [ASM] A-49)

In central Arizona, the ruins of Yavapai County received attention as soon as Euro-Americans arrived in the 1860s when impressive sites like Montezuma Castle were visited and named. The first documented excavation in the county was in 1895 by Jesse Walter Fewkes (1896) of the Smithsonian Institution at Honanki, near what is now Sedona. No other excavation is documented in the county until the late 1920s when J. W. Simmons enters the picture.

Simmons began his archaeological work at a time when three key southwestern archaeologists were in transition: J. W. Fewkes was ending his long career; A. V. Kidder was done with Pecos Pueblo and would shortly be grudgingly pulled away to Mesoamerica; and Byron Cummings was basically done with fieldwork in northern Arizona but was extremely active in consolidating his hold on Arizona archaeology. Each one of these archaeologists-in-transition had a role in Simmons’ early career.

J. W. Simmons finds a calling

What little we know about J. W. Simmons as a person comes mostly from his own letters and notes, numbering nearly 3,000 pages. His first name was James although he signed his letters “J.W.” He was apparently raised in Illinois and found an occasional spearhead as “a hare-foot plough-boy” (J.W. Simmons to E.W. Haury, letter, 8 February 1935, ASM A-53). In one letter, he says that his wife “learned” him how to compose a letter when he was 30 and admits in another letter that he had a “limited education” (J.W. Simmons to H.S. Gladwin, letter, 21 February 1931 and 6 February 1933, ASM A-20). He wrote of the past about “my own family” but in the present about being “single” and free of “family obligations” (J.W. Simmons to H.S. Gladwin, letter, 15 November 1932, ASM A-20; J.W. Simmons to Odd Halseth, letter, 18 April 1937, Pueblo Grande Museum, Phoenix), so we can say that he had been married, but by the time he got to Arizona
was either widowed, divorced, or separated. He made his living as “a building laborer: hod carrier, mortar man, & scaffold builder” (J.W. Simmons to A.V. Kidder, letter, 14 March 1931, ASM A-49).

Simmons gained his interest in archaeology at the age of 50 when in 1926 he visited the excavation of J. W. Fewkes at Elden Pueblo. According to the journal of Fewkes’ assistant, J. P. Harrington, Simmons visited the site on the weekend of June 26-27 and helped place artifacts back into position in graves so that they could be photographed (Harrington 1926). Apparently Fewkes and Harrington had been having trouble with people stealing grave goods after they left for the day. Simmons had a camera and as a result of being in the right place at the right time, four of his photographs of burials and a general overview of the site were published in the preliminary, and only, report on the site (Fewkes 1927: Figures 209, 213-215). Simmons watched Harrington take careful notes on the burials and saw how essential the recording of context was (Simmons n.d. c:56). At Elden, he may have met Paul Mittvalsky (Wilcox 1987:30), whose brother Frank (who changed his last name to Mitalsky and finally Midvale) was an aspiring archaeologist and became a close friend of Simmons.

Fewkes and Harrington both encouraged Simmons by mail, and he was always thankful for their support (Figure 1). In the introductory section to one of his manuscripts, he says, “It is with deep feeling of gratitude when I recount in detail the help these two good men were to me. For they put my feet on the right trail and to honor them I have tried my best to keep them there.” (Simmons n.d. e).

Although assessments of Fewkes’ career in the Southwest have not been particularly positive (Cummings 1926:321-322; Green 1979:30-31, Note 24; Hayes 1985:17; Hinsley 1981:281, 1983:63-65; but see Fowler 2000:161-171), he was a notable campfire speaker (Smith 1988:93, 94; Swanton and Roberts 1931:614) and had a rapport with the “average man” (Swanton and Roberts 1931:616). Simmons’ brief encounter with Fewkes was at the end of his long career and we get a glimpse of someone who encouraged a novice to pursue studies of prehistoric archaeology.

Simmons’ first archaeological collection was made in September 1926 when he returned to Elden Pueblo and worked the dump that Fewkes had left (J.W. Simmons to A.V. Kidder, letter, 14 March 1931, ASM A-49). He began as an artifact collector but was unusual in that from the very beginning he documented his sites with photographs and fieldnotes. He found sites by driving around on the back roads and from contacts with other collectors and pothunters. At least prior to the depression, he also bought provenienced specimens to fill in gaps in his collection (J.W.
Simmons to A.V. Kidder, letter, 14 March 1931, ASM A-49).

While working in Prescott in late 1926, he heard about the Coyote Ruin from Ethel Young, who had an art studio and curio shop in town. He went out to the Coyote Springs Ranch and talked with the owner, Jack Stanley who gave him the O.K. to dig. This site appears to be Simmons' first of many major projects. His manuscript on the site does not provide a good clue as to how long he spent there, whether he had any help, and what exactly he did (Simmons n.d. e). His report does, however, indicate some of the questions that he tried to answer. He felt that the site had an early and a later occupation on the basis of dwelling types and pottery differences. He also found evidence of agricultural features on the site and one nearby and spent considerable effort to document them. Recent reexcavation work at the site indicates that his maps, probably made by a combination of taping and pacing, are reasonably accurate (Mary Spall, personal communication 2004). By late the next year he was excavating the burial area at Fitzmaurice Ruin, one of the largest sites in the region that he later introduced to Byron Cummings and his students.

Simmons also excavated in Groom Creek south of Prescott where he found what he felt was a culture distinct from that to the north, which he called the "Black on Grey Culture." He characterized this "effigy culture" as having large and small sites with forts at strategic points, burials with sherds or a bowl over the skull, pottery of better quality than the black-on-gray sites, and most importantly, effigies, both human and animals, found in abundance (J.W. Simmons to E.W. Haury, letter, 8 February 1935, ASM A-53). A student of Emil Haury wrote his master's thesis on Simmons' figurine collection in the 1950s (Scott 1958, 1960). Until recently archaeologists have not found such a distinction useful, although none have been as familiar with the Groom Creek area as Simmons. On the basis of recent excavations, Motsinger (2000) has revived Simmons' idea that there may have been a subculture that coexisted with the dominant Prescott culture.

Beginning with Fewkes and Harrington at the Smithsonian, Simmons soon developed extensive correspondence with professionals about his work, including A. V. Kidder, H. S. Gladwin, E. A. Hooton, Irwin Hayden, F. H. H. Roberts, Emil Haury, Odd Halseth, L. L. Hargrave, C. A. Amsden, Omar Turney(?), E. H. Morris, C. B. Cosgrove, E. W. Gifford, H. S. Colton, and Byron Cummings. Some of this correspondence was frustrating. Simmons was being logical when he would send artifacts and descriptions of artifacts to "experts," but was naïve to think that they would have much more knowledge about them than he. He was rightly unhappy when he got information from the Chief of the Bureau of American Ethnology, Matthew W. Stirling, that the beads that he sent were an "...excellent example of the diverse types which are found associated with the different cultural remains in your section of the country!" (J.W. Simmons to H.S. Gladwin, letter, 21 February 1931, ASM A-20). Such a stock answer from an overworked government archaeologist was not what the inquiring mind of J. W. Simmons wanted. The experts didn't know the answers to many of his questions, but he needed some intellectual direction for his compulsive behavior.

Simmons correspondence also led to developing a research library. Fewkes sent him BAE publications. He also begged books from Roberts, Kidder, Gladwin, Colton, Cosgrove, and Amsden (J.W. Simmons to E.W. Haury, letter, 20 December 1950, ASM A-53). Near the end of his life, when he was on old age assistance, he sent Emil Haury a money order for $15 for a copy of the Ventana Cave report, more money than he had made in the previous two years (J.W. Simmons to E.W. Haury, letter, 20 December 1950, ASM A-53)!

At the time Simmons moved to Prescott. Except for a brief foray by Fewkes (1913) in the early part
of the century and more recently by Gila Pueblo personnel, there had been no professional interest in the archaeology of Yavapai County outside of Verde Valley. Simmons moved to change that. As he noted in a letter to Kidder (14 March 1931, ASM A-49), "The Black on Grey field I consider of such importance, that a trained man really ought to cover it thoroughly. Expeditions don't come here because you can't back up a truck to a ruin & fill it with contrasty pottery & mummies." He was aware of the traffic in specimens that was going on in the region, much of it from National Forest lands. He says that he wrote the Department of the Interior about the issue, but nothing happened. Of course he also excavated on Federal lands, but he said, "I feel justified in violating the law, when my digging prevents the graves falling into the hands of the professional vandals" (J.W. Simmons to A.V. Kidder, letter, 28 March 1931, ASM A-49).

In mid-1931, when he had the self-assurance to list himself in the city directory as an archaeologist (Figure 2), Simmons tried to get archaeologists interested in his finds. He first sent materials to Byron Cummings, who failed to answer. Next he contacted Harold Gladwin, an amateur himself, who showed interest and encouraged Simmons, but who was busy with publications. He finally hit the jackpot with A. V. Kidder, the most prestigious archaeologist in the Southwest, who was not only excited about Simmons' black-on-gray pottery but was interested enough to come to Prescott and see the sites that Simmons had found.1

Kidder's arrival in town was headline news in the Prescott paper, which was impressed by Simmons and his work (Prescott Evening Courier, July 24, 1931, p. 1):

Dr. Alfred Vincent Kidder, archaeologist of international repute, whose interests are wrapped up in the dim past of the southwestern Indian, was due to arrive in Prescott on the 6 o'clock train tonight from Flagstaff for a brief visit with J. W. Simmons of 302 South Granite street.

While Mr. Simmons chooses to call himself "a laboring man"—at present he runs a concrete machine at the new federal building—his hobby is archaeology, dating back a half dozen years.

And yet it is natural to assume that Mr. Simmons must have something of real interest to attract one of the recognized leading archaeologist of the country to Prescott or else he would not come here.

Those who have been privileged to see Mr. Simmons collection of archaeological articles and glance at his field notes understand at once why Dr. Kidder is coming to Prescott. For in Mr. Simmons' "bachelor" apartment at the Granite street address, where three other old bachelors live there is box after box of material gathered painstakingly from all over Arizona that has genuine interest for even the layman, let alone the man whose life work pivots around excavations and the remnants of dead civilizations...

Mr. Simmons anticipates a visit of three or four days in order to enable the distinguished scientist to go through Mr. Simmons' collections, peruse his field notes, and to visit some of the Indian ruins in this locality with the idea of enhancing Dr. Kidder's knowledge in his chosen field and later possibly to collaborate with Mr.
Simmons in the publication of scientific paper on what has been unearthed by Mr. Simmons, who has developed an unmistakable “scientific urge,” intelligently directed.

Kidder's diary (July 24-25, 1931, courtesy of Douglas Givens) is more terse where he says, “Looked up Simmons at his shack on a side street, but he was out. He came up a little later and deluged me with talk.” The following day Kidder, Simmons, and his digging partner, L. J. Fuller, went south of town to the Groom Creek area to visit the sites that had been producing figurines. Although Kidder describes Fuller, he gives nary a glimpse of Simmons. The next day Kidder and his wife and daughter went with Simmons on a tour of Fitzmaurice Ruin, Coyote Ruin, Granite Dells, and a ruin in Chino Valley (Simmons n.d. d; J.W. Simmons to H.S. Gladwin, letter, 6 February 1933, ASM A VO).

At the end of his visit the local paper interviewed Kidder and reported (Prescott Evening Courier July 27, 1931):

The internationally recognized scientist paid Mr. Simmons, an amateur collector of ancient specimens of life and student of archaeology a high compliment in saying merely, 'He's very thorough, unusually so.'

He also said that he intended to return to Prescott next year to continue his work.

BYRON CUMMINGS AND ARIZONA ARCHAEOLOGY

As was common at the time, once archaeologists began work in a region that was recognized by their peers, they were considered to have proprietary interest in the region and it was considered good form to request their permission before doing work there. When A. V. Kidder wanted to do fieldwork in northeastern Arizona he first contacted Byron Cummings, who had begun work in the area several years earlier (Kidder and Guernsey 1919:13). When he came to Arizona from Utah in 1915, Byron Cummings was the first resident archaeologist in the state. Once he moved to the state and became director of the state museum Cummings began a process of empire building, perhaps modeled upon what Edgar Lee Hewett had done in New Mexico. From a collection of primarily bird skins, he developed the Arizona State Museum into an important regional institution, created a Department of Archaeology as a major teaching and training center, and founded the Arizona Archaeological and Historical Society as a public outreach organization of the Museum (Bostwick 2003:207-209; Haury 2004).

Prior to Cummings' arrival, most artifact collections made in the state, including those of Kidder, had gone to museums in the east. Cummings' bête noire was eastern museums coming to the state and taking archaeological collections away (Cummings 1936; Johnston 1966:43). In 1927, he attempted to formalize his control of Arizona archaeology by getting the Arizona legislature to pass the Arizona Antiquities Act, that required every archaeologist working on state or federal lands to obtain a permit from the State Museum and for half of the collections to be housed in Arizona public museums (Anonymous 1927; Bostwick 2003: Chapter 10; Wilcox 1988:22-23). Although created in part to reduce pothunting, the law was also a reaction to Fewkes' excavations at Elden Pueblo (Anonymous 1927). Although the Federal portion of the law was judged to be unconstitutional, some out-of-state archaeologists were still intimidated from doing work on Federal lands in the state (Wilcox 1988:23).

Simmons had the good fortune to catch Kidder at a time when he was again making motions to return to the Southwest. However, he would not have been aware of the hostility with which Kidder was viewed by Byron Cummings. Antagonism between Cummings and Kidder
came early in their archaeological careers. In 1908, just graduated with a B.A. from Harvard, Kidder was put in charge of excavations at Alkali Ridge in southern Utah by Hewett, when Cummings’ thought that he was in charge. To ease what must have been a tense situation, Kidder agreed to be co-director with Cummings (Bostwick 2003:108). The preliminary report on the work was Kidder’s first archaeological publication (Kidder 1910), the end of which promises that a report on the collections would be done once he had an opportunity to study them at Cummings’ museum in Salt Lake City. No further publication on the work was ever done. It seems probably that this season in the field together provided the initial basis for future tensions.

As noted above, Kidder contacted Cummings prior to the start of his work in the Kayenta area and Cummings provided ideas about where the best sites were. A decade later, when Kidder published his Introduction to Southwestern Archaeology he gave a little prod to Cummings, who had a habit of not publishing his work, when he said, “publication of [Cummings‘] notes will throw much light on many obscure problems” (Kidder 1924:69n). Kidder visited Cummings in the field in 1923 (W.M. Walker, diary, 31 July 1923, ASM) and we can surmise that he was not impressed by Cummings’ digging techniques.

In 1928, Kidder had been looking for a site in Arizona “pine country” to excavate (A.V. Kidder to H.S. Colton, letter, 27 November 1928, MNA). He had completed work at Pecos Pueblo and although becoming increasingly involved in Mayan archaeology through his employer, the Carnegie Institution of Washington, he still managed to get back to the Southwest nearly every summer (Woodbury 1973:67-68). Harold Colton felt that Cummings had a persecution complex with Kidder as the center and was anxious to get the two together. Kidder was troubled by what Colton had said and wondered whether it would not be better to simply keep out of Arizona (H.S. Colton to A.V. Kidder, 5 December 1928, Museum of Northern Arizona, Flagstaff, Colton Archives MS 207 [MNA]; Kidder 1928). That same year, Cummings had to terminate his dig at Turkey Hill because he did not receive a Federal Antiquities Permit. The diary of one of his field assistants says, “Looks like dirty work on part of Nusbaum and Kidder” (W.M. Walker, copy of Arizona Journal, Summer, 26 July 1928, ASM). Colton felt that Cummings’ claims of conspiracy may have had some basis (H.S. Colton to A.V. Kidder, letter, 15 December 1928, MNA).

SIMMONS ATTENDS UNIVERSITY OF ARIZONA FIELD CLASS

Simmons had already contacted Cummings in early 1931 about his work at Fitzmaurice Ruin, but Cummings claimed not to have received the letter (J.W. Simmons to H.S. Gladwin, letter, 6 February 1933, ASM A-20). After Kidder’s visit to Prescott he had a chance to talk with Cummings and suggested that he see Simmons’ collection. Simmons wrote a letter to Cummings laying out the situation as he saw it and argued that Kidder’s presence and money (Kidder had $100 available to support Simmons’ digging) would be invaluable in understanding the archaeology of the region (J.W. Simmons to B. Cummings, letter, 3 September 1931, ASM A-402). At this point Cummings apparently succumbed to the pressure being put on him and came to town. Prior to his visit an Archaeological Committee had been created consisting of prominent Prescottonians, which along with Simmons included Charles Elrod, Sharlot Hall, Kate Cory, Alpheus Favour, Lester Ruffner, and Grace Sparkes. Cummings offered the committee $50 per month to support Simmons work for 6 months if they would match it (B. Cummings to G. Sparkes, letter, 8 October 1931, ASM A-402). The possibility of the Smoki People, a local fraternal organization and amateur anthropology
The Black on Grey Culture of Western Yavapai County....

Unearthing of this interesting material has awakened scientists and archaeologists to the fact that the Black on Grey culture is a distinct local culture and is not related to any other in the Southwest

By J. W. SIMMONS
of Prescott, Arizona

Figure 3
Title of Simmons' Only Publication, 1931.

In late October 1931, Simmons went north of Prescott to Big Chino Wash to select a suitable site for excavation and to obtain pledges from local ranchers to protect sites and allow exclusive right to excavate and study ruins on their property. Recovered artifacts would go to the Smoki Public Museum (J.W. Simmons, field survey for Smoki Museum, 1931, ASM AA-25). The site Simmons selected was one on the King Brother's Ranch in Big Chino Wash, close to 30 miles north of Prescott. Visible on the surface were an eleven-room pueblo, a wattle house, and a stone outline, located in an artifact scatter covering two acres (Simmons n.d. a:4). The pueblo mound had been previously dug in by an artifact collector.

The agreement was that the Archaeological Committee would provide Simmons with $100/month for six months, but amazingly enough he was to provide all food, transportation, equipment, film, etc. He also ended up providing board
for a “general cleanup man” for five days and for L. J. Fuller, his digging friend and volunteer. By his own reckoning, he received less than $30/month above expenses (J.W. Simmons, ASM A-20)!

In the last two months of 1931, sometimes only with his dog as a companion, Simmons excavated in the burial area of the ruin. He ran trenches across the area northeast of the pueblo mound that he either knew or assumed would contain burials (Simmons n.d. a:7). The work began with pick and shovel with a change to trowel and broom once something was encountered. Screening was done when beads and other small finds were present. The notes we have on this early work at the site are typed and are probably based upon handwritten notes Simmons kept while working.

Simmons says that he found 17 burials in about 30 days of work, although his notes have descriptions of only the ones with significant grave goods. He apparently did not number the burials, which is unusual for him. For a little over a week in December, he was stranded at the ruin because of deep snow, but was able to continue excavating by clearing the snow in areas for trenching and by excavating as much as possible before nightfall. Concentrated in the north part of the burial area, Simmons found double interments, a triple one, a child burial with a unique pottery “bear fetish” (Spicer 1936:Plate XI; called an otter in the current Smoki Museum display), and an adult burial with a shell mosaic frog, copper carbonate-stained jaw, and a large quantity of beads. His interpretations of the remains are minimal and his notes focus on describing what he found. Simmons spent the winter, unpaid, repairing pottery, stringing beads, and typing field notes (Simmons n.d. d:17-18).

Byron Cummings had to wait until school was out to begin work at the site. In June, when Cummings appeared with eleven students and a field director Simmons’ nightmare began, to quote him, they “went thru that site like the German Army through Belgium” (J.W. Simmons to H.S. Gladwin, letter, 6 February 1933, ASM A-20). We might put such comments aside as being from someone who had just enough knowledge to see everyone else in a bad light. But we should also remember that Simmons was used to working alone, or sometimes with a friend, had some very definite opinions about archaeology, and took what he was doing very seriously. Although Byron Cummings had much more archaeological experience than Simmons, that did not make him a better archaeologist. Simmons made efforts to get the archaeological committee to “see if the Dean could not be toned down a trifle” (J.W. Simmons to H.S. Gladwin, letter, 6 February 1933, ASM A-20), but things had gone too far by then.

Once Cummings arrived on the site, Simmons was no longer in charge and he had to work together with the university crew. Included in the crew were a number of men who would become known in anthropology: students, Edward H. Spicer, Gordon C. Baldwin (see photo of both in Smith 1983:76), Louis R. Caywood, Earl Jackson and H. Claiborne (Clay) Lockett; former student, Ben Wetherill as field director; and new anthropology instructor at the University, John H. Provinse. Only the last two had much experience in excavation.

There was a fairly well marked division of labor at the site that summer: Simmons was to excavate burials and Cummings’ students would work on the mound, with occasional forays elsewhere. It is curious that Cummings, who was an experienced burial digger and who was primarily excavating the site to obtain artifacts for the Smoki Museum, would give the burial excavation to Simmons. Perhaps because Simmons had already started the cemetery, it was most efficient to leave him there (as well as keep him occupied at a distance from the main crew). Perhaps he saw the pueblo mound as a more appropriate place for his students to work.
Regardless, Simmons spent the next month or so trenching pretty much by himself, with some help from Fuller. Of course, because of the spectacular things that he sometimes found, there were plenty of visits from Cummings and the students. On the other hand, Simmons says that his visits to the Cummings dig were rare and that the main ways he knew what was happening elsewhere on the site was from students’ conversations or when he saw Cummings carry artifacts by the burial area (Simmons n.d. d: 14, 16). His attempts to make helpful suggestions about how to dig carefully and efficiently were met with disdain, although he seems to have found in Provine a sympathetic listener.

This interchange with professionals and professionals-to-be was an important one for Simmons, as limited as it might have been, for it gave him a chance to hear other interpretations of archaeological remains and to have his interpretations subject to scrutiny. One clear effect of this contact on Simmons’ archaeological work was increased details in his field notes including information on things like the sex of skeletons. One training method, if that is the proper term, Cummings used in the excavations he ran was to put students to work digging, and when they found something they would call him over and he would show them how to excavate it. After dinner there would be discussion of the day’s work as well as accounts by Cummings of his travels (Chidester 1986). Such a situation was probably good for Simmons no matter how much he railed about the poor job done.

Cummings sometimes chose to dig a burial or had his students do so, creating some friction with Simmons, who, seeing how Cummings operated, did not want anything removed before he had a chance to record it. As an example, attached to his typed notes of 24 lines on a burial is a piece of paper with Cummings’ handwritten notes on the same one that says, “No. 7. Nothing below right or left shoulder. Bunch of sherds at R. shoulder. Unbroken bowl in N.E. corner. Small bowl in S. E. corner. Skeleton undressed” (Simmons n.d. f:22).

The curious term “undress” or “undressed” appears in notes on skeletons and apparently referred to excavating and removing the burial goods.

Simmons continued trenching in the area northeast of the pueblo mound and numbered the burials from 1 to 40. As multiple bodies in the same pit got the same number, child burials were numbered separately, and the burials found in the pueblo area were not included in his count. The exact number of individuals found in 1932 can only be estimated. Of around 55 that appear in the records, Simmons seems to have dug at least 44.

In his excavations Simmons found a number of rich and unusual burials. Spicer, in his thesis and publication on the site chose to talk in generalities about the burials and grave goods, so Simmons’ notes provide important information on mortuary behavior in the Prescott area that is not available elsewhere. In a discussion of the burials done in July 1932, Simmons summarizes some of the differences he observed between the south burial area and the north burial area, separated by around 40 feet. Table 1 lists the significant differences, which appear to be related to temporal difference between the burials in the two areas. The older southern burials had mostly pottery as offerings and most of that was broken. Burials often had fibrous bark-looking material on or under the burial, sometimes as thick pillows under the head. Several pole covered burials were found in this area. The younger northern burials had a greater diversity of goods including jewelry, “pahos” [painted sticks], bone flutes, and the skeletons were commonly wrapped in matting. Burial pits were often reused features.

Simmons did not speculate about the meaning of these differences that appear to represent a change in mortuary behavior through the occupation of the site that might have been 100 years or more. No mention is made of this spatial/temporal patterning of mortuary behavior in Spicer’s work.
While Simmons was working in the burial area, the university students were concentrating on the mound that contained the rubble of a masonry pueblo. The few wood samples removed from one of the rooms were initially dated by dendrochronology to the mid-eleventh century, but have since been revised to 1204 (Bannister et al. 1966:15-16). The Cummings crew was very selective in what they saved and large quantities of sherds were left on the open ground or in rooms (J.W. Simmons to H.S. Gladwin, letter, 19 December 1932, ASM A-20). Simmons, on the other hand, saved all of the pottery found with burials and wrapped them in bundles for delivery to the Smoki Museum (J.W. Simmons to H.S. Gladwin, letter, 7 January 1933, ASM A-20). Unfortunately, the bulk of these sherds were discarded at some point.

The university crew stayed less than a month and one of Simmons’ complaints was that Cummings was not present all of the time. Simmons worked until early August and then, one month before the end of his agreed term of employment, was told that no more digging would be done (J.W. Simmons to H.S. Gladwin, letter, 19 December 1932 and 6 February 1933, ASM A-20). He felt that this dismissal was related to his refusal to move into the Smoki’s museum with his collection (Simmons n.d. d:21). In the end, Simmons blamed the archaeological committee, rather than Cummings, for the hasty job done on the ruin. Simmons felt that Cummings was pressured into putting his students to work on the ruins when he was planning to spend all of the summer at Kinishba (Simmons n.d. d:24).

At some point Edward Spicer chose to do his master’s thesis on the excavations at the site. This thesis, signed by Byron Cummings in May 1933 (Spicer 1933), is the first formal write-up of the excavation of a Prescott site. It contains the first detailed discussion of a type that Gladwin had named Verde Black-on-grey, but that Spicer felt was more appropriately named Prescott Black-on-grey. Simmons is mentioned three times, although his article where the Black on Grey Culture is first named, was not cited. Also, Spicer made no significant use of Simmons burial data, although they provide important information on the site.

Spicer’s thesis, as known to most through its published version (Spicer 1936) is famous, one might

<table>
<thead>
<tr>
<th>Table 1</th>
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<tr>
<td><strong>King’s Ruin Burial Summary.</strong></td>
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</tbody>
</table>

<table>
<thead>
<tr>
<th>Trait</th>
<th>South End</th>
<th>North End</th>
</tr>
</thead>
<tbody>
<tr>
<td>Date</td>
<td>Early</td>
<td>Late</td>
</tr>
<tr>
<td>No. of adults</td>
<td>13</td>
<td>17</td>
</tr>
<tr>
<td>Pole covering</td>
<td>present</td>
<td>absent</td>
</tr>
<tr>
<td>Burial wrappings</td>
<td>thick fibrous mass</td>
<td>matting</td>
</tr>
<tr>
<td>Stone and Shell Beads</td>
<td>rare</td>
<td>abundant</td>
</tr>
<tr>
<td>Turquoise</td>
<td>rare</td>
<td>abundant</td>
</tr>
<tr>
<td>Pahos</td>
<td>absent</td>
<td>present</td>
</tr>
<tr>
<td>Bowls</td>
<td>broken and used to line walls</td>
<td>nested, resting on sides</td>
</tr>
<tr>
<td>Minerals</td>
<td>rare</td>
<td>azurite &amp; hematite present</td>
</tr>
<tr>
<td>Bone flutes</td>
<td>absent</td>
<td>2 sets</td>
</tr>
<tr>
<td>Burials in previously used features</td>
<td>absent</td>
<td>present</td>
</tr>
</tbody>
</table>
say notorious, for comments on the quality of the Prescott Black-on-gray designs (see Figure 4) and the character of their makers:

It may be safely said that the King's Ruin potters did not understand design. They were not only incapable of handling a paint brush with accuracy and skill, but they also seemed to have had no sense of symmetry. ...This characteristic disregard of symmetry and failure of the imagination, together with the consistent carelessness of execution of the simplest lines or even dots, indicates a total lack of artistic sense. The idea grows on one who examines much Black-on-grey pottery that it is the product of childish minds, lacking in patience, skill, and constructive imagination (Spicer 1933:42-43).

The last sentence was deleted from the published version of the paper, as was the entire concluding section of his thesis that included additional dubious statements such as, “...the people of King's Ruin, although evidently in contact with the higher art of the Pueblos, had a fundamental defect in their nature that made them incapable of assimilating that art” (Spicer 1933:119). Such judgmental statements come early in the career of an anthropologist who went on to be known for his humanism (Gallaher 1984).

Later in 1932 Simmons invited Harold Gladwin to come to Prescott and as a result of this visit he began to work for Gila Pueblo by collecting sherds from sites not covered in the original Gila Pueblo survey of the region (H.S. Gladwin to J.W. Simmons, letter, 10 November 1932, ASM A-20; Gladwin and Gladwin 1930; J.W. Simmons to H.S. Gladwin, letter, 15 November and 19 December 1932, ASM A-20). Apparently a payment of a dollar per site was suggested, although Simmons pointed out that this was only reasonable in fairly high density areas close to a road (J.W. Simmons to H.S. Gladwin, letter, 6 February 1933, ASM A-20).

In the early summer of 1933 Simmons arranged to have his collection exhibited for two weeks in the basement of the Prescott studio of his artist friend Ethel M. (Pokey) Young. For 15 cents you could see his collection of carefully arranged relics, most from Yavapai County (J.W. Simmons, undated clipping, ASM A-20). Apparently few people took advantage of this opportunity (J.W. Simmons to H.S. Gladwin, letter, 13 July 1933, ASM A-20). As he gave the studio as his mailing address, he may have lived there as well. He continued writing up his work at Fitzmaurice Ruin and had Young do some superb drawings of painted ceramics from his digs (Figure 4). Later in the summer he went with Emil Haury, then of Gila Pueblo, to excavate in the San Francisco Valley of New Mexico. Nothing is known about his experiences on this dig, but it did create a lasting friendship with Haury, who took over Byron Cummings' position as head of the Department of Archaeology in 1937 and as director of the Arizona State Museum the following year.

Perhaps this period in the field with Haury initiated discussion about the proper home for Simmons' collection. Anyway, later that year he negotiated its sale to Gila Pueblo, including most of what he had accumulated up to that time --arti-
facts, notes, and negatives — except the King’s Ruin materials that had gone to the Smoki Museum. In one list sent to Gladwin he gave a count of 139 burials from 11 sites that he had excavated, including 57 from King’s Ruin (J.W. Simmons to H.S. Gladwin, letter, 22 September 1933, ASM A-20). The count was for the 40 mile diameter area around Prescott and did not include his numerous excavations in the Verde Valley and Flagstaff area.

It is unclear why Simmons sold his collection. Certainly money was always a necessity and he wanted to continue his archaeological work, which required money for gas, auto repairs, and minimal subsistence. He was also having health problems and wanted to find a home for it, although he had made arrangements about the disposal of his collection with his friends Fuller and Young if he was unable to do so himself (J.W. Simmons to H.S. Gladwin, letter, 22 September 1933, ASM A-20). Receiving only $500 for a collection that cost much more to make, Simmons was still poor, but felt that the collection was in good hands. He was mistaken. There is little evidence that Gladwin had a serious interest in the Prescott area, although he does say in one letter that he would have Haury choose a site in the area to excavate at the “earliest opportunity” (H.S. Gladwin to J.W. Simmons, letter, 26 September 1933, ASM A-20). That opportunity never came.

Simmons had hoped that his collections, and Pokey Young’s drawings, might be used in one of the series on Southwestern pottery that Gila Pueblo was publishing. The only evidence that Simmons collection got significant use is a letter from Gladwin to Simmons where he indicated that Simmons’ sherd collections had been mounted on boards and that Kidder had gone over them “with great interest” (H.S. Gladwin to J.W. Simmons, letter, 13 December 1933, ASM A-20). As a result of this review Gladwin and Gladwin (1934:Figure 1) suggested a three phase system for the Prescott area — Yavapai (BM III-PI), Prescott (PII), and Chino (PIII). The latter two phases are still used. When Gila Pueblo was dissolved, all of its collections went to the Arizona State Museum. Unfortunately, between the time Simmons transferred his collection to Gila Pueblo and the time the Gila Pueblo collections showed up at the State Museum, much of it was gone. There are rumors of a culling process that occurred at some point and Simmons collection is clear evidence of it.

Although Gladwin claimed that he paid more for the collection than Simmons could have gotten elsewhere (H.S. Gladwin to J.W. Simmons, letter, 14 October 1933, ASM A-20), it is not unreasonable to suggest that Simmons would have been better off to sell his collection on the antiquities market. There were certainly collectors in northern Arizona who had the money and he probably would have gotten more for the cream of the collection (i.e., whole pots), while holding onto the bulk of it and the notes, that had no cash, but major scientific, value. Simmons makes it clear in his letters, however, that he was not after money and wasn’t interested in selling to people such as Dr. Hartzell of Prescott who encouraged the pothunting market (J.W. Simmons to H.S. Gladwin, letter, 6 February 1933, ASM A-20).

In 1933, the University of Arizona returned to the Prescott area to excavate at Fitzmaurice Ruin, again following in Simmons footsteps. In the winter of 1930-31, Simmons had excavated 43 burials at the site, keeping notes and taking photographs. The University crew excavated five structures, with Byron Cummings outlining the plan of work and Louis Caywood, Edward Spicer, and another student carrying it out (Caywood 1936). Simmons’ work in the burial ground received one sentence in the report.
SIMMONS MOVES TO PHOENIX

Although the sale of his collection was in many ways traumatic, it did give Simmons the opportunity to make a change. At the beginning of 1934, Pokey Young, in whose studio/store he had apparently lived for at least part of 1933, died (Hall 1934; Prescott Courier, Jan. 2, 1934). At some point that year he moved to Phoenix, although he is listed in the 1935 Prescott City Directory. He had already done much archaeological work in the Phoenix region and now committed himself to the area whole hog, although he continued to maintain an interest in the Prescott region and northward (his mid-30s excavations in Walnut Canyon have never been noted in the frequent historical overviews of the area). To keep expenses at a minimum he found a friend who let him stay in his basement (L.C. Bolles to O. Halseth, letter, 24 June 1934, Pueblo Grande Museum, Phoenix; J.W. Simmons to H.S. Gladwin, letter, 3 April 1934, ASM A-20).

The Public Works Administration came to the rescue in December by funding excavations at Pueblo Grande. Odd Halseth, Phoenix City Archaeologist, received the grant and chose Simmons to be field supervisor, specifically to provide a better standard of excavation and reporting than done in prior work at the site (Downum 1993:153-154). This selection indicates that Simmons had a reputation for doing good work. The job was a mixed blessing for Simmons for although it provided desperately needed income and gave him a chance to do what he loved best, he was forced to work with Halseth, who he detested (Simmons 1949b), and had to run a fairly large, inexperienced crew. As the main focus of excavation was clearing the platform mound for public display, emphasis was upon quantity of dirt moved, rather than quality of information recorded (J.W. Simmons to E.W. Haury, letter, 8 May 1949, ASM A-53). His notes and reports indicate that he took the careful excavation and accurate recording of all archaeological information very seriously (Downum 1993:154, 159). In May of 1935 when the project ended, he was unemployed again.

Although Simmons was pretty much done with Prescott as a home, a project that he had an early role in was completed the same month. Construction of the Smoki Pueblo building had started in January 1931. Although built primarily for meetings and costume storage of the Smoki People, there was also an interest in using the building as a museum. Museums and tourism were viewed as ways of bringing people, and money, to Yavapai County and Cummings’ interest in the archaeology of the area revolved primarily around the possibility of starting a branch museum in Prescott. The Smoki were already putting effort into a museum in 1931 when Simmons was paid $40 to make collections. He was invited to move himself and his collection to the pueblo but he declined because it was “uninhabitable.” He felt that the idea of a distinct Smoki Museum occurred after the Smoki People saw his collection (Simmons 1933b). For a short period in 1933 one of Cummings’ students, Jean McWhirt (later Pinkley), served as curator/tour guide (Prescott Evening Courier, June 8, 1933, p. 1).

The idea of a separate museum building finally led to the use of Federal ERA and CWA moneys to build it. The formal opening of the Smoki Public Museum in its own building was on May 29, 1935, with Byron Cummings giving an address and Louis Caywood doing a slide show on Tuzigoot. Although his collections were a focal point of the Museum, there is no evidence that Simmons attended this event or that his work was acknowledged. In fact, when Grace M. Sparkes, who was on the same archaeological committee as Simmons and who was probably responsible for getting his article published in Yavapai Magazine, synopsized the history of the museum as follows, “seven years ago [sic; actually only four] the archaeological committee placed two young graduate archaeologists, Messrs. Spicer and Caywood, of the University of Arizona, in the field to under-
take research, scientific excavation and general museum work” (Sparkes n.d.:24), she was actually describing what Simmons was hired to do and did! Sparkes’ paper began the tradition of erroneously giving the University of Arizona credit for initiating archaeological excavation in the Prescott area. Until this paper, incorrect credit has been given to Spicer and Caywood (Euler 1958:107; Smith 1983:75; Stone 1987:47), Spicer (Motsinger et al. 2000:1), Cummings (Fish and Fish 1977:33; Harper 1998:1; Macnider et al. 1989:109), or Spicer and Cummings (Whittlesey 1997:67). Only recently has some attention been given to Simmons work in the area. Alan Ferg gave a talk on Simmons at the 1996 Prescott Conference and at the same conference Motsinger (2000) revived Simmons idea of a Groom Creek culture.

For a short period when the Smoki Public Museum first opened Byron Cummings’ son Malcolm lived in the building as curator and conducted archaeological work in the area (M. Cummings 1936). Simmons felt that Malcolm was reaping the benefits of what he (Simmons) had sowed (J.W. Simmons to E.W. Haury, letter, 8 February 1935, ASM A-53). By November 1935, Simmons was writing Kidder that his typewriter ribbon was shredded, he had no money to buy glue or plaster for restoring specimens, and his camera was in the pawnshop. “HELL to be poor” he lamented (J.W. Simmons to A.V. Kidder, letter, 22 November 1935, ASM A-49). When reading Simmons’ letters, one sometimes gets the impression that his excitement in putting his ideas down on paper often overwhelmed the issues of spelling and paragraph construction. But in a calmer mood, he could write quite adequately. Less than a year after finishing with Pueblo Grande, and just at the end of his period on PWA relief, Simmons unexpectedly found himself working as a writer for the WPA Federal Writers’ Project. Although the project was principally for out-of-work writers, archaeologists were one category of professional workers needed (Mangione 1983:47).

The Project’s main task was to produce the Arizona edition of the American Guide Series, that provided readers with background on landscape, history, and commerce, followed by tours along the principal roads of the state (Federal Writers’ Project 1940). Simmons was first assigned to describe archaeological points of interest in the Phoenix area (J.W. Simmons to E.W. Haury, letter, 5 March 1935, ASM A-53). Later his scope was expanded northward. A few of these studies were condensed by editors for use in the guide (J.W. Simmons to H.S. Gladwin, letter, 16 October 1936, ASM A-20). Apparently as part of his Writers’ Project work he did a survey of the Mt. Floyd-Round Mountain area north of Seligman in 1938 and recorded 43 sites (J.W. Simmons to E.W. Haury, letter, 22 March 1949, ASM A-53).

In all, between early 1936 and early 1937, Simmons produced about 50 reports based upon sites that he recorded or at least visited and over a dozen based upon previously published materials. The range of sites indicate the breadth and depth of his knowledge of central Arizona archaeology: Elden Pueblo, Groom Creek Culture, King Pueblo (i.e., Kings Ruin), the Cashion sites, Pueblo Grande, Fort Mountain, Grasshopper Ruins, Baby Canyon Ruin, Chevelon Ruin, Gila Bend Fort, and so on. At this time, he vied with Byron Cummings and Harold Gladwin as having the most personal knowledge of sites in that part of the state. The guide was published without any indication of who the writers were or even who the director of the program was (Ross Santee), so the extensive research done by Simmons is another example where his work is invisible.

Simmons’ friendship with Frank Midvale was strong. He may have met Midvale (then Mittvalsky) and his brother in Flagstaff in 1926, and was working with Midvale, presumably for
pay, at La Ciudad in January 1927 (Wilcox 1987:30). Twenty-eight years younger than Simmons, Midvale started as a collector and struggled to gain an education and make it in archaeology. Simmons and Midvale were kindred souls - fascinated by anything archaeological, always excited about what they were doing, and surprised that all others weren't equally so - going through their lives more often frustrated than not and unrecognized for the good work that they did. An inscription written by Frank Midvale in a book given to Simmons sums the situation up admirably:

To a real Archaeologist, untutored but sincere, who has labored unmindful of personal expense or comforts, devoting his efforts and limited means purely for the love of his work. More than one Project of Lasting Benefit to Arizona was conceived and accomplished by this man and then in near-completion seized by Institutions whose leaders assumed fully the credit (Wilcox 1987:i).

Midvale worked for years on the site of La Ciudad, east of downtown Phoenix, and Simmons was often there to help. Midvale owned a piece of property on the site and from 1937 until his death, Simmons lived in a small house located there (Figure 5).

Compared to the Prescott area, Simmons’ work in the Hohokam area has received somewhat more recognition because of several CRM projects having a strong archival component. His work at Pueblo Grande (Downum 1993), La Ciudad (Wilcox 1987:51), Casa Buena (Wilcox 1984; Wilcox and Howard 1988), and in the Gila Bend area (McGuire 1982:116) has been recognized and commented upon favorably.

Figure 5
J. W. Simmons Excavating at La Ciudad, 1951. Note screened dirt piles on either side of and to rear of Simmons. The caption to this photo says “The little shack, 12 x 10 feet, houses my collection. My living quarters are a building to the rear of it.”Courtesy of Arizona State Museum Archives, University of Arizona, Tucson.
“SOME SMALL TOWN IN THE GILA-SALT BASIN”

At least as early as 1928, Frank Midvale had worked in the Buckeye Valley about 25 miles west of Phoenix, where he recorded a number of sites that had relied upon canal irrigation (Antieau 1981:259; Midvale 1974). One of the largest of these sites was Van Liere which Midvale estimated was 100 acres in size with a ballcourt and 30 trash mounds (Midvale 1974:37). Simmons no doubt learned about the site from Midvale and beginning in the late 1930s excavated cremations and other features. Simmons’ typed notes on his work at the site are fairly detailed and are accompanied by a site map, photographs, and drawings. Claims in reference to the Van Liere work that Simmons was only collecting complete, decorated vessels because they were easier to sell (Christian 1988:12) is contradicted by Simmons’ notes and photographs that show screening for sherds and also document his intensive efforts to reconstruct vessels from sherds.

At Van Liere as at previous sites he had excavated, Simmons’ found amazing things, some never reported before. Carved stone images and vessels were common (see illustrations in Antieau 1981:321-327). One of the most intriguing finds was a set of pottery figurines that Simmons felt was a Hohokam family. Both Kidder and Haury were interested in doing something with Simmons’ manuscript on the site (J.W. Simmons to E.W. Haury, letter, 22 March 1949, ASM A-53), but nothing ever came of this.

By the early 1950s Simmons was winding down his quarter century of excavation and looking for a home for his collection. He wrote Haury that his collection would “go to some small town in the Gila-Salt Basin” (J.W. Simmons to E.W. Haury, letter, 20 December 1950, ASM A-53). Since he had sold his first collection to Gila Pueblo in 1933, he had added materials from a variety of places, but with the Van Liere artifacts being the most significant. In 1951, Simmons approached citizens in the town of Buckeye, six miles west of the Van Liere site, about the possibility of housing his collection in a museum (Parkman 1951). In a letter to Haury, Simmons suggested that as the University of Arizona was swamped with Hohokan specimens, a museum in Buckeye located on a major east-west highway would be a good place for the collection (J.W. Simmons to E.W. Haury, letter, 11 May 1951, ASM A-53). In August, the Buckeye and West Gila Valley Old Settlers Union bought Simmons’ collection for $1.00, agreed to house and display the collection suitably, and provide a place for Simmons to live and act as curator of the collection (Contract of Sale, August 14, 1951, copy at Buckeye Valley Museum).

Simmons transferred to Buckeye much of his Van Liere collection and some photographs as well as materials from elsewhere in the state. Strangely, he gave most of the palettes, some other artifacts, and the field notes from the site to Thomas Hinton, a Tucson anthropology student to “dispose of as he pleased.” Hinton chose to donate the collection to the Department of Anthropology at UCLA (Meighan n.d.), where he went to graduate school. Simmons collection was at first displayed in March 1953 in a storefront window and by the end of the year 700 people from 21 states had visited (Parkman 1953). It was moved to a separate building in the following year and as part of the fiftieth anniversary of the museum in that building a case devoted to Simmons and his work at Van Liere has recently been prepared. There is no evidence that Simmons ever got to see the display of his collection. In his final year he was apparently seriously ill and spent his last days in a Phoenix hospital. He died on September 16, 1953 (obituaries at Buckeye Valley Museum).
J. W. SIMMONS — ARCHAEOLOGIST OR POTHUNTER?

If I had my life to live over again I'd probably make no great changes if it were possible with the exception of taking up archaeology.

(J.W. Simmons to E.W. Haury, letter, 8 February 1935, ASM A-53)

Early on Simmons claimed that he did not class himself as an archaeologist but as a student of archaeology (Simmons 1931:12, editor's explanation). “Archeology & photography are my hobbies” said Simmons (1931f), and he sometimes used the term "amateur." (J.W. Simmons to B. Cummings, letter, 3 September 1931, ASM A-402; J.W. Simmons to E.W. Haury, letter, 6 March 1935, ASM A-53). On the other hand, he also called himself a pothunter (Simmons 1931d) and several archaeologists did so as well. Lyndon Hargrave of the Museum of Northern Arizona wanted Simmons to write an article called “Confessions of a Pothunter” presumably for the Museum’s periodical Museum Notes (Simmons n.d. c:62; J.W. Simmons to E.W. Haury, letter, 11 February 1935, ASM A-53). But beginning in 1931, he listed himself in the Prescott City Directory as an archaeologist (Figure 3) and when he moved continued that designation in the Phoenix directory.

Simmons' work by himself on his own problems, viewed as pothunting by some, was at least partly a response to archaeologists not taking him seriously and allowing him to work with them. In the fall of 1935 in Flagstaff, he was employed intermittently in construction at $3 a day, sleeping under a tree much of the time. In looking for archaeological work, “Two outfits were asked for work and [I] stated my qualifications but was turned down by both [MNA and Beloit College]. So I will DIG any way, regardless of those that demand that a man should be a formally trained excavator... Information we want, and if we can not get it with a legitimate outfit, we will go and get it 'un-ethically' as the 'CLOSED SHOP' archaeologists call it.” (J.W. Simmons to E.W. Haury, letter, 26 October 1935, ASM A-53). Of course such behavior did not help his status with professionals and resulted his conducting his archaeological research outside the boundaries of what many archaeologists considered appropriate.

What attributes of an archaeologist did Simmons possess? These might be examined under the headings method, research problems, and results. In terms of methods he operated similarly to many of his archaeological contemporaries. As did his first archaeological “mentor” J. W. Fewkes as well as his nemesis Byron Cummings, he focused on burials. However, a number of instances of him excavating structures are present in his notes and Simmons was appalled with the way that Cummings and his students excavated and recorded the architecture at King's Ruin, so he was certainly aware of its importance.

He recorded his sites on maps, either USGS 15 min. quadrangles or sketch maps and in at least one case had a regional blueprint map made (Midvale Collection, ASU Library). The detailed measurements on some of his sketch maps indicated that they were done with a tape (Figure 6). On one site, a tripod with instrument is visible (Simmons n.d. b:20). He took photographs of his sites, and sometimes undertook major effort to get a good picture (A photograph at the Arizona State Museum shows his “dependents” [2 dogs] lying at the edge of a rock outline where extensive brush clearing had been done). He took field notes on sites, recording artifacts and features present and on occasions obtained Gila Pueblo or Museum of Northern Arizona site numbers. He wrote up both survey and excavation work with varying amounts of detail. Generally he numbered burials sequentially and provided information on age, sex, orientation, and accompaniments. He frequently excavated by trenching (Kings Ruin;
Simmons had several research issues that he was interested in. One of them, no doubt coming directly from Harold Gladwin's work, was determining the geographic extent of the black-on-gray culture (J.W. Simmons to H.S. Gladwin, letter, 6 April 1931, ASM A-20), a term that he originated (Simmons 1931), but which he later preferred to call the Yavapai Culture, after the county (Simmons n.d. c, n.d. e; J.W. Simmons to A.V. Kidder, letter, 14 March 1931, ASM A-49). He also was interested in the presence of pottery in central Arizona (Tusayan White Ware or Little Colorado White Ware) in contexts that appeared to indicate "invasion" of the "black-on-white people" into the region (J.W. Simmons to H.S. Gladwin, letter, 7 March 1931, ASM A-20). Another research interest, unusual for his time, was documentation of agricultural features at central Arizona sites. He guided soil scientist Guy R. Stewart to many of them, although his assistance is not acknowledged in the publication (Stewart 1940). Finally, he was interested in forts (Figure 6) and the communities that they may have protected (J.W. Simmons to H.S. Gladwin, letter, 6 February 1933, ASM A-20).

In terms of results, Simmons had an important role in the formation of both the Smoki Museum and the Buckeye Valley Museum and his collection forms the heart of the latter and has an important part of the archaeological displays in the former. Unfortunately, the museums and repositories that he chose to donate to were not always good to his collections, so that judgment of him as an archaeologist has been based not upon what he collected and documented, but what has survived in attenuated form. Fortunately, there are thousands of pages of notes and photographs or negatives that can help to put his surviving collections into context.

Regardless of his apparent low level of formal education, putting words to paper came naturally to Simmons and was almost obsessive in nature. When he was in the field, the evening would be
spent writing or typing up notes on the day’s work. These notes were then rewritten, sometimes several times. In the days before copying machines, anytime he wanted to send information to someone he had to pull out the typewriter and peck away. In early letters he complained about lacking the vocabulary necessary to describe his finds (J.W. Simmons to A.V. Kidder, letter, no date, ASM A-49) but in fact quickly picked up what he needed.

Except for his one article, Simmons’ approach to “publishing” was to send copies of his notes and observations, and sometimes artifacts, to professionals. This approach worked in getting Kidder at least briefly interested in the region, got Cummings to make a hit-and-run foray to the area and to put Prescott archaeology briefly on the map, and got Gladwin to spend a little time investigating the area. Perhaps most productive was the information that he gathered in the Red Lake and Mt. Floyd areas that led John C. McGregor (1951, 1967) to do work on the little-known Cohonina culture. From this standpoint, Simmons can be seen to have been successful because he got professionals involved in areas he was interested in. Of course, the negative aspects of getting other people involved in your area of research is that they may do things in ways you don’t like or draw conclusions that you disagree with. They may also fail to give you credit for your pioneering work, and this certainly happened with Simmons and the University of Arizona group. Simmons claimed not to be interested in receiving credit for his work, although his letters also have comments about the lack of such credit (see Christenson 2004 regarding his lack of credit for discovery of the famous Winona Meteorite), so his claimed indifference to such things should be viewed with skepticism.

As with any classification, the pothunter-archaeologist dichotomy can break down when applied to borderline individuals such as J. W. Simmons. Although having very strong characteristics of an archaeologist he fell down in the eyes of some in critical respects. Certainly, his failure to publish his findings was an important fault, although as noted above he made every effort to make his knowledge available to those he thought would be interested. When people like Hargrave and Amsden classified him as a pothunter, however, they were focusing primarily upon one characteristic, possession of a personal collection. The issue of the ethics of private ownership of archaeological materials is too complex to develop here but it is likely to have had its roots in the formation of public museums in the nineteenth century and the development of a profession of archaeology whose principal source of work at that time was in museums. It is a major leap, however, from issues of conflict of interest to the statement that “No individual has any moral right to ownership of prehistoric relics” (Halseth 1928:12).

Archaeology by its very nature requires collecting and most everybody interested in the past ultimately must collect (Wissler 1929), but Simmons found himself in a period when some archaeologist were taking private collection as a moral issue and were beginning to see a “fence” between themselves and collectors, to use Eli Lilly’s term, who made the decision to stop collecting after working with archaeologists for several years (Madison 1988:3). Certainly Simmons’ critics, Hargrave, Amsden and definitely Cummings, had the passion to collect, but they had one thing that Simmons didn’t through much of his life, an institutional umbrella to morally validate their work.

At times desperate for professional help and at times disdainful of professionals, for over 25 years, with limited funds, encouragement, and recognition, J. W. Simmons followed his self-created archaeological path. His legacy of archaeological records and collections is only in the early stages of being recognized and used.
ACKNOWLEDGEMENTS

The bulk of Simmons’ papers reside at the Arizona State Museum Archives, University of Arizona. I thank Alan Ferg for permission to quote from and to use photographs from these materials. Simmons’ Federal Writers’ Project reports are at the Arizona State Archives and on microfilm at the ASU Library Archives. I thank Todd Bostwick for copying many of these for me and for making available a few letters pertaining to Simmons at the Pueblo Grande Museum. Jerry Howard of the Mesa Southwest Museum, Mesa provided access to the Frank Midvale papers and gave permission to use the Midvale photograph. Michael Sullivan and Barbara Macnider of the Buckeye Valley Museum have been extremely helpful in my research on Simmons at the Van Liere site. The late Winslow Walker allowed me to copy his field diaries and place these in the Arizona State Museum Archives.

ENDNOTES

1 Kidder’s involvement with and support of amateur archaeologists is poorly known. His work with Hattie and Burton Cosgrove and ultimate hiring of them for work at the Peabody Museum is well-documented (Davis 1995:21; Woodbury 1993:25). As Chairman of the Committee on State Archaeological Surveys, he was in a key position to correspond with amateurs and I have recently documented his important influence on George Langford in Illinois (Christenson 2003). Simmons becomes a fourth amateur who was significantly encouraged by Kidder.

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INTRODUCTION

Classic period painted vessels are a major focus of research in the Mimbres Mogollon area. They are a strong line of archaeological evidence in studies of social status, gendered divisions of labor, social networks, and belief systems (Gilman 1990; Munson 2000; Powell 2000; Ruth 1996; Shafer 2003:188-193; Shafer and Taylor 1986). This focus is understandable; Mimbres painted pottery, with its Escher-like geometric patterns and figurative designs is visually arresting and offers glimpses into their creators' lifeways not found in other southwestern ceramics. Mimbres utility wares have received much less attention, perhaps because date ranges for their manufacture are less fine-grained than the temporal distinctions which have been developed for painted wares (Shafer and Brewington 1995) and because changes in surface treatments are difficult to interpret. This study examines Mimbres Mogollon smudged ceramics (Figure 1), a type archaeologists consider "utility ware" due to the absence of painted designs. However, Jerry Brody (2003 personal communication) has pointed out there is more than one way to decorate a pot. Surface treatments like slip without paint, patterned corrugations, and smudging should be included when considering how decorative treatment affects vessel function.

I analyze a sample of Mimbres Mogollon smudged ceramics from a feature at LA 8675 (the West Fork Site) that dates to the Classic period (A.D. 1000-1140). Experimental and ethnoarchaeological research has demonstrated smudged pots are superior cooking and storage vessels (Longacre et al. 2000; Schiffer 1990; Schiffer and Skibo 1989; Schiffer et al. 1994; Skibo et al. 1997). Smudging makes pots less porous, less susceptible to thermal shock, and more resistant to abrasion. There is also evidence the smudging process creates polymers which limit the growth of bacteria and fungi, a desirable property in vessels used to store solid food and fermented liquids (Rogers 1980). Starting with Brody's expanded definition of decorated ceramics, I assess the technical choices potters made as a way to determine the role smudged vessels played in Mimbres Mogollon lifeways.

Figure 1.
Mimbres Mogollon smudged bowl (photo courtesy Museum of Indian Art and Culture, Santa Fe).
Three technological decisions are examined: porosity or permeability, firing conditions, and the selection of vessel form. Porosity determines how much liquid penetrates the clay body. Highly porous cooking pots are more likely to crack when liquids seep into their walls (Rice 1986:230-231). Firing conditions determine many characteristics of a pot, including the color or colors of the finished vessel. Manipulation of firing atmospheres is critical to the production of the distinctive Mogollon smudged ceramics, with their brown or red exteriors and lustrous black interiors. A vessel’s form, to a great extent, determines whether it is used for cooking, storage, or serving. The primary function of smudged vessels is inferred by determining the proportion of jars and bowls in the site’s assemblage.

LA 8675 is a small, multicomponent Mimbres Mogollon site, with at least 20 surface rooms and 14 pit structures that was excavated during a 1966 highway project (Ice 1968). It is located in the Gila National Forest, on the first bench of the West Fork of the Gila River and approximately two kilometers east of the Gila Cliff Dwellings. Although the Gila Forks are considered part of the “Mimbres heartland” (Shafer 2003:Figure 1.1), the area’s archaeology has been less intensively studied than the Mimbres Valley (Russell 1992:87-106). Except for the Cliff Dwellings, information is limited to surveys (Bradford 1992; Warnica 1975), a small test excavation project (Diehl 1995), and the unanalyzed data associated with two salvage projects (Hammack 1966; Ice 1968; Janes and Reeves 1974).

Written material on West Fork is sparse. The only publication is a nine page preliminary report (Ice 1968) which provides basic descriptions of features and site chronology. Harry Shafer’s graduate students at Texas A&M have included a small number of West Fork sherds in their INAA (instrumental neutron activation analysis) sourcing studies of ceramics in the Mimbres Valley (Dahlin 2003; James et al. 1995). In 2002, Chris Turnbow and his team at the Laboratory of Anthropology began an in-depth analysis of the site’s field documents (New Mexico Historic Preservation Division, Archaeological Records Management Section, LA 8675 site file) and artifact collections. The Laboratory’s goal is the publication of the final site report in conjunction with other studies it is conducting in the Upper Forks.

Archaeologists working with Mimbres Valley ceramics usually devote no more than a few sentences to smudged vessels. Texas A&M’s INAA study has confirmed earlier assumptions that smudged pots were trade wares (Anyon and LeBlanc 1984:153; James, et al. 1995:114-115; Shafer 2003:187-189). This compositional analysis also indicates most of LA 8675’s smudged pots were manufactured locally; some were traded to people in the Mimbres Valley. Smudged vessels are more common in the Upper Forks than in the Valley. Five percent of LA 8675’s sherds are smudged. In contrast, smudged sherds at Galaz and NAN Ranch in the Valley constitute less than one percent of the ceramic assemblages (Anyon and LeBlanc 1984:154-157; Shafer 2003:Table 10.1).

There is evidence that the last use of some smudged vessels in the Upper Forks and the Mimbres Valley was different. The appearance of smudged bowls in burial contexts is extremely rare in the Valley and its tributaries. Only one percent of the ceramic funerary offerings at NAN Ranch (Shafer 2002:262) and three percent at Cameron Creek (Bradfield 1929:65-105) are smudged. Although none of LA 8675’s burials included smudged vessels, 37% of the ceramic grave goods at the nearby X-S-X Ruin are smudged (Forrester 1992:186-187).

Smudging is a labor-intensive, multi-step process. The lustrous black finish is achieved by intensive polishing while vessels are in a leather-hard state and then exposing surfaces to a reducing atmosphere which deposits carbon on ceramic surfaces.
Potters can achieve an all-over black vessel by maintaining a reducing atmosphere within the kiln. Producing a pot which is only smudged on the inside is more difficult. The vessel must be fired in an oxidizing atmosphere. The blackened interior is made by either packing the pot with organics and inverting it for the firing, or removing the pot from the kiln while it is still extremely hot and filling it with combustible material. The packing will ignite and, if the vessel is inverted, the fire inside will be oxygen-poor and smoky. This mini-reducing atmosphere lays down carbon on the pot’s interior. The transfer of smudging technology is different than the adoption of many decorative techniques because it requires face-to-face contact. A potter cannot examine a smudged vessel traded into her village and understand how to make the lustrous surface; a knowledgeable potter has to demonstrate the process.

Smudging technology appeared in the Mogollon region of east central Arizona around A.D. 600 (Haury 1940:87-90). The knowledge moved slowly eastward, arriving at sites in the Upper Forks around A.D. 900. During the pithouse to pueblo transition, smudged bowls were a common burial accompaniment in the Mogollon region outside the Mimbres area. Mogollon smudge vessels were also trade wares. They appear in small quantities throughout Chaco Canyon, including Pueblo Alto’s trash mound, and at Chacoan Great House communities in the San Juan Basin (Toll 2001:57). Margaret Nelson (1999) and her team have recently identified smudged pots in Post-Classic Mimbres (12th and 13th centuries) sites on the eastern slope of the Black Range. They are interpreting the smudged vessels as evidence of co-residential situations involving migrants from the Mimbres Valley and the Reserve area.

**PROCEDURES**

**Sample Selection**

Thirty sherds each of plain, corrugated, and smudged varieties were selected for the analyses of apparent porosity, firing temperature, clay resources, and temper size. The 90 samples are from LA 8675’s Feature 5, a Classic period (A.D. 1000-1140) surface room. There are no chronometric dates for the site, but taking the sample from fill in one room imposed a modicum of chronological control on the selection process. My sampling procedure shifted for the determinations of vessel form and use-alteration. The entire site’s assemblage of smudged, corrugated, and plain rim sherds containing at least 5% of the rim’s diameter and 4 centimeters of the vessel's body were used to obtain the distribution of jars and bowls among the three types. The smudged and plain bowl rims from this expanded sample were used to assess use-alteration. Corrugated bowl rims were not included because the sample size was too small to be meaningful.

**Apparent Porosity**

Apparent porosity measures permeability by determining the bulk volume of pores, or spaces, open to the exterior (Rice 1986:350-354). Potters can manipulate porosity through firing temperature and the size, shape, grading, and density of particles in the clay body. Treatments like glazing, slipping, and smudging reduce porosity by filling open pores.

The procedure for determining the apparent porosity values is a multi-step process. The samples were saturated by boiling in distilled water for four hours. Each sherd was then weighed while suspended in water and in air. The volume of water \((V_f)\) displaced by the saturated sherd was...
calculated by first subtracting sherd weight in water (Sw) from saturated weight in air (Sf):

\[ Sf - Sw = \text{weight of water} \]

and then changing grams to cubic centimeters:

\[ Sf - Sw \times 1cc/gr = (Vf) \]

Finally, apparent porosity was calculated using the formula:

\[ \frac{Sf - Wf}{Vf} \times 100 \]

Original Firing Temperatures and Clay Sources

Changes in the colors of re-fired sherds help identify original firing temperatures and clay sources. I tracked color changes with the Munsell Soil Color Charts (2000). Because color perception is subjective, even when the light source remains constant, changes in hardness were also noted as a check on the color evaluations. Sherds were re-fired at progressively higher temperatures (intervals of 50°C) until two changes in paste color and an increase in hardness on the Moh’s scale occurred.

The number of clay sources represented by a sample can be estimated by re-firing at temperatures which exceed the sherds’ original firing temperatures (Rice 1986:427-428). Similar clay sources are suggested when sherds re-fire to the same color. All sherds were re-fired at 950°C for 15 minutes. Final colors were determined using the Munsell charts.

Original Firing Atmosphere

Owen Rye has developed guidelines (Rye 1981:114-116) correlating the colors of sherd cross-sections with firing atmospheres. In low fired pottery like the LA 8675 assemblage, cross-section colors are due primarily to the removal of carbon by oxidation or the deposition of carbon through reduction. I used his system to determine whether kiln fires were oxidizing or reducing. Sherds were examined under a binocular microscope and compared to Rye’s published charts.

Vessel Forms

I employed two procedures to obtain information about vessel forms. First, the site’s corrugated, plain, and smudged rim sherds, with at least four centimeters of vessel body below the rim, were examined. Sherds with direct rims (straight, no curvature) and smoothened, burnished, or polished interiors were classified as bowls. Jars were identified by the presence of everted rims (set off from the vessel body at an angle) and unpolished interiors (Rinaldo and Bluhm 1956; Shafer 2003:186). Everted rims with smoothened or burnished interiors were considered jars. Vessels with smudged interiors and corrugated exteriors were classified as smudged. Following this sorting, Pottery Information Equivalents (Banning 2000:106), or the proportions of rim circumferences represented by the rim sherds, were calculated to determine the percentages of plain, corrugated, and smudged bowls and jars at the site. The formula for determining Pottery Information Equivalents is:

\[ P_i = \frac{X_i}{t} \sum_{i=1}^{t} X_i \]

RESULTS

Apparent Porosity

Test results indicate differences in the apparent porosity, or permeability, of the smudged, plain, and corrugated sherds are not statistically significant. The average porosity of the smudged sample was 28.1% (s.d. 7.69). The average porosity of the corrugated sherds was 29.8% (s.d. 5.9), while the average porosity of the plain sherds was 29.9% (s.d. 5.0). However, the range of porosity values was greater in the smudged sherd sample (Figure 2).

The experimental work of Schiffer, Longacre, Skibo, and Rogers suggests the porosity of the
Smudged sample should have been significantly lower than the porosity of the corrugated and plain sherds because smudging creates an impermeable surface. Several possible reasons for the discrepancy between this study's results and the results of other researchers were investigated. A different firing temperature could account for the high values of the smudged sample. However, when I investigated this variable there were no statistically significant differences between the original firing temperatures of the three types (Figure 3). The proportion of large (>1.0 mm) temper particles in a clay body also affects porosity. The plain sherds had fewer large temper particles than corrugated and smudged sherds at a level that was statistically significant. The proportions of large temper particles in the corrugated and smudged samples were similar. If temper size caused the high porosity, the smudged sherds would contain more large temper particles than the corrugated sherds.

Clay constituents also have an impact on porosity. Color comparisons of sherds refired at 950°C provides a general picture of the number of different clay sources represented in a sample. When I refired the sherds a final time, they displayed similar proportions of red and light red, suggesting only two clay sources were used for the manufacture of LA 8675's smudged, plain and corrugated vessels.

I also investigated use-alteration as a possible cause of the high porosity values. All smudged and plain bowl rim sherds containing at least 4 centimeters of the vessel's body were examined under a binocular microscope for signs of wear. Interior rim edges of both types had nicks and multiple scratches. The bowl bodies contained abraded and pitted patches which often exceeded 0.5 cm in diameter. Without magnification, smudged surfaces appear uniformly smooth and polished, but magnification reveals many irregularities. With use, striations, spalls, and abrasions remove portions of the smudged surface and expose the underlying vessel walls.

Firing

Figures 4 through 7 represent idealized cross-sections of sherds smudged by different firing processes. Three smudged sherds, or 10% of the sample, had gray cross-sections with thin (<1.0 mm) shiny...
black interior and exterior margins (Figure 4). This color pattern is produced in a reducing kiln atmosphere. Packing bowl interiors with combustible materials is not necessary to achieve the smudged affect.

The most common (62%) smudged cross section (Figure 5) had a black interior margin between one and three millimeters wide, a gray core, and an oxidized red, brown, or pinkish exterior two to three millimeters wide. A short firing in an oxidizing atmosphere produces these layers of colors. The dark core is due to a brief “soaking” time and/or incompletely combusted organics in the clay body. The smudged interior results when a mini-reducing atmosphere is created within the bowl by packing it with organic materials.

Twenty-eight percent of the sample had cross sections with black interiors and oxidized exteriors of approximately equal widths and no gray cores (Figure 6). None of the cross sections had black interiors, wide gray cores and thin (<1mm), oxidized exterior margins (Figure 7) indicating vessels were fired in a reducing atmosphere, then removed from the kiln and inverted. Reduced exteriors, exposed to oxygen develop brown, oxidized surfaces while the interiors retained their smudging.

Vessel Forms

Pottery Information Equivalents indicate there is a differential distribution of jar and bowl forms among the plain, corrugated and smudged rim sherds. Eighty nine percent of the smudged vessels are bowls, while bowls represent only 28% of the plain and 5% of the corrugated pots.

DISCUSSION

The high proportion of smudged bowls in the LA 8675 assemblage indicates potters did not smudge these vessels to produce superior cooking and storage pots. In the Mogollon region, cooking and storage vessels have necks with out-curving or everted
This morphology is advantageous for cooking because necks and curved rims reduce the likelihood of boil-over and send condensation back into the cooking food, thereby minimizing the risk that the contents will burn (Rice 1986:237-240). Everted rims also facilitate pouring (Rice 1986:Table 7.2). Jars with restricted orifices function well as storage containers because the narrow necks make it easy to protect the contents with covers. On the other hand, bowls are used primarily as food preparation, serving, and individual eating containers. Their large orifices allow easy access and unrestricted viewing of the contents, but limit their utility as cooking and storage vessels.

The preponderance of smudged bowls indicates the black lustrous finish was valued for its visual rather than mechanical performance. Although smudged vessels account for only 13% of LA 8675's "utility wares," they represent 50% of the site's unpainted and unslipped bowls. Fully 89% of the smudged vessels are bowls for food preparation, serving, and eating.

I did not expect smudged sherds to be highly porous because smudging creates a relatively impermeable barrier on ceramics. My analyses found that clay selection, firing temperature and temper were not responsible for the high values. Potters selected the same clays and fired at the same low temperatures regardless of whether they were creating plain, corrugated, or smudged pots. Use-alteration (pits, abraded surfaces and spalls) appears to be responsible for the destruction of the smudged surfaces. Microscopic examination of surfaces revealed the impermeability of the surfaces had been compromised by repeated stirring, scooping, and probably cleaning with an abrasive material like sand (Skibo 1992:118-120). Experiments testing the effects of abrasion on different surface treatments demonstrate smudging is initially quite resilient. Eventually the surface becomes susceptible to the scratches and scrapes of repeated use (Skibo et al. 1997:315). It is noteworthy that the decline in mechanical performance does not correlate with a similar decline in visual performance. The deep abraded patches, spalls, and striations evident under a microscope are barely visible to the naked eye.

There are at least four different ways Mogollon potters could have smudged their bowls. The simplest would have been to fire vessels in a reducing atmosphere. This method would produce the black interior and exterior pictured in Figure 4's cross-section. No special manipulation of the pots is necessary; the reducing atmosphere lays down a layer of carbon on the polished surfaces. Bowls could also have been fired in a reducing atmosphere and removed from the kiln to cool. This technique creates a thin oxidized exterior layer of red or brown clay (Figure 7). However, my analysis of color patterns on smudged sherd cross-sections indicates the majority (90%) were fired in an oxidizing atmosphere which left exteriors with wide brown or red margins (Figures 5 and 6). A mini-reducing atmosphere inside the bowl produced the black surface either during the firing, or after removal from the kiln. These are not the easiest ways to smudge a pot. To achieve a smudged interior in an oxidizing atmosphere bowls had to be packed with organic material and inverted. This procedure could occur as the pot was going into the kiln or later when it was removed from the fire and still very hot. In both scenarios, the hot walls of the vessel would ignite the organics and produce a smoky fire in the oxygen-poor bowl interior.

CONCLUSIONS

My study indicates that, in addition to painting vessels, some Mimbres Mogollon potters employed smudging as a decorative technique. Experimental work has demonstrated the enhanced mechanical performance of smudged cooking and storage vessels. The visual performance of smudging cannot be quantified or tested in the same manner as resistance to thermal shock and abrasion but the deci-
sion of potters in the Upper Forks to produce smudged bowls rather than jars suggests the makers were unaware or uninterested in the superior mechanical properties of smudging. Potters did not choose to fire their bowls in a reducing atmosphere, a technique requiring less effort and fewer steps than firing in an oxidizing atmosphere and creating a mini-reducing atmosphere inside the bowl. The firing technology these potters selected required a multi-step process of packing vessels with organic material before or after firing. This more complex firing regimen also suggests contrasting colors, in this case black interiors and brown or red exteriors, was the goal. By manipulating two different firing atmospheres, potters produced sharp color distinctions between interiors and exteriors. Potters in the Upper Forks made technological choices for their visual rather than mechanical affects when manufacturing smudged bowls.

Because smudging is a technological style that requires face-to-face learning, the extremely low proportions of smudged vessels at Classic sites in the Mimbres Valley and the INAA evidence that these vessels were imported from the Upper Forks indicates potters in these drainages represent different ceramic traditions with different learning frameworks. The higher proportion of smudged bowls found in Upper Forks’ settlements suggests Forks’ residents maintained broad social networks with the Mimbres Valley and the Reserve/San Francisco area to the northwest, where smudged bowls were common. Further investigation of the manufacturing loci and social networks represented by Upper Forks smudged bowls is being undertaken in conjunction with an INAA ceramic sourcing project based at the University of Texas.

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CERAMIC USE-WEAR IN THE AMERICAN SOUTHWEST

In the American Southwest, ceramic vessels became important tools for a variety of activities beginning by about A.D. 200. If frequency is an indicator of intensity of use, then pottery became increasingly important in mundane and ritual contexts over the ensuing centuries. Pots occur in all possible contexts, from refuse to ritual. Determining the actual use of these vessels requires multiple lines of evidence, including contexts of use, use alteration, and use residues. In this paper, I examine the use-wear of ceramics from the American Southwest. Fortunately, increasing numbers of researchers record use-wear in their studies of whole vessel collections, making it possible to compare use-wear on various types. I primarily review existing studies, but include a few additional recent studies that are unpublished. I conclude that the majority of whole ceramic vessels recovered in the American Southwest show evidence for use, but that vessels with lower amounts of use-wear suggest stockpiling of vessels in anticipation of later use and differential use of some special types or forms.

USE-WEAR STUDIES IN THE AMERICAN SOUTHWEST

For well over a century, archaeologists working in the Southwest have observed use alteration on prehispanic ceramic vessels. This is particularly true for sooted vessels, with many researchers using presence of soot to interpret vessel function (Corwin 2000; Crown 1994; DiPeso 1951; DiPeso et al. 1974; Hally 1983; Hill 1970; Holmes 1886:272; Jones 1989; Kidder 1931; Lindauer 1988; Pierce 1999; Rinaldo 1959; Sprehn 2003; Turner and Lofgren 1966 among many others). Examination of wear on ceramic surfaces is a more recent scholarly occupation in the Southwest. While a number of researchers noted use-wear on surfaces (Bradfield 1931:65-109), few looked for correlations with other aspects of the archaeological record to explain the use found. In the 1970s and 1980s, researchers outside the Southwest began to examine use-wear in a more systematic, interpretive fashion (Chernela 1979; Griffiths 1978; Hally 1983). This was followed by an increasing number of southwestern case studies of use-wear (Bray 1982; Corwin 2000; Crown 1983, 1994; DeGarmo 1975; Fenner 1977; Hardin and Mills 2000; Jones 1989; Lindauer 1985, 1988; Pierce 1999; Powell 2002; Sprehn 2003; Van Keuren 2001; Washburn 1980). However, many of these studies are dissertations or available only in contract reports, making accessibility difficult. Finally, publications by Michael Schiffer (1989) and James Skibo (1992) provided a more systematic method for examining and recording use-wear than simple presence/absence.

The goals of use alteration studies in the American Southwest have concentrated on a few issues. First, many researchers attempt to get at physical use of the vessels through use alterations. Second, a few have examined use alteration as a means of assessing the social function of vessels. This includes questions of whether the vessels
were manufactured for use in ritual (particularly burial ritual or feasting) versus more mundane quotidian uses. A few studies have also examined variation in use alteration relative to specific aspects of the vessels themselves, such as quality of the painted decoration or vessel shape. Most use-wear studies focus on decorated vessels and bowls rather than undecorated wares and jars. There now exist a sufficient number of studies from different areas and times of the Southwest to make comparison possible. I first review the studies, and then compare their results.

In his dissertation study of ceramics from the Coyote Creek Site, Glen DeGarmo (1975:168-175) examined use-wear on the interiors of 88 decorated bowls, divided into two size classes. He found that 13 bowls (15%) showed evidence for use over a fire, and 47 (53%) had heavy abrasion. He concluded that there were no significant differences in the use-wear on the two bowl classes, and that the bowls were used for mixing, serving, eating, and storage.

Dorothy Washburn (1970) examined use-wear on 109 Chaco cylinder jars excavated by Pepper. She does not provide quantitative data, but notes that use-wear was present “on the inside of several jars (Washburn 1970).” She interprets these results as indicating that raw matrix of shell or turquoise was stored in the jars, scratching the sides as the material was moved in or out of the jars.

Gloria Fenner (1977:136-137) collated the data on use-wear first published by Wesley Bradfield (1931) for 129 Mimbres black-on-white vessels from Cameron Creek Village. Her research involved determining whether flare rimmed bowls had a different function from hemispherical bowls. Bradfield had described use-wear on the vessels, of which 90 (70%) out of 129 came from burials. Fenner collated the use-wear data for 127 of the vessels and found that 85% showed moderate to heavy wear, and 15% showed no wear. She did not find significant differences between the two bowl shapes in wear categories, although none of the flare-rimmed bowls were in the “not worn” category.

In 1982, Alicia Bray published a detailed study of use-wear on Mimbres decorated vessels. Bray used 124 black-on-white bowls from many Mimbres site collections housed in the Arizona State Museum for her study. She used an ordinal scale to record wear in the interiors and exteriors of vessels, ranging from no wear to extensive wear. She tested four hypotheses:

1. that bowls with geometric versus zoomorphic designs had different uses;
2. that bowls produced exclusively as mortuary goods should have no wear (tested by comparing use-wear on bowls with kill holes versus those without);
3. that bowls with different forms had different uses;
4. that finely painted bowls were used differently than poorly executed bowls.

She found no significant difference in the use-wear patterns on bowls with geometric versus zoomorphic designs and no significant difference in use-wear patterns on bowls with kill holes versus those without. She did find differences in the use-wear related to vessel shape, with unusual (called “novelty”) shapes exhibiting less use-wear than hemispherical bowls, and small bowls showing less use-wear than large bowls. Finally, she found that finely painted bowls had less wear than bowls with poorly executed designs. She concluded that the most finely painted vessels were used as status items and more highly valued than the poorly executed vessels (Bray 1982).

I examined use-wear on 603 vessels from Classic Period Hohokam sites including Los Muertos, Las Acequias, Las Fosas, and three sites on Queen Creek Delta excavated by Gila Pueblo (Crown 1983:119-203). The sample included 21 separate
types, roughly divided into redware, plainware, red-on-buff ware and polychrome, and all forms represented in these assemblages. Use-wear was recorded on an ordinal scale and by position on the vessel. The purpose of the use-wear analysis was to examine intended use of the vessels. Ultimately, use-wear could only be recorded for 414 of the 603 vessels. The results indicated no difference in use-wear by ware for plainware, redware, and polychrome vessels. There were significant differences in use-wear by form however, with bowls exhibiting a higher proportion of wear on both the interior and exterior, jars and pitchers a higher proportion of exterior wear alone, and scoops a higher proportion of interior wear and no wear than expected.

Owen Lindauer assessed use-wear on Hohokam red-on-buff vessels in two studies. In the first, he examined 93 Santa Cruz and Sacaton Red-on-buff bowls to ascertain if the vessels were strictly mortuary ware or used prior to placement in burials (Lindauer 1985:585). Based on the fact that 20% of the bowls lacked use-wear, he speculated that some classes of vessels might have been made especially as mortuary furniture. Lindauer (1988) expanded this pilot study for his dissertation. He examined use-wear on 1324 Sacaton Red-on-buff vessels from a variety of private and museum collections. He again asked whether vessels from burial and non-burial contexts have similar varieties of use-wear, in order to determine if vessels from burials are representative of the entire suite of vessel forms and uses. He recorded only presence/absence and location of wear. Because he only included vessels from known contexts for this aspect of his study, his sample size was reduced to 865 vessels of many forms. He found that vessels from burial contexts were not significantly different from vessels from other contexts in presence of use-wear. He concluded that most burial vessels were not made specifically as mortuary vessels. He also found that more than half of the vessels from caches exhibited no use-wear, and interprets this as meaning that new vessels may have been stock-piled. Overall, the assemblage included 26% of vessels with little to no use-wear (Lindauer 1988:80). He found no sooting on any decorated vessels, suggesting that decorated vessels were not used for cooking (Lindauer 1988:143). He also examined the presence/absence of use-wear relative to design richness. He found that bowls and shouldered jars with use-wear had a richer diversity of design motifs than those without wear, while censers with wear had a much lower richness of elements (Lindauer 1988:277). These results raise questions that he is not able to answer due to sample sizes.

In 1989, Bruce Jones published a report of his use-wear and sooting analysis of 74 White Mountain Redware bowls from Grasshopper Pueblo (Jones 1989; see Van Keuren [2001:166-167] for a critique of this study). Jones examined the White Mountain Redware bowls to determine if there were differences in use-wear related to vessel size and decorative style. He divided the sample into three size classes. Nine bowls (12% of the sample) showed evidence for possible use in cooking, including carbon deposits and pitting. He suggests that the bowls might have been placed near a fire or on embers to warm their contents. Finally, he found no significant difference in the amount of use-wear on Pinedale versus Fourmile style vessels (Jones 1989:359). Unfortunately, he does not provide tables with actual occurrences of use-wear for the sample.

As part of an extensive study of 779 Salado polychrome whole vessels, I examined evidence for use-wear and post-firing exposure fire (Crown 1994:99-113). The assemblage included all forms, bowls, jars, and eccentrics. Wear was recorded on an ordinal scale from none to heavy. Eighty-one percent of the vessels showed moderate to heavy abrasion marks on the interior, exterior or both, while 19 percent of vessels had little to no wear. Ten percent of vessels had evidence of post-firing exposure fire (sooting, crackling of slip, warping, or slip/paint discoloration); however, because this
evidence was usually pervasive over the entire vessel, I believed that the vessel damage might be related to something other than cooking. The exception is bowls with a recurved rim shape, which have a high incidence of burning and often occur in non-burial contexts, suggesting that they might have been used for cooking (Crown 1994:102). Eccentric vessel forms, including effigies, had little/no use-wear more than other forms, suggesting special uses for these special forms. The percentage of vessels with moderate to heavy use-wear decreased over time, from Pinto (86%) to Gila (83%) to Tonto Polychromes (74%). I interpreted this pattern as suggesting either that the older Pinto Polychrome vessels had more opportunity to acquire use-wear marks or that use of the vessels changed over the 150 year period of their manufacture (Crown 1994:100). Examination of use-wear relative to context of recovery revealed significant differences, with vessels showing little or no abrasion occurring twice as often in burial contexts as expected. Nevertheless, the majority (79%) of vessels recovered from burials had abrasion on one or both surfaces, suggesting that most vessels were not made as mortuary vessels. Following Bray's results discussed above, I examined the incidence of use-wear in relation to quality of decoration (recorded on an ordinal scale from fine to sloppy). The vessels with the finest painting did show significantly less use-wear than expected, but the vessels with the poorest painting did as well; the vessels with medium artistry showed considerably more use-wear than expected.

A 1999 dissertation by Christopher Pierce examined use-wear on utility pottery. This study is unusual because it focused on utility ware, and also because he examined use-wear on sherds rather than whole vessels. A sample of 4174 sherds from six sites in the Mesa Verde region was used, dating from the eighth to the eleventh centuries. He was interested in the function of corrugated pottery, and so selected material that covered the range of utility ware from plain-walled to fully corrugated. Pierce recorded use-wear on an ordinal scale, and monitored both wear and sooting. He found that wear increased from the 8th to the 10th centuries and then declined. The overall incidence of use-wear is low for these assemblages, with 86% of sherd interiors and 95% of sherd exteriors showing no use-wear. Wear is primarily associated with basal sherds, and Pierce argues that these grayware vessels were used for cooking. Over 40% of all base fragments have pitting on the interior surface (Pierce 1999:109). He interprets these results as suggesting either that individual vessels were more intensively used in the 9th and 10th centuries or that a greater proportion of utility vessels were used in cooking at this time. The reduction in use-wear in the 11th century corresponds with full-body corrugation in this area, and may result from the corrugations differentially protecting the vessels from use-wear rather than a change in use intensity (Pierce 1999:111). He finds sooting on sherds from all portions of the utility jars in all time periods. A shift in vessel forms over time is interpreted as indicating that utility vessels became increasingly specialized for use in cooking over time (Pierce 1999:116). While the results of this study are intriguing, it is not possible to compare the results to other studies because of the use of sherds instead of whole vessels.

In 2000, Crystal Corwin (2000) completed an MA thesis at Northern Arizona University on use-wear. Her goal was to develop a method for analyzing use-wear on decorated bowls. She was particularly interested in the function of decorated vessels in the Pueblo IV time period. She used two assemblages for her study: 132 Jeddito Black-on-yellow bowls and 69 Rio Grande Glazeware bowls, most from mortuary contexts. She recorded use-wear on an ordinal scale from none to extensive, and she also recorded sooting and naturalistic versus geometric designs. For the Jeddito bowls, she found that 28.7% had minimal to no use-wear, while 57.9% of the Rio Grande Glaze Ware bowls had minimal to no use-wear. Only four Jeddito vessels were sooted; no Rio Grande Glaze Ware bowls were sooted. She found no correlation...
between degree of use-wear and vessel volume or size class for either ware. She also found no correlation between use-wear and design style for either ware. She concludes that vessels with different sizes and design styles may have had similar uses. A chi-square test of differences in degree of use-wear for the two vessel samples was significant, showing differences between the two ware categories, with the Jeddito sample showing considerably more use-wear than the Rio Grande Glazeware sample. To test whether these differences were significant, she did Mohs' scratch tests on a sample of sherds. She found that the Jeddito Yellow Ware was considerably harder than the Rio Grande Glaze Ware, despite the fact that it showed more use-wear. She suggests three possible interpretations for this patterning: 1) people might have used ladles to scoop food from the Jeddito Yellow Ware and fingers or gourds to scoop food from the Rio Grande Glaze Ware; 2) the Jeddito Yellow Ware may have lasted longer and accumulated more use-wear, while the softer Rio Grande Glaze Ware broke more frequently accumulating less use-wear; 3) decorated bowls might have been perceived and used differently in mortuary contexts by the Hopi versus Rio Grande peoples; 4) large decorated bowls might have been used for different purposes than the small bowls, and since there are bowls with larger volumes from the Rio Grande area, their possible use in feasting activities might leave fewer use traces; however, Corwin (2000:71-72) shows that there is no significant difference in use-wear between larger and smaller Rio Grande vessels, suggesting no difference in use; 5) finally, there might be a difference in when people from the two regions retired used vessels. She recommends additional research to determine which interpretation is correct.

Margaret Hardin and Barbara Mills (2000) examined use-wear on 748 historic Zuni vessels. They recorded use-wear on a four value ordinal scale from absent to high use-wear. They find higher amounts of use-wear on vessel basal exteriors than interiors, and that basal wear correlated with vessel bowl class. Small bowls in this sample showed the least basal wear (71% moderate to heavy wear), with increasing amounts of wear on medium bowls (89%) and the highest amount of use-wear on the largest bowls (100%). They interpret this pattern in terms of vessel use. They argue that the small bowls were used as individual eating bowls, and that they were moved frequently and had short uselives, but were not dragged over surfaces. The medium bowls they interpret as communal eating bowls, subject to abrasion from moving among the family during meals. The largest bowls were bread bowls, subjecting them to considerable abrasion. On the basis of these results, they argue that the three bowl sizes were used differently and should be analyzed separately, that the small bowls had the highest replacement rate so that their bases did not have time to become abraded (Hardin and Mills 2000:148).

Scott Van Keuren examined use-wear as part of his dissertation study of White Mountain Redware vessels from East-Central Arizona. He examined use-wear on 758 bowls ranging from St Johns Polychrome to Fourmile and Grasshopper Polychromes. His study had several goals: measuring how intensively polychrome vessels were used, how they were being used, and whether use changed through time and space. He recorded use on an ordinal scale on three areas of the vessel (bottom, interior, rim). He found that the bowls in his sample were more intensively worn on the exterior base than on the interior or rim, with the percentage of vessels with moderate to heavy use-wear increasing from St Johns to Pinedale Polychrome, and then decreasing from Pinedale to Fourmile Polychrome (Van Keuren 2001:173-174). He interprets these results as indicating a possible shift in the use of White Mountain Redware bowls over time. Overall use patterns suggest the bowls were used for serving food. He found odd use abrasions on one exterior side of many vessels, a pattern which he suggests might occur because the vessel owners propped them on their sides to display the vessel interiors when they were
empty (or alternatively from display in museum
exhibits). While most vessels came from mortu-
ary contexts, they still displayed use, indicating
that they were not strictly mortuary wares. He
finds greater use-wear and fewer mend holes on
Fourmile Polychrome vessels found at villages
where it was produced than at villages where it
was an exchange ware (Van Keuren 2001:172).
Finally, he notes that Tularosa and Pinedale Style
bowls fall into two size class, and he suggests that
the small bowls were used by individuals for food
consumption while the large bowls were used
communally in food serving/preparation. Smaller
sized bowls exhibit greater interior use-wear than
larger bowls of the same time period (Van Keuren
2004:200). In contrast, Fourmile Polychrome
bowls do not fall into two distinct size classes, and
wear patterns vary by area, suggesting differential
use by different villages.

Melissa Powell examined use-wear on 273 whole
vessels from Pecos Pueblo and nearby sites for her
dissertation research on the organization of pro-
duction in the Upper Pecos Valley from A.D.
1200-1400. She was particularly interested in
determining vessel function for her assemblage.
Her sample included Pajarito White Ware (Santa
Fe and Galisteo Black-on-whites) bowls, jars and
exotic shapes and Rio Grande Gray Ware utility
ware jars and duck pots. Forty-five percent of the
vessels came from burial contexts. Evidence for
use varied by form: 80% of black-on-white bowls,
88% of black-on-white jars, 22% of exotic black-
on-white forms, 61% of utility ware jars, and 65%
of utility duck pots had use-wear (Powell 2002:
251). She concludes that the condition of the
vessels indicates they were not produced as mor-
tuary furniture.

In her dissertation examining the organization of
production of Chihuahuan Polychrome vessels,
Maria Sprehn (2003) discusses use-wear on the
large assemblage of whole vessels she documented.
While she did not provide frequencies for all types
in her dissertation, she shared her data with me.

DISCUSSION

Over the last thirty years, we have accumulated a
large body of data on use-wear for vessels from the
American Southwest. I now compare the methods
and results of these studies. An initial question is
whether the various results should be considered
comparable since they were collected by so many
different scholars. Although no tests have been
performed to check for replicability among the
various researchers, I believe we can safely com-
pare the results at a gross level for several reasons.
First, there is surprising concordance in how the
many researchers recorded use-wear during this
period; virtually all researchers recorded use-wear
on an ordinal scale ranging from no visible wear to
heavy wear. In most cases, they used a four point
scale, but some researchers used a greater number
of points on the scale (see, for instance, Van
Keuren 2001). Second, use-wear is not difficult to
detect, except at the microscopic level, and most
of these researchers confined their observations to
what was visible to the eye or through a hand lens.
Finally, to avoid questions of comparability in
scales, it is easy to collapse the various points on
the scales into two: little to no use versus moder-
ate to heavy use, and to consider this largely a mat-
ter of absence versus presence of visible use-wear.
Future studies should include a test of replicability
of results among different individuals.
There are a number of generalizations we can make about southwestern vessel use based on these studies. First, all of the assemblages show a range of use from no use on some vessels to heavy use. We can assume that all vessels with heavy use-wear once had little to no use and that use-wear accumulates over time. The fact that virtually all assemblages showed a range of use from none to heavy suggests strongly that most use-wear accumulates from many uses rather than in a single or small number of events. What we do not know is whether use-wear is a function of slow abrasion over a long period of time or more aggressive abrasion over a shorter period of time. In either event, it is likely that if we were able to record use-wear on a continuum instead of an ordinal scale, we would find normal bell-shaped curves for most assemblages in degree of wear. However, we would also likely find that the mean for that curve would differ by ware, form, and type based on the results seen in these studies.

Although there are few studies of utility ware, the results suggest that utility ware generally shows proportionately less wear than decorated bowls and jars. Unusual/exotic/eccentric shapes also tend to show different amounts and kinds of wear than bowls and jars, confirming their special use (Bray 1982; Crown 1994; Powell 2002; Sprehn 2003). Results of use-wear studies on bowl forms are less clear. Two studies indicated that small bowls had less use-wear on average than large bowls, including Hohokam Red-on-buff vessels (Lindauer 1988) and Rio Grande Glaze Ware bowls (Corwin 2000). Future studies might consider the performance characteristics of the decorated wares used for cooking and whether they were more suited to cooking than the decorated wares that were not used in this way.

Regarding use of these vessels for cooking, some researchers found sooting on decorated vessels, including some Salado Polychrome bowls and jars (Crown 1994), Jeddito Yellow Ware bowls (Corwin 2000), White Mountain Red Ware bowls (Jones 1989) and Chihuahuan Polychrome jars (Sprehn 2003), while other researchers found no soot on decorated vessels, including Hohokam Red-on-buff vessels (Lindauer 1988) and Rio Grande Glaze Ware bowls (Corwin 2000). Future studies might consider the performance characteristics of the decorated wares used for cooking and whether they were more suited to cooking than the decorated wares that were not used in this way.

Figure 1 compiles the results of virtually all of the studies described here, ordered by assemblage with the most evidence for use to the assemblage with the least. The first set of bars shows the percentage of little to no use, and the second set the percentage of moderate to heavy use for the same assemblages. The two sets of bars are thus parts of the same results and are both displayed to emphasize patterns in the data. Table 1 lists the actual percentages of the same data for each ware. Several aspects of the data should be emphasized here: first, some studies included many vessel forms, while others included only bowls or jars; second, some studies included multiple wares, while most included only a single ware; third,
most bars represent a single ware with many types, while a few bars include only a single type. These differences are significant because the individual studies summarized above demonstrate that use-wear may (but not always) vary by form, type, quality of design style, portion of vessel, and context of recovery. This means that when we use assemblages that combine these, we may be missing important parameters for interpreting use-wear.

Such cautions notwithstanding, Figure 1 and Table 1 reveal interesting patterns. Most interesting is the fact that, despite possible differences in methods and recorders, most researchers report similar amounts of use-wear on large whole vessel collections, with nine out of fifteen reporting that 78-86% of the vessels they examined showed moderate to heavy use. All but two studies revealed a majority of vessels with moderate to heavy use, and these were the 69 Rio Grande Glaze Ware bowls studied by Corwin (2000) and the 124 Mimbres Black-on-white bowls studied by Bray (1982). A simple conclusion then is that most southwestern vessels were made to be used and were indeed used before being placed or discarded in their contexts of recovery. In addition, it appears that there is a fairly normal proportion of used to unused vessels in most assemblages, so that it is the assemblages with proportions outside this norm that should particularly provoke additional questions.

If vessels generally are made to be used, why do so many exhibit little to no use-wear? In all of the assemblages examined, at least 13.5% of the vessels showed little to no use. It is possible that the vessels with little to no use may be vessels that were manufactured and stockpiled, awaiting eventual use. While most of the vessels in these assemblages come from mortuary contexts, the fact that vessels with a range of use-wear intensities appear in burials suggests that these vessels generally were not made to be placed in burials. Instead, they were made to be used, but some were placed in
burials before they accumulated much wear. In Ben Nelson’s (1991:171) ethnoarchaeological study of Highland Maya household vessel assemblages, he emphasized the large number of vessels stockpiled in households awaiting use. Vessels were stockpiled to have a ready replacement for a broken pot. He also notes that stockpiling will increase with the number of anticipated failures (Nelson 1991:180). In the American Southwest, stockpiling would likely occur because in most areas, ceramic production would be circumscribed by climate. Because it would not be safe to fire vessels in cold weather, potters in the colder parts of the Southwest would produce most of the pots they needed on an annual basis in the warmer months, stockpiling the years supply for use as needed. If the unused vessels were stockpiled, it is possible to speculate that southwestern households stockpiled an average of about 15-25% of their vessels for future use. However, a potential problem in making this interpretation is that stockpile size should vary throughout the year, with the greatest number of unused vessels available in the warm months and waning through the winter, as broken vessels are replaced with vessels removed from the stockpile. Perhaps the 15-25%
represents an average for stockpiled vessels. It is also possible though that many of these vessels were in use and simply entered the archaeological record through abandonment, breakage, or placement in burials before they accumulated sufficient use to be placed in the “moderate to heavy use” category. A new vessel that is dropped is just as likely to break as an old vessel. Only one of the studies summarized here compares the condition (whether reconstructed or whole) of the vessel to the use-wear, and that is the study of historic Zuni vessels purchased for the Smithsonian and all whole at the time of purchase (Hardin and Mills 2000). The other assemblages appear to include a mix of whole vessels and reconstructed vessels. Interestingly, the Zuni collection shows the same patterning of use-wear as the majority of other collections. It would be worthwhile to know if broken/reconstructed vessels also show the same range of use-wear as their whole counterparts. Here, obviously it would be important to separate those vessels broken in prehispanic times from those broken after deposition.

Future studies of use-wear will likely resolve some of these issues. Based on the studies summarized here, it is possible to make some general recommendations for use-wear analysis. These studies indicate that use-wear may vary with vessel type, form, size within a single form, portion of vessel, and design. All of these attributes should be recorded. In addition, it may be important to consider not only the context of recovery, but also whether the vessel was whole or broken when it was deposited. An ordinal scale with four to six points seems adequate for addressing general questions of use.

In conclusion, studies of use-wear contribute to our understanding of southwestern pottery manufacture, use, and social function in important ways. Careful examination of use-wear may help us understand expected use lives of vessels, actual use of vessels, differential use of different forms and types, and how different groups regarded vessels with use versus those without. There are many issues concerning use-wear that have not yet been examined. For instance, while many studies have considered whether a ware was made for mortuary purposes by examining use-wear on vessels in burials, none has considered use-wear in relation to other aspects of the individual burial. How does degree of use-wear relate to the age, gender, or status of the individual? Are high status or infant burials more likely to receive vessels with no evidence of use? Were older, worn vessels held in higher or lower regard? While helpful in determining probable uses of specific vessel forms, use-wear holds additional potential when wear on different large assemblages is compared. We have much yet to learn from use-wear on southwestern ceramics.

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A HISTORIC RED-ON-BROWN VESSEL FROM NUESTRA SENORA DEL SOCORRO, SOCORRO, TEXAS

INTRODUCTION

Recent archeological research has focused on the effects of colonization on indigenous peoples. The following paper provides a material example of the social adjustments that are concomitant with indigenous peoples as part of the process of accommodating colonization. In this paper I will first describe the Socorro Mission in its historical context, and its partial excavation by the late Dr. Rex E. Gerald. I will then present an overview of the historic brown ware ceramic tradition that is present in the El Paso area and elsewhere in northern Mexico and New Mexico. The designs on the vessel will be compared with contemporary decorated ceramics and images derived from Catholic and Western European iconography. The painted vessel will then be used to illustrate how a pre-biological theory of vision, and promulgated by the church, used such iconography to reify religious practice in the Socorro community.

A BRIEF HISTORY OF THE SOCORRO MISSION(S)

The history of the location of Nuestra Señora del Socorro is the result of a complex interaction of Spanish racial politics and Rio Grande flood history. The initial settlement of Nuestra Señora del Socorro began with the Pueblo Revolt of 1680. The area surrounding the site of the first Socorro Mission site in the El Paso area was settled in 1682 by Piros from the New Mexico pueblo of Socorro and Tompiro who had fled previously from the Salinas Pueblos along with a few Jemez who had come south with the New Mexico governor Antonio de Otermin as refugees (Hughes 1914:323). The Piro leaders petitioned Governor Otermin for an area to establish their own community for purposes of farming on what is now the Texas side of the Rio Grande (Hughes 1914:364-366). The site was occupied, a church was constructed and burials were interred at the church (Gerald 1990 citing Chavez 1957).

In response to a possible local Native revolt in 1684, Otermin's successor to the New Mexican governorship, Don Jironza de Cruzate, consolidated the dispersed Spanish and Pueblo refugee settlements in the El Paso area. Resettlement of the Socorro community was directed by Fr. Nicholas Lopez who established separate Native and Spanish communities Nuestra Señora de la Purisima Conception de los Piros de Socorro del Sur for the Natives and San Pedro de Alcantara for the Spanish (Walz 1951).

The church established in 1684 was destroyed by a flood in 1740, and another church was established at a nearby location. In 1760, Archbishop Benito Tamarron y Romeral stayed at the convent of Socorro during his visit to the area (Kessell 1980). An unpublished Spanish document indicates that in 1795 there was a new church at Socorro and that the convent of this church was in ruins (Gerald 1990:47). The locations of these two churches are unknown. In 1829, the Socorro church, presumably the 1795 structure, was damaged by flooding...
and abandoned. The roof timbers from the nave of this church were salvaged and used in the present Socorro church that was dedicated on August 1, 1843 (Gerald 1990). Recent excavations conducted within the nave of the present Socorro church may have uncovered the 1795 structure beneath the present church (Stephen Mbutu, personal communication December 1, 2004).

The site of the presumed 1684 Socorro church and the artifacts recovered from excavation were investigated by Dr. Rex Gerald and his students from 1981 until his death in 1990. Excavations at the Socorro Mission exposed portions of the east-facing church wall, four rooms from a presumed convent attached to the northern wall of the church and a possible bell tower (Gerald 1990; n.d.). Evidence of this structure being the site of the 1684 Socorro church is based on the recovery of sherds from a Tewa Polychrome Spanish-style “soup-plate” and a shoulder from a Tewa Polychrome jar, along with individual majolica sherds of San Agustin Blue-on-white and Huejotzingo Blue-on-white and several sherds of Puebla Blue-on-white, all of which date to the late seventeenth and eighteenth centuries (Goggin 1968:187-190; Hill 1990). A few unidentified green-on-white sherds were also recovered (Gerald 1990:17). Subsequent excavations in and around the current (1843) Socorro church have taken place as the result of various local cultural resource management oriented projects and in conjunction with recent stabilization of this National Register property (Peterson and Brown 1992; Vierra 1999). None of these latter excavations have reported features or concentrations of artifacts that can be attributed solely to seventeenth or early eighteenth century occupations. Deep chisel plowing of the immediate area and a long history of flooding may have mixed earlier and later archaeological deposits rendering early historic occupations difficult to detect or discriminate from later materials except by temporally sensitive artifacts.

The decorated partial vessel that is the subject of this paper was recovered during the partial excavation of Room 4. Room 4 was considered to be a part of a possible convent (Gerald n.d.). The mission site had been subject to chisel-plowing that removed all but the upper 6 cm in the walls and most likely disturbed the fill in the room. The excavation of the room was oriented towards exposing the walls of the possible convent and shared a common wall with the church. The middle portion of the room was not completely excavated (Gerald n.d.). Excavation of the room failed to find a prepared floor. White and blue-gray lime plaster was present in patches on the walls of the room. The partial vessel was recovered from the fill of the room and appears to have been broken at the time of deposition (Gerald 1990:12-14; Gerald n.d.). The vessel was deposited sometime after the church was destroyed by flood but prior to the complete collapse of the convent’s walls. Other items recovered from the excavation of Room 4 included a second partially reconstructable red-on-brown vessel, one complete and the base of a second candlestick made using the same type of ceramic paste as the brown ware vessels, a brown ware soup plate, a bovine horn core, a copper disk, and a sherd of Puebla Polychrome (Gerald n.d.).

INDIGENOUS HISTORIC CERAMICS IN THE EL PASO AREA

Brown Ware ceramics dating to the historic period in the El Paso area have been variously referred to as Socorro Brown Ware or Socorro Red-on-brown, Ysleta Brown Ware or Ysleta Red-on-brown and Mission Red-on-brown (Hill 2002; Marshall 1997, 1999). Marshall (1997) groups all of these wares under the term Valle Bajo Brown Ware tradition to avoid assigning a particular ethnic identification to a corpus of ceramics that was produced by different indigenous peoples living in the El Paso area.
Petrographic analysis of historic pottery from the El Paso area indicates that the historic brown ware and red-on-brown pottery from the El Paso area was tempered primarily with sand; however, crushed potsherds, volcanic tuff and fine-grained felsic rock fragments have also been reported in trace amounts (Brown et al. 2004; Hill 1990; Marshall 1999). The use of sand temper in historic Brown Ware in the El Paso area is distinct from the use of granite found in prehistoric Brown Ware ceramics of the El Paso tradition (Hill 1988). The production of brown and red-on-brown pottery with sand temper is a wide-spread phenomenon during the Colonial and Mexican Republican Periods encompassing Casas Grandes and northern Mexico, the La Junta area of west Texas, and Hispanic and Genizaro communities in New Mexico (Carrillo 1997; Cloud et al. 1994; Dick 1968; Di Peso et al 1974; Gerald 1968).

With the exception of the vessel to be discussed below, decoration on red-on-brown ceramics from historic contexts in the El Paso area are simple and usually consist of red paint on the rims of jars, or small loops or triangles, and on one instance both that are pendent from the rims of both bowls and jars either on the exterior or interior (Leach et al. 1995; Marshall 1997; 1999). The other red-on-brown vessel recovered from Room 4 is decorated by a band above the shoulder composed of a line of vertical triangles filled with red pigment. A series of loops are pendent from the base of the band of triangles (Figure 1).

In the El Paso area, red-on-brown ceramics were not produced until the late eighteenth or early nineteenth centuries. Occupations in the El Paso area dating to the Spanish Colonial and early Mexican Republican Periods have ceramic assemblages composed entirely of undecorated Brown ware with a low frequency of Mexican lead-glazed pottery and Chinese porcelain (Hill 2002; Miller and O'Leary 1992). Based on published data from six excavation projects in the El Paso lower valley, red-on-brown ceramics account for less than 2% of the excavated ceramic assemblages (Hill 2002).

In the El Paso region, the earliest independently dated contexts where red-on-brown ceramics have been recovered are from Fort Filmore (1851 to 1862) and the Rough and Ready stage stop located on the Butterfield Stage line (1857-1861) located in Doña Ana County, New Mexico. As such, they may not represent the initial appearance of decorated ceramics (Hill 1986; Staski 1990). The decorated ceramics from the Butterfield stage-stop and Fort Filmore could also represent more local Tortugas and/or Manso pottery production, rather than being traded from the El Paso area. Casas Grandes is located only about 130 miles southwest of the Socorro church. Brown wares with red decoration are present, but rare, in the ceramic assemblage from San Antonio de Padua (A.D. 1660-1684) at Casas Grandes, Chihuahua (Di Peso et al. 1974:326). Capote Red-on-brown (A.D. 1600-1700) and Conchos Red-on-brown (A.D. 1700-1800), are present in ceramic assemblages from the La Junta area of west Texas (Cloud et al. 1994). By the beginning of the twentieth century, indigenous ceramic pro-

Figure 1.
Reconstructed red-on-brown vessel from Room 4 of the Socorro church convent and with characteristic designs for historic pottery in the El Paso region.
duction had ceased in the El Paso area. Jesse W. Fewkes in 1901 while collecting ethnographic material, including ceramic vessels, for the Smithsonian Institution, reported that pottery was no longer produced by the Tigua of Ysleta or at any of the contemporary indigenous communities, presumably including the Piro living in the Socorro community (Fewkes 1902).

INTERPRETING THE PAINTED VESSEL FROM ROOM 4

The vessel to be discussed is unique in its decoration when compared with other contemporary red-on-brown ceramics from the El Paso area (Figure 2). The partial vessel is a wide-mouthed jar and has a maximum height of 25 cm. The partial vessel does not present a complete base-to-lip profile and so was likely slightly taller. The vessel has a maximum body diameter of 25 cm and a mouth diameter of 17 cm. The vessel's walls are about 6.7 mm in thickness but vary slightly. The designs were executed using a red, presumably hematite-based pigment. Distinct striations from polishing are present over the painted designs, and nowhere else, on the vessel. The paste of the vessel was tempered using sand.

Two complete images are present on the vessel. Two or more incomplete images are also present along one of the broken edges of the vessel. An irregular band was painted around the exterior of the rim of the vessel. Rather than images from traditional Pueblo art, the two complete images on this vessel recall the iconography of the Catholic church. The figure in the upper left closely resembles the "Sacred Heart" of Jesus (Dilasser 1999:22). The "Sacred Heart", which is often surrounded by flames or depicted covered by tears is emblematic of the Passion of Christ (Dilasser 1999:22). The five dots, painted within the heart and executed by single brush strokes, could represent either flames or tears.

The figure composed of a crude possibly quadrupedal body, has one foot displaying three claws. The other three feet are missing. Single lines are used to fill in the upper portion of the legs. The body is filled with a single line parallel to the lines that create the body and is crossed by four additional lines. The body ends in a "V" shape and lacks a tail. The head of the figure is roughly square and sits on a neck that was filled in by red paint. The face has eyes, eyebrows, a nose and a mouth. Three triangles are pendent from the top of the head, while two other triangles protrude from the sides of the face.

Given the presence of another example of Catholic iconography on the vessel, an appropriate interpretation of this figure is a lion, a...
grotesque or possibly a combination of both types of image. The lion symbolizes supreme power and its privilege of dispensing justice. The lion evokes the image of the risen Christ and when pictured with a crown, the Lion of Judah or the Vanquishing Christ (Dilasser 1999:37). The three triangles on the top of the head could represent a crown, with the two triangles on either side of the head serving as ears. The five triangles together could also represent the “glory” or rays depicted emanating from the heads or bodies of divine personages in Medieval and Renaissance art (Palmer and Pierce 1992).

The image could also represent a grotesque. A grotesque is an artistic convention defined as a person whose lower half is bestial (Randall 1966). Frequently, the lower half of the bodies of grotesques were merged into other design elements, often with floral terminations (Pierce 1990:266). Grotesques, also known as droleries, were a common decorative motif in Western European Medieval and Renaissance art, most commonly found as marginal infillings in religious and secular texts (Randall 1966:8). Grotesques could also be used as decorative marginal elements including paintings, weaving, enameled designs on armor and ceramics (Kingery and Vandiver 1986:Plate XVI; Kossourowa 2000).

Previous studies addressing the appearance of Catholic iconography in Pueblo art have focused on its appearance in Colonial period and Pueblo contexts. The depiction of Catholic or European imagery on seventeenth century rock art and decorated ceramics has been interpreted as a form of resistance to Spanish domination (Liebman 2002; Mobley-Tanaka 2002). A similar argument for art as contestation has been made for the adoption of Los Matachines by contemporary Pueblo communities (Rodriguez 1996). However, the images derived from European Catholic culture painted on a brown ware vessel from a room in the Socorro church that was produced as much as a century or later after the Pueblo Revolt requires an alternative explanation. The designs on the vessel from Room 4 are unique in terms of contemporary decorated ceramics that were produced in the El Paso area. Like the fragments of the two candlesticks also recovered from Room 4, this vessel likely represents an object that was crafted especially for use in the church. The potter, a female member of the congregation, would have been familiar with Catholic iconography from images on the alter screen, or from missals or other liturgical texts and would have produced the vessel under the guidance of the priest.

But why would seemingly unrelated figures from Catholic iconography be painted on a ceramic vessel? Interpretive theory from art history may give us some idea of why images are important in Medieval and Renaissance religious contexts and perhaps why the Socorro vessel was decorated using images from Catholic iconography. Saint Augustine wrote that the soul takes the shape of (incipit configuari) the object of its attention and affection (De Doctorina Christiana 1.20.19). Prior to the discovery of the biological basis of sight, Plato described the act of seeing as performed by a visual ray projected by the fire that warms and animates the physical body and is most intense in the eyes. This ray, initiated by the viewer, creates vision when it touches the object. The ray travels back to the viewer and is stored in memory. For Plato, rather than the object catching the eye, the eye catches the object. Images, then, make a stronger more lasting impression on the individual, than do descriptions (Miles 2003:5-6). Given the importance of iconography to the process of religious conversion and reinforcing the continuity of religious practice, an object such as the red-on-brown jar from the Socorro church would likely have been on public display. The vessel would have been discarded when broken or replaced. Given the destruction of the Socorro church by flood in 1740, either of these options is a possibility.
ACKNOWLEDGEMENT

I would like to thank Joe and Ofelia Ledesma for allowing archaeologists to dig up their front yard during Rex Gerald's excavation of the Socorro church and for permission to photograph the two vessels from Room 4. I would also like to thank Meg Gibson, Curator at the Institute of Texan Cultures at San Antonio, for allowing me to photograph the vessels.

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Liebmann, Matthew J.

Marshall, Michael


Miles, Margaret, R.

Miller, Myles, R. and Beth L. O'Leary

Mobley-Tanaka, Jeanette

Palmer, Gabrielle and Donna Pierce
Peterson, John A. and David O. Brown

Pierce, Donna

Randall, Lillian M. C.

Rodriguez, Sylvia

Staski, Edward

Vierra, Bradley, J. Richard C. Chapman and June el-Piper (editors)

Walz, Vina
EXCAVATIONS AT THE WEST 800 INSTRUMENTATION SHELTER, TRINITY SITE NATIONAL HISTORIC LANDMARK, WHITE SANDS MISSILE RANGE, SOCORRO COUNTY, NEW MEXICO

INTRODUCTION

Trinity Site National Historic Landmark (TSNHL), Site LA 100,000, is located in the northern portion of White Sands Missile Range (WSMR). Several archaeological surveys have been conducted to inventory and evaluate the historic properties associated with the detonation of the first atomic bomb (Mendez et al 1996; Rieder and Lawson 1994, 1995). These properties include the various scientific instrumentation sites, wood posts that held the communication wires between experiments and personnel bunkers, the George McDonald Ranch house where the bomb was assembled, and base camp where all the personnel lived (Merlan 1997, 2001).

Based on data from these surveys WSMR developed a preservation plan for the TSNHL including one for instrumentation shelters. The missile range is continuing to implement its integrity-preservation plan for the Trinity instrumentation shelters within the National Historic Landmark. The phased plan, Integrity Preservation Field Report: Trinity Ground Zero Area Instrument Shelters (Slater 1996b) includes an architectural assessment, evaluation of archaeological resources, and stabilization activities, such as rebuilding the earthen berm that covers each shelter.

At the request of Mr. Robert Burton, White Sands Missile Range archaeologist, Human Systems Research, Inc. (HSR), prepared an archaeological testing plan for the West 800 Instrumentation Shelter at the TSNHL. It presented a research design and discussion on field and laboratory methodologies, artifact curation arrangements, treatment of human remains, and HSR personnel assignments. The plan proposed testing around the shelter and exposing the structural footings to identify the ground levels and artifacts associated with the Trinity test. This testing proposed to include metal detection to determine the extent of the artifact scatter around the shelter. It also proposed that work be conducted so that stabilization of the shelter may proceed without destroying associated artifacts, as recommended in Slater (1996).

The test excavations were conducted in October, 2000, by archaeologists and volunteers from Human Systems Research, Inc. (Table 1). David T. Kirkpatrick served as prin-

<table>
<thead>
<tr>
<th>Personnel involved with the test excavations.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Deborah Dennis</td>
</tr>
<tr>
<td>David T. Kirkpatrick</td>
</tr>
<tr>
<td>James Connelley</td>
</tr>
<tr>
<td>Delton Estes</td>
</tr>
<tr>
<td>John Fitch</td>
</tr>
<tr>
<td>Steven Phillips</td>
</tr>
<tr>
<td>Robert Pick</td>
</tr>
<tr>
<td>Sara Eidenbach</td>
</tr>
<tr>
<td>Gail Wimberly</td>
</tr>
<tr>
<td>Nancy McMillian, Ph. D.</td>
</tr>
</tbody>
</table>
cipal investigator and directed the project. This project provided an opportunity for volunteers to assist with the field work and laboratory analysis. Their work contributed to the success of the project. Prior to the field work, safety training was provided by a member of the Environment and Safety Directorate, White Sands Missile Range.

This paper will present a description of the West 800 Instrumentation Shelter, the Fastax camera recording area, and the gamma sentinel experiment based on archival information and the results of the test excavations. The recovered artifacts will be briefly described. Information about the testing plan, excavation methods, artifact analysis, and a detailed photographic description of the events at West 800 Instrumentation Shelter are presented in Kirkpatrick (2001). A history and an illustrated description of the various experiments associated with the Trinity explosion are presented in Trinity Experiments (Merlan 1997).

WEST 800 INSTRUMENTATION SHELTER

West 800 (800 yards west of Ground Zero) was one of the numerous locations for various different types of experiments associated with the first atomic explosion (Figure 1). Seven experiments were identified as being conducted at West 800 (Merlan 1997: Appendix 3). These included gamma sentinel (Types A and B) and high speed Fastax camera equipment. West 800 consists of the concrete instrumentation bunker, Fastax camera location (Feature 1), a Type B gamma sentinel area, an artifact scatter, and a borrow pit (Figure 2).

A very sparse artifact scatter surrounds the shelter. Fragments of concrete are to the south of the shelter, possibly left over from pouring the walls and roof and/or the small slab. West of the shelter, a few fragments of glass and small amounts of building debris were observed. The large depression to the east of the shelter is probably the source area for the fill used to cover the shelter. Limited test excavations conducted by Mendez (1997) showed the presence of subsurface artifacts.

The West 800 Instrumentation Shelter, Area 3 of the TSNHL, is a concrete shelter (Figures 2 and 3). Merlan (1997) located construction plans for “Bldg. No. 1, 11-1-44” in the archives at Los Alamos. By comparing the plans with the integrity-preservation records, West 800 and North 800 Instrumentation Shelters appear to have been built following the “Bldg. No. 1” plans.

West 800 Instrumentation Shelter is a cubical chamber of reinforced concrete surrounded by an earthen berm (Figures 3 and 4). The interior room measures 6 ft square by 6 ft-7 inches high. Large, concrete pipes are set into the berm, protecting the chamber’s three viewing ports aimed toward Ground Zero. A fourth viewing port of steel pipe
Figure 2.
Map of West 800 showing features and excavation units (Kirkpatrick 2001: Figure 8).

Figure 3.
West 800 Instrumentation Shelter, architectural plan and section.

Figure 4.
West 800 Instrumentation shelter before the explosion (Courtesy of Los Alamos National Laboratory, TR 406)
is set into the south wall at an angle. Originally, the chamber was covered with 2 ft of earthen fill and a 12 inches thick concrete slab was placed on top. Extending off the west wall of the chamber is a plank retaining wall for the fill. Two plank walls form an entrance hall leading to the door in the west wall. A concrete slab is adjacent to the south wall and has four \(\frac{3}{4}\) inch bolts in each corner. The function of this slab is unknown as it was covered over with dirt prior to the blast. The viewing ports were also covered with dirt.

The shelter was designed for high-speed, Fastax cameras to record the event through the three concrete-pipe viewing ports. The decision was made later to mount the cameras outside the shelter on sleds attached to cables for retrieval (Merlan 1997:11). The Fastax camera, on the south side of the shelter, was housed in a metal box sitting on a sled which was on a concrete pad (Figure 5). A low wood retaining wall was on the north. Cables attached to the framework would allow the camera sled to be towed a safe distance away after the blast. Air movement from the explosion moved dirt from the shelter, uncovering parts of it and partially covering the camera location (Figure 6).

Another experiment conducted at the West 800 Instrumentation Shelter was the measurement of gamma rays to record energy release resulting from the explosion (Merlan 1997:22-28). The purpose

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**Figure 5.**

Fastax camera.
(Courtesy of Los Alamos National Laboratory, TR 782)

**Figure 6.**

West 800 Instrumentation Shelter and Fastax camera after the blast.
(Courtesy of Los Alamos National Laboratory, TR 366).
of the sentinels was to provide information to personnel on the radiation danger level before entering into an area. Two types of gamma sentinels were used; Type A used photographic film, and Type B was an ionization chamber. Little is known about the Type A gamma sentinel and no physical remains were found at West 800.

The remains of a Type B gamma sentinel box are located to the southwest of the shelter (Figure 2). The box was a wood frame set on a concrete base that had a vertical metal bar with a hole for a bolt (Figures 7 and 8); the sides were covered by dirt. The ionization chamber sat inside the box. The
top of the box was covered with a lid and a counter-weight that controlled the opening of the box. The blast moved fill from the sides of the box and broke the counter weight (Figure 9).

**EXCAVATION RESULTS**

The excavations focused on the south side of the shelter (Figure 2). Three excavation units were completed, although Feature 1 included portions of several units. The units were placed near the Gamma Sentinel (Type 2), an area to the southwest of that feature, and in the vicinity of the Fastax camera. In looking for the Fastax camera pad, an excavation unit was placed to the south of what was thought to be part of the wooden retaining wall visible in several historic photographs (Figures 5 and 6). The concrete pad was found to be partially covered with lead. In the middle was a square depression with the remains of a bundle of wires that connected the camera to other instruments through a buried pipe. The detailed results of these excavations are presented in Kirkpatrick (2001).

The trench on the north side of the shelter showed a profile of the wall. It appears that four concrete pours, each 24 inches high, were made to build the walls. The walls were poured on an existing concrete floor 6 inches in thickness. After the roof had been poured, the cap was then poured.

The location of the Type B gamma sentinel was identified by the exposed vertical metal with a hole in it for a bolt (Figure 7) and a scatter of wood. The feature itself was not excavated, but the area around it was excavated to find associated artifacts.

**ARTIFACT ANALYSIS**

All of the artifacts found on the surface and in the test and excavations fall within the historic dates pertaining to the Trinity Experiment. Melted metal fragments were collected from a concentration southeast and south of the datum. These irregular-shaped metal fragments have a shiny crust on the surface and are dark brown to gray-brown in color. A sample of these fragments analyzed by Dr. Nancy McMillian, Geological Sciences, New Mexico State University and was found to be lead.
Table 3
Artifacts by function.

<table>
<thead>
<tr>
<th>FUNCTIONAL CATEGORY</th>
<th>NUMBER</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Building Materials</strong></td>
<td></td>
</tr>
<tr>
<td>Nails: 2 in. Common 6d</td>
<td>1</td>
</tr>
<tr>
<td>2 1/2 in. Common 8d</td>
<td>35</td>
</tr>
<tr>
<td>3 1/2 in. Common 16d</td>
<td>2</td>
</tr>
<tr>
<td>Washers, one 3/4 in. with 1/4 in. hole and one 3/4 in. with a 3/16 in. hole</td>
<td>2</td>
</tr>
<tr>
<td>Hex nuts, 5/8 in. dia. with 3/16 in. hole</td>
<td>3</td>
</tr>
<tr>
<td>Lead fragments from covering placed on concrete camera pad</td>
<td>45</td>
</tr>
<tr>
<td>Gypsum on black paper</td>
<td>19</td>
</tr>
<tr>
<td>Wire ties used on rebar and other functions</td>
<td>5</td>
</tr>
<tr>
<td>Metal wire, 1/8 in. diam. x 18 1/4 in. long, probably a fastening device</td>
<td>1</td>
</tr>
<tr>
<td>Concrete with fine aggregate</td>
<td>2</td>
</tr>
<tr>
<td>Metal bar, 1 1/2 in. long x 1 in. wide x 3/8 in. high</td>
<td>1</td>
</tr>
<tr>
<td>Wood:— indentations and rust from metal pipe threads on inside curve of wood, probably was mounted at top of the wiring well to stabilize metal conduit pipe. —from wooden frame wall at north edge of Feature 1</td>
<td>7</td>
</tr>
<tr>
<td>—from unknown origin</td>
<td>6</td>
</tr>
<tr>
<td>Lead slab, measuring 8 x 8 x 1 in., weighing 26.5 lbs., found off south edge at west end of concrete pad</td>
<td>1</td>
</tr>
<tr>
<td>Melted lead with brown crust on surface, found in isolated scatter south of datum, possibly from the preparation of lead covering placed on concrete slab of camera pad</td>
<td>24</td>
</tr>
<tr>
<td><strong>Subtotal</strong></td>
<td>157 (69%)</td>
</tr>
<tr>
<td><strong>Scientific Materials</strong></td>
<td></td>
</tr>
<tr>
<td>Metal ring, 1 1/4 in. diam., with 1 1/4 in. long cotter pin attached</td>
<td>1</td>
</tr>
<tr>
<td>Rubber gasket ring, 1 3/8 in. diam. x 1/8 in. thick</td>
<td>1</td>
</tr>
<tr>
<td>Yellow crusty material, probably sulfur (has sulfurous smell)</td>
<td>2</td>
</tr>
<tr>
<td>Metal mounting plate, 11 in. long x 6 in. wide</td>
<td>1</td>
</tr>
<tr>
<td><strong>Subtotal</strong></td>
<td>5 (2.2%)</td>
</tr>
<tr>
<td><strong>Photographic Materials</strong></td>
<td></td>
</tr>
<tr>
<td>16-mm. movie film snips, in lengths from 3/4 in. to 6 in.</td>
<td>21</td>
</tr>
<tr>
<td><strong>Subtotal</strong></td>
<td>21 (9.2%)</td>
</tr>
<tr>
<td><strong>Electrical Materials</strong></td>
<td></td>
</tr>
<tr>
<td>Copper wire with plastic sleeve, 1 in. long</td>
<td>1</td>
</tr>
<tr>
<td>Copper wire, bare, in lengths of 5, 5 1/2, 6 3/4, and 8 1/4 in.</td>
<td>4</td>
</tr>
<tr>
<td>Electrical friction tape, hardened pieces, 1 3/4 x 1 1/2 in., and 1 3/4 x 1 in., originally forming a two-layered wrap</td>
<td>2</td>
</tr>
<tr>
<td><strong>Subtotal</strong></td>
<td>7 (3%)</td>
</tr>
<tr>
<td><strong>Ammunition</strong></td>
<td></td>
</tr>
<tr>
<td>30-caliber bullet</td>
<td>1</td>
</tr>
<tr>
<td><strong>Subtotal</strong></td>
<td>1 (0.04%)</td>
</tr>
<tr>
<td><strong>Materials Of Unknown Use</strong></td>
<td></td>
</tr>
<tr>
<td>Hard plastic, red/brown color</td>
<td>1</td>
</tr>
<tr>
<td>Rusted metal fragments found in Level 1 of excavation pit</td>
<td>22</td>
</tr>
<tr>
<td>Glass, slightly convex with opalescent patination on surface</td>
<td>5</td>
</tr>
<tr>
<td>Black, soft, greasy material, smears easily with knife blade</td>
<td>8</td>
</tr>
<tr>
<td><strong>Subtotal</strong></td>
<td>36 (15.8%)</td>
</tr>
<tr>
<td><strong>TOTAL</strong></td>
<td>227 (99.24%)</td>
</tr>
</tbody>
</table>
A total of 227 cultural artifacts was collected (Table 2). These consist of fragments of metal, 16 mm motion-picture film, glass, rubber, wood, concrete aggregate, mineral, cloth, and unknown materials. For analysis, the artifacts were placed into functional categories (Table 3). These categories consist of building, scientific, photographic, and electrical materials, ammunition, and materials of unknown use.

INTERPRETATION

This section provides a discussion of the artifacts, features, and historic photographs associated with West 800 Instrumentation Shelter. The research themes focus on the architectural characteristics of the West 800 Instrumentation Shelter, the nature of the remains of the experiments associated with the Trinity test, the physical set-up of the various experiments, and the daily-life activities of the personnel working at the shelter.

Research Theme 1: What are the physical remains of the different experiments including the Fastax camera and gamma sentinel

A review of the pre- and post-blast photographs shows only the Fastax camera and the Gamma Sentinel (Type B) experiments. The Gamma Sentinel (Type A), Excelsior, Condenser, and the Peak Pressure were not visible in the photographs. It is very likely that the remains of these experiments were removed from West 800 Instrumentation Shelter during cleanup activities after the blast. Physical remains of these experiments were not found during the testing project or the earlier project (Mendez 1997).

The Fastax camera was located on the south side of the shelter, according to historic photographs (see Figure 6). Excavations uncovered the concrete slab and remains of the wood retaining wall. Associated artifacts include fragments of 16-mm photographic film, construction materials, and a set of electrical and communications wires. The large-gauge electrical wires probably connected the camera to the 12-volt batteries to power the four cameras. The other wires were probably associated with timing devices to start the cameras.

The excavation units around the Gamma Sentinel (Type B) did not yield any additional artifacts. Today, the surface remains of this experiment consist of a few pieces of concrete, primarily wood fragments from the walls, and the metal eyelet that held a bolt that went through the lid (Figure 7). A nut was screwed to the bolt to hold the lid in the down position. When the nut was removed, the counterweight would drop and the unit was open. It is assumed that the concrete pad is still in place holding the metal eyelet.

Research Theme 2: What are the architectural details of the instrumentation shelter? How do these agree with and vary from the architectural plans?

The north wall of the shelter was exposed in a backhoe trench. The shelter walls were built by pouring concrete into forms that were 10 and 12 inches high. A total of four courses were observed for the 7 ft-8 inches high wall. The base of the wall slightly overlaps the floor, which may indicate that the floor was poured first and then the wall forms were placed on the floor. The roof thickness is 2½ to 3 inches. The concrete cap is a separate unit on top of the shelter roof.

The wooden wing wall was attached to two 8-by-8 inch posts at least 6 ft long. The bottom section has eroded away and is exposed. The wall was also braced by a deadman anchor that was buried in the dirt fill used to cover the shelter. The base of the deadman was found in the bottom of the backhoe trench.

The fill exposed in the walls of the trench did not show evidence of stratigraphy. Rather, the fill is massive and consists of poorly sorted materials, mostly on the finer side (loamy). Gravel and larger-sized rocks are not present. The exposed fill does not contain trash and other recent artifacts.
Research Theme 3: What were the spatial relationships and activity areas linking the various experiments with the shelter? Can these be documented archaeologically as depicted in the known photographs?

Two experiments, Fastax camera and Gamma Sentinel (Type B), are visible in the photographs. The remains of these experiments were documented during this project. The Fastax camera location was confirmed through excavations that exposed the concrete pad that supported the camera. The Gamma Sentinel (Type B) location, unexcavated, is marked by a metal loop that secured the tilting platform of the gamma sentinel. It was not possible to relocate the communications box using the metal detectors. The box may have been removed during project clean-up activities or post-blast road improvements. Future excavations may be able to locate the box or evidence of the box’s location.

Research Theme 4: What were the daily-life activities that occurred during the construction of the shelter, the setting up of the experiments, and the retrieval of the data and equipment?

Diagnostic artifacts associated with food or personal use were not found during the test excavations or the surface metal detecting activities.

CONCLUSIONS

The excavations near the shelter exposed the concrete pad for the Fastax camera, and portions of the wood retaining wall between the camera and shelter fill. Other excavation units contained only a few artifacts. The original ground surface was identified in the excavation units. A stratigraphic trench on the north side of the shelter revealed that covering fill was deposited en masse. The concrete bunker walls appear to have been constructed through a series of four pours over a previously poured floor. The roof was a separate unit. Overburden was obtained probably from the area to the east, where a small depression exists today. Modern road construction and maintenance have impacted the area north of the shelter. However, a buried communications line, possibly Trinity-era, was found at a shallow depth. A buried communications box may still be present in this area.

Most of the fill around the concrete structure is original material that was blasted away from the structure and later washed off the mound. This has covered up some of the original ground surface, except on the north side of the shelter. Artifacts are probably still present in a subsurface context.

ACKNOWLEDGEMENTS

I would like to thank Robert Burton and Michael Mallouf, for their assistance with the study at West 800. Thomas Merlan provided information about the various experiments at West 800 and the Trinity test. Much of the work could not have been accomplished without the help of the volunteers, John Fitch, Steven Phillips, and Robert Pick. Thank you for your time and effort in the field and laboratory.
END NOTES

1 Approved for public release: distribution is unlimited.
   OPSEC review conducted on January 24, 2005.

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Mendez, Lonnie

Mendez, Sergio, Jeanie Hart, Meade Kemrer and Dorothy Webb

Merlan, Thomas


Rieder, Morgan and Michael Lawson


Slater, Mary
My assignment for the Ladder Ranch Conference in 2001 was to contribute a paper dealing with the impact that the Mimbres Culture had on their environment. It was the first time that I have considered the question in a holistic way. I came up with more questions than answers and have undoubtedly only scratched the surface of this complex subject. Determining how the Mimbres affected their environment begs four major questions:

1. What is the range of archaeological sites that we can comfortably call Mimbres?

2. What is the areal extent that Mimbres (as defined) can be found?

3. What was the nature and range of the natural environment at that time?

4. In what manner did the Mimbres occupy, utilize and impact that range of environments?

Large questions, and I will endeavor not to bog down in them, but rather answer them as I see the answers to be, and then move on to the matter of impact. I will endeavor to limit my discussion to the time of the Mimbres Classic ca. A.D. 1000 - 1130).

WHICH SITES ARE MIMBRES?

I take a broad view as to which sites are Mimbres. While there is little doubt that the Mimbres Valley is both the geographical and cultural center of Mimbres, I see no reason not to include all those areas which boast sites/pueblos on which the dominant ceramic assemblage is composed of Mimbres Black-on-white, Mimbres Corrugated, and Mimbres Red Washed. So long as the complete spectrum of Mimbres ceramics are present and are in the majority, then, to me at least, the sites are Mimbres. I use this criteria because while I can accept the presence of just the Mimbres painted ware as intrusive or "trade" ware, I view the presence of the full ceramic spectrum as indicative of an intact archaeological culture.

WHAT ENVIRONMENTAL ZONES DID THE MIMBRES UTILIZE?

So, with the definition of what constitutes Mimbres resolved (probably to no one's satisfaction but my own), the territory and environments utilized/impacted by the Mimbres become much easier to define. In the geographical center of their world, the Mimbres utilized all or portions of three major river valleys, the Mimbres, the upper Gila, and a portion of the Rio Grande and all of its Black Range tributaries. The San Francisco, which boasts Mimbres sites at least in the Alma area and below, is considered to be a tributary of the Gila.

Other than the archaeological remains of Mimbres culture, these three drainage systems have two things in common. First, their headwa-
ters meet in the center of the mountainous area of southwestern New Mexico, diverging from the Black Range to flow south, west, and east-south-east respectively. Second, each of these drainages flow through three major environmental zones, each with a distinct set of resources. The zones, from highest to lowest have been defined on the Mimbres (Minnis 1985:77-81) as the Transitional (ponderosa), the Upper Chihuahuan (pinyon-juniper Grasslands), and the Lower Chihuahuan (desert scrub grassland). A central riparian zone, created by the drainages themselves, forms a connecting ribbon between these elevational zones of divergent vegetation. The Mimbres used all four, not just near the rivers edge, but high into the mountains and across the basin and ranges of the lowland deserts through which these drainages flow or empty into.

By dint of geographical fact and geological determination, these drainages emanate from a single source, like three spokes on a wheel, the ponderosa zone (or Transitional) is the smallest in area, followed in size and downslope in elevation by the pinyon-juniper (Upper Chihuahuan) and desert scrub grassland (Lower Chihuahuan) zones. The Transitional and Upper Chihuahuan zones utilized by the Mimbres lie in the center of southwestern New Mexico. These zones hold many resources, are comparatively well watered, and have excellent climates. They saw the highest density of population and accordingly absorbed the most human impact. The San Francisco River in the vicinity of Alma, the Rio Alamosa near Dusty, and the extreme northern headwaters of the Gila River mark the northern extent of Mimbres land use.

The Lower Chihuahuan Zone, located at the end of each drainage is many times greater in size than that of either the Transitional or Upper Chihuahuan zones. Accordingly, from west to east, the following broad basins were utilized: San Simon, Lordsburg Mesa, Playas, Animas, Mimbres, Macho, the Jornada del Muerto, and the Tularosa. These basins are defined by a number of desert mountains to include (again from west to east): the Peloncillos, Steins Peak, the Big and Little Hatchets, the Floridas, the Uvas, the Caballos, and the San Andres. Some of these ranges offer islands of Upper Chihuahuan and Transitional Zone vegetation, but little in the way of running water. This extent defines the realm of Mimbres land use on the west, south, and east. Admittedly the survey data from those portions of the Playas and Animas Valleys that are within the bootheel proper suggest that these areas were on the fringe of Mimbres activity (Lekson 1992). The extent and nature of Mimbres assemblages in northern Mexico is not well understood.

**WHAT WAS THE CLIMATE AND ENVIRONMENT OF THE MIMBRES AREA DURING THE 11TH AND EARLY 12TH CENTURIES?**

The brutal environmental truth of the last 10,000 years is that there has been a steady drying trend that shows little sign of letting up (Wells 1977). There has been a tendency to blame all stands of mesquite on the cattle which first began to really impact southwestern New Mexico only about 140 years ago (York and Dick-Peddie 1969:155-166). The fact is that the mesquite and other elements of the Chihuahuan Desert were already present 4000 years ago (Van Devender and Toolin 1990:104-133). The isolation of Sonoran vegetation associated with the Madrean Evergreen Woodland and the Madrean Montane Conifer Forests (Brown 1982; Pase and Brown 1982) in the Gila is another result of the steady climatic change that “like the mills of the Gods” slowly but steadily threatens what we perceive as the status quo. This change is staggered with wetter and drier cycles, each wet cycle sprouting not just seedlings but prosperity and hope in the human heart, encouraging agricultural pursuits and increased population. It is critical to keep in mind that while the Mimbres (like our-
selves) had an impact on their environment, they (like ourselves) were also at the mercy of both short term and long term changes over which they had no control.

Due to the timely work of the Mimbres Foundation associates (LeBlanc 1983) and the subsequent research by Harry Shafer at the NAN Ranch and Darrell Creel at Old Town, the Mimbres River is the only drainage of the three principal drainages from which there is a reasonably interpretable body of chronological and environmental data. Minnis (1985b) offered the first synthesis of environmental data recovered by the Mimbres Foundation. As a result, the general Mimbres period environment will be described using data from the Mimbres River with comparisons to both the Gila and the Rio Grande.

Rainfall

Tree-ring sequences from both the Reserve and Mimbres Valleys clearly indicate that the 11th century was, in general, a wet cycle (Grissino-Mayer et al. 1997; Minnis 1985:83-89). Even during that marvelous century of growth there were occasional drought years to contend with and by the late 11th century a series of drought years occurred. The early part of the 12th century was sporadic and finally, a serious drought began at A.D. 1130 (Creel 1996; Grissino-Mayer et al. 1997; Shafer 1999:98-99). Given the generally synchronic relationship of the tree-ring record between the Mimbres and Reserve Valleys, it follows that the Rio Grande and the Gila experienced similar although not identical weather patterns.

Hydrology

Macrobotanical, pollen, and faunal data recovered from Mimbres sites suggest that the Mimbres River was a slower and more meandering stream than we see today, with a fair amount of ponding (Minnis 1985:91-93). Specifically, the presence of muskrat bone, cattail pollen, bulrush seeds, and common reeds from prehistoric sites and a minute modern population of Chihuahua Chub (35 individuals in 1985) are the indicators that the Mimbres River has changed considerably since the 12th century.

It is likely that both the Gila and the Rio Grande were meandering streams with oxbow ponds as well. However, it is important to note that the Gila, the Rio Grande, and the Black Range drainages have very different characters than the Mimbres River.

The Mimbres River

Of the three, the Mimbres is clearly the smallest, the most controllable, and perhaps the most reliable. Once the East and West Forks join, the flood plain is of a reasonably consistent width all the way to below Old Town where the waters now sink into the Mimbres Bolson (Minnis 1985:75). Surface flow in this stretch is affected by volcanic formations which force groundwater to the surface (Creel 1996). The consistency of both the stream and its floodplain made this stretch the Mimbres motherland as more than a dozen sizable ruins are strung up and down its length (Anyon and LeBlanc 1984; LeBlanc 1983). The Mimbres River currently carries “a modest and manageable 7200 to 9400 acre feet per year” (Lekson 1992: 34) Below Old Town, only very good years would have carried waters to pueblos in the Deming area. And yet, the pueblos are there, all the way to the north end of the Florida Mountains (Blake and Narod 1977).

The Gila River

The Gila River in New Mexico is the very beginning of a very large river which today helps keep the swimming pools in Arizona filled. In the area once occupied by the Mimbres people, it runs more than 10 times the waters of the Mimbres at 89,000 to 141,000 acre feet annually. Its floodplain in the Cliff and Red Rock valleys is broad and fertile, but in other areas, the stream is narrow and swift, contained by steep escarpments. But in the Cliff and Red Rock valleys are numerous small
Mimbres sites and two (one in each valley) are 200-300 room monsters, if Lekson’s room estimates (1984; 1992) are correct. Sizable Mimbres sites are also found along Duck Creek, a large tributary to the Gila’s Cliff valley. High mountain cienegas in the upper Gila tributaries and Sapillo Creek provide subirrigated lands very suitable for agriculture. The upper tributaries are associated with Mimbres pueblos ranging from 5 to 50 rooms and even larger (Bradford 1992; Stokes 1999).

The Rio Grande

The Rio Grande is New Mexico’s big river running an approximate average of 1,200,000 acre feet at the San Marcial gaging station each year. Prehistorically that volume would have been increased as dams now block its flow. However the Rio Grande flow, which begins some 400 miles to the north, would have been different from that of the Gila and the Mimbres in another aspect other than volume. While both the Mimbres and the Gila experienced spring and late summer flooding, the pre-dam Rio Grande flow was even more intensely pulsed due to its length and the size of its watershed. As a result, the Rio Grande experienced alternating periods of radically increased discharge followed by periods of relatively low discharge (Ackerly 1998). The periods of low discharge included times when large portions of the river were absolutely dry. The periods of high discharge often resulted in abrupt changes in the channel across the broad flood plain. Oxbow lakes would have been formed and the Santa Barbara Valley north of Hatch probably contained isolated ponds and marshes as well as a bosque. During historic times, these ponds provided water for all sorts of wildlife, including bear and deer, but also were a breeding ground for mosquitos and encouraged the spread of sickness (malaria) (Wilson 1985: 40).

The Mimbres population of the Rio Grande would have been smaller than that of either the Mimbres or the Gila. This is likely due to the difficulties of managing the inconsistent flow and huge size of the Rio Grande. Small Mimbres sites are found along the Rio Grande north of Elephant Butte Dam while larger sites are found in the Santa Barbara Valley with the flagship site being LA 1082, the Rio Vista Site (Lekson 1992; Mayo 1994).

The Black Range Tributaries

The Black Range tributaries to the Rio Grande vary considerably in annual flow and in the consistency of that flow. The southernmost Black Range tributary is the Berrenda Creek while the northernmost, the Rio Alamosa, actually separates the Black Range from the San Mateo Mountains. Of these drainages, the Alamosa, the Palomas, and the Las Animas have the most consistent and longest flows today. Compared to the big three, their floodplains are limited. Surface flow is determined by dykes which force water to the surface. Summer rains can cause severe, intense flooding which reaches the Rio Grande. Otherwise, stream flows cease several miles upstream from the canyon mouth. These streams don’t meander much as the narrow canyons restrict such activity. As a result oxbow lakes are not formed. In the few areas that dykes bring water to the surface, subirrigation in the form of cienegas does occur. Mimbres pueblos ranging from 1 to 30 rooms are found from the confluence of these streams with the Rio Grande to the Transition Zone with the largest sites (from 50-100 rooms) located near surface water and floodplain (Laumbach 1982; Laumbach and Kirkpatrick 1983, 1985; Lekson 1985; Nelson 1999).

The Agricultural Potential of the Available Soils and Water

Lacking the pumps that irrigate many fields in southwestern New Mexico today, only basic forms of agriculture were available to the Mimbres, irrigation farming, dry land farming, and modified dry land farming (Minnis 1985:75-76). Irrigation depends on consistent surface flow, dry land farming depends on rainfall, and modified dry land farming depends on both rainfall and the ability to control run-off.
Irrigation Farming

Irrigation farming requires a consistent source of water. Each of the drainage systems provide such a source in vary quantities. Irrigation would be possible on the river floodplains and downslope from spring locations.

Floodplains

Floodplains tend to have the best agricultural soils and are best suited to irrigation. The evidence suggests that, with the exception of the Black Range drainages, all of the streams meandered during normal flow and all streams, even the Black Range drainages, flooded. Both flooding and ponding would have had the effect of recharging floodplain soils with nutrients. These deeply developed soils had the potential to support crops for many years and would have been the most difficult to deplete. Evidence of prehistoric irrigation farming on the Mimbres River has been largely destroyed by modern farming. Several researchers have documented irrigation features near the NAN Ruin in the Mimbres (Shafer 1999:99). The most complete set of irrigation features have been documented in the much drier Arenas Valley which flowed regularly until 1926 (Herrington 1979). These irrigation features include water diversions, ditches, and bordered fields.

Springs

Some spring locations could have been ditched or water simply carried to provide sufficient water for at least small fields. As pumping would not have lowered the water table, springs would have been more reliable than today. The agricultural potential of these locations would have been increased during periods of high precipitation as the springs would have been even more highly charged. Because springs encourage local vegetation, these areas usually have organic soils amenable to agriculture.

Dry Land and Modified Dry Land Farming

Survey data indicate that dry land and modified dry land farming was practiced in a wide variety of locations including naturally subirrigated cienegas, natural and artificial terraces, soil catchments on highly intermittent upland tributaries, tributary mouths, and the slopes of desert mountains (Eidenbach and Wimberly 1980; HSR 1992; Laumbach and Kirkpatrick 1983; Nelson 1999; Russell et al. 1998; Sandor 1983).

Terraces

Dry land farming in the mountainous areas of Mimbres occupation could have occurred in several locations. These locations include the broad terraces above the floodplain, highland terraces and mesa tops where short drainages create small catchments of developed soils, and artificial terraces created by lines of stones which form a catchment for both soil and runoff. The agricultural potential for all of these soils would be limited as the nutrients would not quickly recharge unless the soils were fertilized. All of them would be dependent on rainfall. Those at higher elevations might have an increased opportunity to experience rainfall.

Cienegas

Cienegas or subirrigated areas are present in the higher elevations and occasionally lower on the drainages where water ponds in stream-formed oxbow lakes or is pushed to the surface. These soils are highly organic and contain ample nutrients necessary for agriculture. However, the danger, particularly in the higher elevations with more rainfall, would be too much water. This would require planting crops on the margins in wet years.
Tributary Mouths

The mouths of tributaries both large and small in all of the drainages occasionally have alluvial fans with developed soils. Seasonal drainage in these tributaries could have been easily ditched or spread with terraces to irrigate crops. Irrigation of alluvial fans at the mouths of tributaries may have been particularly critical to successful agriculture on the Rio Grande.

Desert Slopes

The slopes of the desert mountains offer another opportunity to control run-off during periods of high rainfall. The soils in these locations are sandy and like the higher mountain areas contain catchments where soil development can occur. Whether or not Mimbres populations were farming these slopes has yet to be proven. Survey in the San Andres Mountains has located what appear to be the remains of jacal structures associated with a Mimbres ceramic assemblage and trough metate fragments (Eidenbach and Wimberly 1980; HSR 1992; Huntington 1914). Those same slopes undoubtedly supported corn agriculture 150 years later during the late El Paso Phase (HSR 1992).

Playa Locations

Lowland desert playa locations may have provided some agricultural opportunities. However these locations fill only sporadically and usually during late summer. While some playa soils may be arable, seasonally high temperatures and a lack of consistent moisture would have made agriculture in playa locations very difficult unless the playa were located near a slope where run-off could be controlled.

Variety and Abundance of Natural Vegetation and Wildlife

The vegetation and wildlife of the four zones during the Mimbres period would have been generally similar to that of today with some important differences in vegetation density and the presence of certain species of both vegetation and wildlife.

Transitional Zone

The highland transitional zone would have had a relatively cool climate and a shorter growing season than the lower elevations. Vegetation would have been grama grasses and oak brush with an overstory of ponderosa pine, Douglas fir, and pinyon pine. All of these species would have been denser than today. However the lack of fire control would have created zones where the relative density of brush and overstory would have varied cyclically depending on the extent and nature of fires.

As for wildlife, wolves, grizzly bears, and jaguar would have joined the mountain lion as large predators, creating a different balance than is seen today. Based on the archaeological record (Minnis 1985; Olsen and Olsen 1996), deer and mountain sheep seem to have been the largest common herbivores. Elk may have been present but are not commonly represented in the archaeological record. A few elk remains were found at Wind Mountain Site in the Burro Mountains (Olsen and Olsen 1996:404) and at Swartz Ruin on the lower Mimbres (Cosgrove and Cosgrove 1932:5). Elk do not commonly appear as motifs on Mimbres bowls. One such bowl resides in the collections of the University Museum at the University of Colorado at Boulder. That particular bowl depicts a bear trying an elk head on for size. Occasional bowl motifs depict bears (Cosgrove and Cosgrove 1932:4, Plate 225d), whether or not any of them are grizzly bear is conjectural. To my knowledge no jaguar are depicted on Mimbres bowls.

The Upper Chihuahuan Zone

The Upper Chihuahuan Zone would have been a pinyon-juniper grassland as it is today. However the stands of pinyon would have been denser and mesquite and other Chihuahuan Desert plant species would have been less dense or perhaps not present at all. In fact, the first mesquite in the record
of Mimbres Foundation excavations shows up in
the Mimbres Classic period (Minnis 1985:105).
Small stands of ponderosa pine and possibly even
Douglas fir would have been present in the shaded
canyons at lower elevations than today.

Dense stands of oak brush would have made this
area a particularly good deer habitat. Antelope
would have been more plentiful in this zone than
today and even a few bison might have been pre-
sent. Minor quantities of bison remains have been
documented at Swartz Ruin (Cosgrove and
Cosgrove 1932:3-4), Wind Mountain (Olsen and
Olsen 1996:404), on a Postclassic site in the
Palomas (Nelson 1999:156) and Emil Haury
reported a calcined bison associated with Mimbres
sherdson the Lordsburg Mesa (Laumbach
1976:27). Dipeso (1974: 244-245) states that
bison roamed the area near Casas Grandes,
Mexico, in apparent abundance during the prehis-
toric period.

The Lower Chihuahuan Zone

Lower Chihuahuan Zone vegetation would have
been much the same as today, except with very
different distributions. Juniper, and perhaps piny-
on, would have been found in greater quantity and
at lower elevations than at present. Grasslands
would have been more verdant with fewer or no
mesquite in some areas. The graveled creosote
zones would have been essentially the same as
today. The coppice dunelands stabilized by
mesquite would have been present. The extent of
mesquite at this time, however, is a matter of some
controversy.

Much has been made of the modern expansion of
the mesquite community due to overgrazing (York
and Dick-Peddie 1969). While there is no doubt
that mesquite communities have expanded in the
last 100 years, I believe that the case has been
somewhat overstated. The packrat middens tell us
that the mesquite was introduced to southern
New Mexico 4000 years ago (Van Devender and
(1980:15-16) argue that the historical data used
by York and Dick-Peddie was skewed by fraudu-
 lent surveyors. The dating of soils in the northern
Chihuahuan desert revealed a period of aridity
about 2200 years ago that dramatically increased
the desert scrub community, including mesquite
(Monger et al. 1998; Buck and Monger 1999).
Archaeological data from hearths found in the
Lower Chihuahuan Zone across the Mimbres and
neighboring Jornada and Tularosa Basins of south
central New Mexico as well as on the Lordsburg
Mesa south of the Red Rock Valley of the Gila
River all contain mesquite charcoal and produce a
range of radiocarbon dates beginning early in the
first millenia if not before (Brethauer 1978:147;
Laumbach 1976: 57-63; Laumbach and Duran
1988: 145-147; Shafer et al. 1999:299). Further-
more many of these hearths were dug into aeolian
sand, indicating that dunes already existed
(Laumbach 1976: 30). A survey on the mesa west
of El Paso revealed a direct correspondence
between the distribution of mesquite dunes and
the presence of prehistoric sites (Sudar-Murphy
and Laumbach 1975).

During the Mimbres Period mesquite was not
restricted to just areas of high water table.
Coppice dunes hold moisture quite effectively.
Large areas of high coppice dunes with dense
stands of mesquite was already well established by
the Mimbres era. What we don’t know is just how
large those dunal areas were at A.D. 1000.
Modern expansion of mesquite has been into pre-
viously non-dunal grassland areas (Buffington and
Herbel 1965; Gibbens et al. 1983; Hennessey et
al. 1983). As a result, modern mesquite dunelands
have formed in deep sandy soils while mesquite
expansion into areas of heavy, shallow, or gravelly
soils has not resulted in dune formation.

The prehistoric dunal areas also supported four-
wing saltbush and numerous wild grasses, includ-
ing those of the dropseed family. The grasslands
would have supported antelope and jackrabbits
while the dunal areas would have been habitat for cottontail and quail. These dunal environments were an important resource area for the Mimbres population.

The rocky mesa tops and slopes near the drainages in the Lower Chihuahuan zone would have supported dense stands of creosote just as they do today. There is little evidence for Mimbres period use of this vegetation zone. Interestingly, the most consistent temporally diagnostic artifacts found in this zone in western Sierra County are Early and Middle Archaic points, suggesting that the area might have been a grassland prior to 4000 B.C. (Laumbach and Kirkpatrick 1983).

Just as the Arenas drainage flowed regularly until 1926 (Herrington 1979), desert springs would have generally flowed better than today and now intermittent arroyos would have flowed more frequently because the water table had not been lowered by drilling. Oral history from early ranchers suggest that now dry arroyos in the San Andres Mountains had a sustained, if seasonal, flow at the turn of the century. Away from the drainages, the playa bottoms would have filled during wet years, supporting a greater range of plant and animal life.

**The Riparian Zone**

The riparian zones would have had denser stands of cottonwood than are seen today. Willows and reeds could be found in the slower and ponded areas of the river. Game of all kinds could be found in the resulting bosque. It would have been a particularly good cottontail and deer habitat. All three drainages would have been part of a flyway for migratory birds and waterfowl may have been hunted. More and larger fish would have been present (eg. catfish and trout) in the Gila and Mimbres. The Rio Grande would have had a wider array of fish and probably denser flocks of water fowl. Beavers and muskrat were plentiful and contributed to the slower flow and extended width of the river through dam construction. On the Mimbres and Gila, at least some of the floodplain environment would have already been affected by the pithouse period occupation.

**AIR QUALITY**

For the most part, air quality would have been even better than today. Sandstorms and unrestricted forest fires might have created temporary problems in air quality.

**HUMAN SANITATION**

The assumption is that Mimbres populations would have used abandoned structures or designated areas near the pueblo as latrine areas. Little evidence has been documented.

**IN WHAT MANNER DID THE MIMBRES OCCUPY AND IMPACT THE RANGE OF ENVIRONMENTS?**

*Land Use Models*

The answer to this question depends a great deal on which model of Mimbres land use one puts the most faith in. In the early years of southwestern archaeology, pueblo agriculturists were viewed as completely sedentary, with each valley occupied for generation after generation and leaving a sequence of archaeological remains which reflected the long term occupation. In this scenario, population movements were seen as major events that usually marked the end of an era. LeBlanc (1983) initially outlined such a stable sequence of occupation and growth for the Mimbres.

Since at least the 1980's, a number of archaeologists have suggested that perhaps movement was much more common and that the seemingly stable records of occupation that we see in the various river valleys were actually produced by inter-
mittent populations which occupied the valleys for only portions of each of the definable archaeological periods. As most of these archaeological periods are more than 100 years in length, such a scenario is quite plausible. I will try to briefly summarize some of these models here:

Stuart and Gauthier, using their basic theme of power and efficiency, suggest that the Mimbres population dispersed in reaction to environmental stress at the end of the pithouse period, reaggregated during the Mimbres Classic only to disperse again at the end of the Mimbres Classic (1981:198).

Nelson and LeBlanc (1986) developed a model of short term sedentism to explain the Salado period occupation of the Mimbres Valley. The model of short term sedentism involves "the strategy of a single community, maintaining an aggregated residence pattern surrounded by relatively large open territory, and moving its residential base from valley to valley within that territory on cycle of 20 to 75 years" (Nelson and Anyon (1996: 277).

Nelson and Anyon (1996) applied the "fallow valley" model of short term sedentism to the post Mimbres pueblo periods of the area. They suggest that a relatively small pueblo population simply moved from valley to valley in response to changes in the climate and resource base available during any particular period. They question the perceived density of Mimbres population during the Classic Period and suggest that the Mimbres population might have been somewhat lower and more mobile than commonly thought.

Lekson (1985,1992:132) applied an Apache model to the Mimbres data and suggested that a core population of agriculturists were bolstered by seasonal hunting and gathering over a wide area of Southwestern New Mexico thereby explaining the distribution of Mimbres Whiteware in the archaeological record.

Shafer (1999:99) suggests that certain Mimbres families with claim to prime agricultural lands in the Mimbres drainage were permanent residents while others with no such claims moved in and out of the valley in response to climate, crop production, and the availability of crop production or other resources elsewhere.

For the Black Range Mimbres Classic populations, Nelson (1999:164-166) sees a relatively stable pattern of farming villages supplemented by field houses and extensive foraging as an adaptive response to the limited amount of irrigated floodplain. This pattern changes during the Postclassic as the Mimbres villages disperse and the population organizes in small hamlets with a high degree of residential mobility focused on extensive cultivation and dependence on local resources.

**THE IMPACTS**

Obviously the level of impact in any particular area would have been reduced if some or all of the Mimbres population were not in residence for some portion of the year. However the nature of the impacts would be the same in any case, just more or less amplified by the different strategies. Minnis (1985b) discusses most of the potential impacts in the Mimbres River area. Impacts on the Rio Grande, Gila, and the deserts of the Lower Chihuahuan Zone would have varied with the size of the respective Mimbres populations inhabiting those river valleys and the diverse nature of those rivers as discussed herein. The Rio Grande Mimbres population appears to have been smaller and would have had to deal with a less consistent flow in the river. The larger Gila Mimbres population would have had a consistent flow for irrigation that would have been more difficult to manage than the Mimbres River due to its greater volume.

Adverse effects on the environment tend to be linked just as the natural environment is linked. If
you affect the soils, then vegetation is affected, if vegetation is affected, then animal populations are affected, and so on. Most of what follows is guess work based on related archaeological data.

**Air Quality**

Air quality was probably not a problem, although the valleys would certainly have been full of smoke on a winter morning. More critical might have been ventilation within structures and its effect on the Mimbres population in terms of both lung and eye problems (glaucoma). The depopulation of the Mimbres River as projected at the end of the Mimbres Classic would have had an immediate positive effect on air quality.

**Human Waste**

Human waste undoubtedly created odor, drew flies and scavengers, as well as contributed to the spread of disease. Death rates by age at Galaz Ruin (Anyon and LeBlanc 1984:175) indicate that the incidence of infant and child mortality equaled that of adults with adolescents being the least affected. Whether or not any of these deaths were linked to diseases associated with human waste is unknown. However, movement of populations between valleys would have encouraged the spread of disease if present.

**Riparian Vegetation**

Floodplain vegetation may have been severely impacted by field development and harvesting of wood for both firewood and architectural elements (Minnis 1985:91). Whether or not the Mimbres occupation of the river valleys was continuous, it was certainly intense and repeated. Shafer (personal communication) feels that “the main change [in the floodplain] would have been a greater surface area and higher evaporation rate due to irrigation. This would have lowered the stream flow somewhat”. Also eliminated would have been areas which had been previously subirrigated by the spreading and ponding of the water. Despite Loren Potter’s opinion that clearance of floodplain trees would have had little effect on flooding (Minnis 1985:91), I believe that removal of dead wood and living trees would have increased the speed of the river and encouraged floods to cut deeper direct channels just as it did with Hohokam floodplain in Arizona (Redman 1999:151). This would have served to reduce ponding, effectively eliminating habitat for certain animals (muskrat), vegetation (reeds, willows), and fish (Chihuahua chub). While this may have been true on the Mimbres and the Gila, I have to question the level of such impact on the Rio Grande due to lower estimated population levels.

Field development may have also reduced cottontail habitat and increased that of jack rabbits (Minnis 1985:93). My own experience with fields is that cottontails like them just fine, but, in the desert, all that green is irresistible to jackrabbits that normally would be out on the high lonesome. Cottontails are also preferable to jackrabbits in terms of tenderness and may have been more aggressively hunted. Sanchez (1996) and Nelson (1999:158) discuss the effect of elevation on relative frequencies of jackrabbit/cottontail populations. In short, cottontails are more frequently found at higher elevations with dense vegetation but can thrive at lower elevations (as they did on the Palomas) if the riparian vegetation is maintained.

Fields would also have encouraged weedy species such as goosefoot, pigweed, and purslane. Minnis (1985b: 93) feels that archaeological record does not adequately reflect this latter phenomenon simply because these plants were edible and are naturally dispersed, resulting in a consistent presence through time. Nelson (1999: 150) discusses a similar dilemma in interpreting sites on the Palomas. However, Nelson’s (1999:152,164) data clearly indicate that neither the riparian wood species on the Palomas or the floodplain fauna were never depleted to the same extent as on the Mimbres, as those species remain dominant during both the Classic and Postclassic.
Soils

The floodplain soils would not have suffered greatly through agricultural depletion of nutrients due their depth and quality. However, channelization of the floodplain due to removal of vegetation would have meant that some areas would not have been recharged with fertile soils through flooding or ponding. Shafer (2003:132,221) feels that given the channel cutting documented for the Gila River, channel cutting in the Mimbres River is very plausible and may have been the critical factor if irrigation gates were destroyed as a result. As stated, given the lower projected size of the Mimbres population on the Rio Grande and the nature of flooding on that river, it is doubtful that the flood plain vegetation or the soils were impacted to any great extent.

Soil depletion at the various types of terrace locations that supported dry land or modified dry land farming could have been a significant factor depending on the length of time such locations were farmed. Sandor (1983; 1992:228) clearly shows that depletion of vital soil nutrients such as organic content, nitrogen and phosphorus occurred in artificial terraces associated with Mimbres dry land farming on the upper Sapillo drainage. These terraces appear to have been purposely placed in areas of old volcanic based soils with a high clay content (Sandor 1992:222). The terraces would have served to trap similar soils from above the terrace and in that manner concentrate the vital nutrients. Nutrient analyses did not suggest fertilization. It follows that, without fertilization, all of the terrace locations and natural soil catchments used by the Mimbres population would have quickly suffered the same sort of depletion to varying degrees.

Erosion of soils sometimes resulted from the terracing even though the Mimbres were encouraging soil catchments. Sandor (1992:226) references a study by Hubbell and Gardner (1944) which concludes that sedimentation like that derived from agricultural terracing was especially detrimental to blue grama. Sandor (1992:226) states that the resulting sparse grass cover on old cultivated areas is associated with sheet and gully erosion. Such erosion cuts through old stable soils that required thousands of years to form. Gullies cut through terraces and obvious attempts to patch terraces suggest that the erosion in the upper Sapillo drainage was begun by agricultural impacts.

Ditching or otherwise directing run-off on even more vulnerable desert slopes had the potential of creating new channels for arroyo development. Field preparation on desert slopes may have denuded the natural vegetation of shrubs and grasses, leaving the fragile soils of the desert slopes more vulnerable to erosion from hard summer rains. Eidenbach and Wimberly (1980: 75-81) see similar erosional and vegetative patterns at the Huntington Site, a large architectural site with an extensive Mimbres ceramic assemblage located on the eastern slope of the San Andres Mountains. They suggest that the observed patterning is the result of old agricultural fields.

Non-riparian Vegetation

Wooded species in each of the zones would have been reduced over time as firewood and construction materials were gathered. It is doubtful that the Mimbres population had the devastating effect on the wooded areas that late 19th century mining did (Minnis 1985:96-97), but a long lived local population rapidly depletes available firewood, even though flooding does replenish the supply of dead wood. Reduction in these wooded species would have increased the volume and speed of seasonal run-off and encouraged arroyo cutting.

Upland vegetation such as ponderosa pine, Douglas fir, and mature pinyon pines that were isolated in the shaded canyons of the Upper Chihuahuan zone would have been removed as construction material. The effect would have been to restrict their presence to higher elevations
than they are found today. Furthermore the larger, construction quality, pinyon and Juniper would have been depleted, forcing lowland builders to go ever higher to find materials, Creel (1996:2) documents the increased use of ponderosa pine and Douglas fir late in the sequence at Old Town. He suggests that builders would have had to travel some 16 km. for ponderosa pine and even further for Douglas fir. Creel further comments that if this surmise is correct, it substantially strengthens the possibility of intense depletion of riparian species as advance by Minnis (1985b:85-91).

Construction materials from the Berrenda Creek site on the upper Berrenda Creek in the southern Black Range included juniper, pinyon, and Douglas fir. Located well into the Upper Chihuahuan zone, the Berrenda site may have had close access to Douglas fir. A non-cutting date of A.D. 1105 from a pinyon sample suggests construction very late in the Mimbres Phase. Farther north in the Black Range on the middle and lower Palomas, Nelson (1999:153) reports that local species, primarily cottonwood and juniper are the dominant construction materials.

Introduction of Chihuahuan plant species to higher elevations and areas not previously vegetated by those species undoubtedly occurred. Human transportation and use of plants would have created pockets of introduced vegetation.

The first mesquite reported in the Mimbres Foundation’s excavation records is during the Mimbres Classic period (Minnis 1985:105). Mesquite is second only to cottonwood as the dominant wood in hearths in both Mimbres Classic and Postclassic hearths in the middle and lower Palomas drainage in the Black Range (Nelson 1999:147). As discussed, extensive collection of mesquite beans by Mimbres populations is suggested by numerous sites in the dunal areas of the Lower Chihuahua zone. These beans, with their high grape sugar content, were undoubtedly transported into highland areas where mesquite was not then common. Within the Lower Chihuahuan zone, the range of mesquite could have been increased by Mimbres populations living in procurement base camps located in mesquite dune fields and foraging in the surrounding grasslands. Also, Mimbres populations hunting and gathering or practicing slope agriculture in the desert mountains would have increased the upslope range of mesquite in those areas.

Other plants including wolfberry, agave, and variety of grasses (eg. dropseed) were likely introduced to site areas in the same fashion. Some of these plants remain site indicators today. The records of the Mimbres Foundation show no evidence that the agave or century plant was utilized by the Mimbres despite its presence in the area today (Minnis 1985:102). Excavations at Wind Mountain (Miksicek and Fall 1996:295-306) report agave remains from two houses and a roasting pit with modern specimens growing on the slope below the site. Minnis (1976) noted that agave in eastern Arizona were found only on archaeological sites, suggesting that it was an import started from food remains.

Wildlife

Archaeological data suggest that wildlife was dramatically impacted by dense human populations in the Mimbres Valley. Deer and cottontail remains are found in much lower numbers than jackrabbit bone in the Classic Mimbres sites suggesting that both populations had been diminished. (Minnis 1985:108; Powell 1977). It is suggested that cottontail habitat would have suffered from field development. As previously discussed, an alternative explanation has been advanced by Sanchez (1996), who, using data from NAN Ruin and Old Town, argues that variation in elevation and local environment played a significant role in relative frequencies of cottontails and jackrabbits.

Deer populations appear to have been reduced on the Mimbres River or perhaps the recovered faunal remains simply do not accurately reflect hunt-
ing activities (Minnis 1985:108). Different patterns of butchering activity would provide significantly divergent results (Anyon and LeBlanc 1984:222). Deer were certainly common enough on the Black Range where they clearly outnumber antelope at Berrenda Creek (Gomolak and Ford 1976:126). Both deer and antelope are common on the Palomas (Nelson 1999:160-161).

As appropriate to their presence on the lower grasslands, Anyon (1984:221) remarks on the increased frequency of antelope in sites from the Lower Mimbres Valley. Gillespie (1987:39) observes the same phenomenon in his analysis of the faunal assemblage from Saige-McFarland on the Gila River where antelope outnumber deer by a ratio of 3:1. Antelope are ubiquitous in Mimbres sites at lower elevations and unlike the bison and elk, frequently join Mountain Sheep as bowl motifs. Gillespie (1987:39) comments that antelope would have been more susceptible to group hunting than deer. Having hunted both, I am not sure that I agree, but the Mimbres were obviously successful hunters of both species.

The other unknown factor is the effect of droughts and natural predators on deer, rabbit, and antelope populations. Deer and rabbit populations naturally cycle with climatic changes and with the cycles of natural predators. For example, drought would have limited fawn crops and increased wolf kill ratios. Even without drought wolves could have easily depleted the deer population.

Elk and Mountain Sheep are rare but present in the archaeological record. Elk would have been difficult to hunt with a Mimbres period reflex bow. However, so would Mountain Sheep and their remains appear consistently, albeit in small numbers. Elk remains are not commonly found in Mimbres sites (Minnis 1985:104; Nelson 1999:155). A few elk bone were identified at Wind Mountain (Olsen and Olsen 1996:404) and at Swartz Ruin (Cosgrove and Cosgrove 1928:4). It is doubtful whether either elk or sheep populations were significantly impacted by the Mimbres population.

Waterfowl are rare in most excavated Mimbres sites. Only four duck bones were recovered from sites of all periods excavated by the Mimbres Foundation (Anyon 1984:221-222). Bird bone is also rare on the Black Range sites (Nelson 1999:164; Gomolak and Ford 1976:126). However, Gillespie’s (1987:21) analysis of the faunal remains from Saige-McFarland on the Gila identified no less than ten species of waterbirds! These included Canada Goose, Mallard, Pintail, a rare Mexican Mallard subspecies, Bufflehead, Ruddy Duck, and two varieties of Teal. Last, but not least, was an American Coot, or mudhen. Clearly waterfowl were pursued with some vigor on the Gila and probably elsewhere when possible. The Rio Grande should have provided excellent duck populations in season. The total impact on waterfowl, given available hunting techniques, would have been minimal.

Turkeys are rare in most Mimbres sites with only a few specimens per site documented. Anyon (1984:221-222) comments that no turkey bone was recovered from Late Pithouse contexts by the Mimbres Foundation and only twenty-two turkey bones were recovered from all of the Mimbres Classic sites. Two individuals are reported from Berrenda Creek in the Black Range (Gomolak and Ford 1976:126). Turkey carcasses are occasionally found as interments under Mimbres floors (Shafer 1990:16). Most researchers have attributed the turkey remains found to wild rather than domesticated birds (Gillespie 1987:23) and even suggest that domestic turkeys were imported from Mexico. A recent analysis of turkey remains from Elk Ridge Ruin on the west fork of the Upper Mimbres River clearly indicates that wild turkeys were domesticated and raised in some quantity at that site (Morrison 2000). Elk Ridge is at a much higher elevation than most other excavated Mimbres sites, a factor which may explain the concentration of turkeys at Elk Ridge.
Fish bone is rare on most Mimbres sites because it easily deteriorates and is difficult to recover. Minor quantities of chub and sucker bones were found at Galaz on the Mimbres. The Rio Grande and the Gila would have provided a wider variety of fish than the Mimbres. Residual pools on the Rio Grande during low flow periods would have been easy to fish. There is currently no data from the Rio Grande, but Gillespie (1987:30) reports 15 fish bones from Saige-McFarland on the Gila. While most remained unidentified, some are clearly the remains of rather large fish. The major impact to fish populations by the Mimbres would have been destruction of habitat resulting from flood plain alteration. Impacts to fish populations due to procurement would have been minimal.

There is one last debate that I am purposefully avoiding for lack of available references at the time of this writing. This concerns the non-native fish and animal species which appear as motifs on Mimbres bowls. Some have said that these motifs reflect extinction of riverine species in the Mimbres River. Others reply that the fish and animals are misidentified or relate to religions which originated in very different environments. My impression is that the latter is probably closer to the truth.

Clearly, Mimbres faunal assemblages vary significantly with differential preservation, elevation, and local environment. The broad outlines of that variation have not yet been completely drawn. This affects our ability to assess Mimbres impacts on wildlife.

IN CLOSING

In review, the primary impact of the Mimbres population seems to have been on the irrigable floodplains of the Mimbres and Gila rivers. These environments were the least fragile and were more able to sustain long term impacts than the dry land environments surrounding them. However, as those impacts became more and more concentrated due to population pressures, the opportunity for long term impact increased. Throughout the 11th century, Mimbres population growth and agriculture was encouraged by a relatively wet cycle. When that cycle ended abruptly, so did the over extended Mimbres lifeway.

Some of those impacts were short term while others were long term to the point that we can still detect them today. The wildlife, some plant species and riparian woods could recover given time. Conversely, other resources, such as soils that had taken thousands of years to develop on the terraces and in the floodplains, could not be replaced in a matter of one hundred or even 800 years. Small arroyos begun then are deeper and longer now. Lowland plant species (eg. mesquite) once introduced to higher elevations, could not be easily eradicated. Highland plant species (eg. ponderosa pine), once eradicated at lower elevations, could not easily be re-established in lowland climes while competing against the general warming trend of the American Southwest.

To briefly recap the situation:

The Mimbres Culture impacted three major river valleys and three major environmental zones differentiated by elevation and vegetation.

By A.D. 1000, the affected environment in each zone had already been impacted by the activities of Mimbres ancestors, but not at the level of population density and agricultural intensity reached during the Mimbres Classic.

Population density and land use on the major rivers during the Mimbres Classic, particularly the Mimbres and the Gila, was sufficiently intense to deplete the local resources and alter the floodplain habitat. This was probably less true on Rio Grande, but drouth and flooding would have been even more severe on that erratic drainage. Upland habitats adjacent to the highly populated drainages were also altered as construction material and firewood grew scarce.
Population density and land use on the smaller drainages (e.g., Palomas Creek) was not so intense as to cause major changes in either the floodplain or upland habitats.

The impact of the Mimbres population on any one area may have been somewhat lessened depending on the nature and mobility of Mimbres land use. Assuming that Mimbres land use was at least partially mobile, the impact of that mobility was the introduction of dry land agricultural techniques (terracing, catchment farming) into marginal areas of the highlands and the desert which encouraged both erosion and depletion of soils. Mobility combined with procurement activity also resulted in the movement and expansion of plant communities into previously foreign environments.

Despite the potential lessening of impact on the river valley flood plains that mobility allowed, a sufficiently dense population continued to impact the floodplains of the Mimbres and the Gila until irrigation farming was no longer possible during periods of severe drought. Drought and soil depletion also meant the end of effective dry land farming in marginal environments and substantially decreased the dependability of the waters in the Rio Grande. The population that was Mimbres dispersed to find new ways, the old Mimbres lifestyle shattered into sherds of its former self.

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When I was asked to contribute a paper to this volume I was working on a project on Southwestern jars. I was using information from the Awatovi excavations stored at the Peabody Museum, and from the book on the Hawikuh excavations (Smith et al. 1966). Richard Woodbury played a significant role in the formation of both of these data sets, and Natalie Woodbury was also one of the authors of the Hawikuh book, and so it seemed fitting to contribute a paper to this volume using this information.

In studying the prehistoric Yellow Ware pottery from the Hopi area, it appeared to me that jars, traditionally designated as water carrying jars, increased in size over time (AD 1300-1600), and perhaps became so big that their primary function was no longer to carry water. I further conjectured that if the new function of the jars was for use in kivas or other ceremonial contexts, perhaps there was not only a change in function but also a change in who made, or at least painted, the jars. For example, jars may originally have been made by women, but if use in kivas became important, perhaps men began to make them, resulting in new shapes, new designs, or both. While this idea was not especially likely, it seemed worth exploring.

I then asked, how do you tell when a jar was for transporting water? Simple common sense is helpful here. Very small jars would not be worth using to carry water, and very large jars, especially with shapes not suited to carrying on one’s head or tied as a bundle on one’s back, would also not be practical. Shape is also relevant, as jars with very small openings were unlikely to be used for cooking or storing solids. It is obvious, then, that critical variables for jars to be used as water jars would be the weight of the water they could carry, and to a lesser degree the shape of the vessel.

Rather than do a world wide ethnographic survey to help understand these variables, I felt adequate information was available from the Southwest. Surprisingly, few archaeological studies have been done involving jar volumes, and some of those, e.g. Nelson and LeBlanc 1986, focused on cooking jars. We know the size and shape of jars that were carried on one’s head from historic Zuni and Acoma as witnessed by various photographs including famous ones by Curtis (see Figure 1). In addition, jars from late

Figure 1.
Zuni women at a spring. Photograph by Frashers, ca. 1925. Courtesy of the Maxwell Museum of Anthropology.
Pueblo III times from Cibola (the Mariana Mesa area), and Tusayan (Antelope Mesa) have very small mouths and many of those from the Cibola area have indentations on their undersides that fit a thumb for balance (see Figure 2c and 2d), and it is clear they were used to support the jar when it was on one’s head. Clearly the historic jars and the PIII jars (Tularosa B/W, St. Johns Polychrome, and Jeddito B/O), were for carrying water. The weight of these vessels when laden should provide an estimate of how much water would have been carried in one vessel. While these estimates might not provide an absolute upper bound, as we may not have the largest of all such vessels in our collections, they certainly would provide a good idea of the weight range involved.

I, with the assistance of Jennifer Mimno, of the Peabody Museum of Archaeology and Ethnology, undertook a small study of jar volumes and weights. We created accurate profiles of a series of jars and then weighed each jar that was reasonably complete and not heavily filled with plaster. All jars were from the collections of the Peabody Museum except for four jars we profiled from published photographs from Hawikuh (Smith, Woodbury and Woodbury 1966: Plate 23). The Jeddito B/Y and Jeddito B/O jars are discussed in Smith (1971). An example of a Jeddito B/Y jar is shown in Figure 2B. The PIII Cibola jars came from the Upper Gila Expedition of the Peabody Museum (McGimsey 1980). The three historic jars from Zuni were collected by James Stevenson in the 1880s, and the historic jars from Acoma were collected early in the 1900s by A.V. Kidder. The Sikyatki Polychrome jars came from the Antelope Mesa and were from the sites of Awatovi and Kokopnyama. All the Peabody vessels can be looked up on the Peabody Museum Collections Online web site and

Figure 2.
Examples of Southwestern water jars: A, historic; B, Jeddito B/Y; C, Tularosa B/W; D, detail of thumb indentation on jar C; and E,F, Sikyatki Polychromes.
for those readers who would like more information about them, I have included their catalogue numbers in Table 1. We produced the profiles from photographs we took for this purpose. The photographs were taken at right angles to the jars and so good profiles could be obtained. As noted, we weighed relatively complete jars, but most jars that were profiled and the volumes estimated could not be usefully weighed because of their fragmentary nature. In all we computed estimates for 34 jars, listed in Table 1. (Other jars were recorded, but are not relevant to this particular inquiry).

**CALCULATING VOLUMES OF JARS**

In order to calculate the volume of the jars, we first obtained the height, width, mouth diameter, body thickness, and weight of the jar. Then a digital image of the profile of the vessel was taken. We put the digital images into Adobe Photoshop software with grid lines showing over the image. It was then easy to calculate the radius of the jar along its horizontal axis at any point by counting the number of grid boxes across each row from the midline to the edge. The photograph was scaled by using the actual measurements of the jar. This scaling was done by using both the orifice diam-

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<td>35-126-10/3621</td>
<td>Sikyatki Polychrome</td>
<td>17.7</td>
<td>1</td>
<td>37.3</td>
<td>22.8</td>
<td>10.2</td>
<td>9.4</td>
<td>20.8</td>
<td></td>
<td></td>
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<tr>
<td>37-111-10/10251</td>
<td>Sikyatki Polychrome</td>
<td>15.9</td>
<td>0.9</td>
<td>35.7</td>
<td>21.9</td>
<td>12.1</td>
<td>11.1</td>
<td>24.3</td>
<td></td>
<td></td>
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<tr>
<td>39-97-10/22463</td>
<td>Sikyatki Polychrome</td>
<td>18.5</td>
<td>1</td>
<td>32.6</td>
<td>27.6</td>
<td>17.7</td>
<td>16.3</td>
<td>35.9</td>
<td></td>
<td></td>
</tr>
<tr>
<td>36-131-10/6902</td>
<td>Sikyatki Polychrome</td>
<td>16.7</td>
<td>0.7</td>
<td>29.5</td>
<td>24.8</td>
<td>9.8</td>
<td>9.1</td>
<td>20.1</td>
<td></td>
<td></td>
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<tr>
<td>37-111-10/10001</td>
<td>Sikyatki Polychrome</td>
<td>17.1</td>
<td>1</td>
<td>43.5</td>
<td>22.4</td>
<td>18.5</td>
<td>12.6</td>
<td>38.7</td>
<td>10</td>
<td>48.7</td>
</tr>
<tr>
<td>37-111-10/10291A</td>
<td>Sikyatki Polychrome</td>
<td>19.6</td>
<td>1</td>
<td>39</td>
<td>27.8</td>
<td>19.8</td>
<td>18.3</td>
<td>40.3</td>
<td></td>
<td></td>
</tr>
<tr>
<td>36-131-10/8332</td>
<td>Sikyatki Polychrome</td>
<td>15.2</td>
<td>1</td>
<td>38.6</td>
<td>19.4</td>
<td>11.3</td>
<td>10</td>
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<td>7.5</td>
<td>29.6</td>
</tr>
<tr>
<td>36-131-10/8336</td>
<td>Sikyatki Polychrome</td>
<td>15.7</td>
<td>1</td>
<td>36.6</td>
<td>21.2</td>
<td>11.3</td>
<td>10.4</td>
<td>22.8</td>
<td>8</td>
<td>30.8</td>
</tr>
<tr>
<td>35-126-10/5499G.1</td>
<td>Sikyatki Polychrome</td>
<td>14.5</td>
<td>1</td>
<td>36.5</td>
<td>20.5</td>
<td>11.4</td>
<td>11.6</td>
<td>25.6</td>
<td></td>
<td></td>
</tr>
<tr>
<td>35-126-10/5357</td>
<td>Sikyatki Polychrome</td>
<td>15.8</td>
<td>0.8</td>
<td>38.1</td>
<td>25.9</td>
<td>13.7</td>
<td>12.7</td>
<td>27.7</td>
<td>5</td>
<td>32.7</td>
</tr>
<tr>
<td>NMAI 8-6401*</td>
<td>Matsaki polychrome</td>
<td>20</td>
<td>7.1</td>
<td>6.9</td>
<td>15.1</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>NMAI 8-1308</td>
<td>Matsaki polychrome</td>
<td>22.9</td>
<td>10.8</td>
<td>10.3</td>
<td>22.6</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>NMAI 8-9373</td>
<td>Matsaki polychrome</td>
<td>24.3</td>
<td>11.4</td>
<td>10.9</td>
<td>24.1</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>NMAI 6-8092</td>
<td>Matsaki polychrome</td>
<td>25.7</td>
<td>11.0</td>
<td>10.5</td>
<td>23.1</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

* NMAI references figure numbers in Smith et al.: 8-6401=Plate 23d, 8-1308=Plate 23f, 8-9373=Plate 23h, 6-8092=Plate 23i.
** Polychrome jar technically Sikyatki, but the red filler is minor and the shape is the same as other Jeddito B/Y jars.

Table 1
The water jar sample used in this study. Definitions of the column headings are given in the text.
ter and the height of the jar. If these two values resulted in different scale factors the average of the two calculations was used.

The volume of the jar was calculated by dividing the pot into “slices” (denoted by the grid rows in Photoshop). The thickness of the jar was subtracted from the radius, as it obviously would hold no volume. The volume of each slice was computed using the formula \( \pi r^2 h \). The height (h) is the height of each grid box. This step was repeated for each slice of the pot so that all the resulting volumes could be summed in order to get the total volume of the jar. This was the same logical procedure done with vessels described in Nelson and LeBlanc (1986), but without the aid of Photoshop.

The total volume of each jar was calculated, but this value was not an accurate estimate of the capacity because it was unlikely that anyone would ever fill a jar to the top. In order to adjust the measurements to reflect this, the first three volume slices (usually the neck of the jar) were subtracted from the total volume. Then, knowing that the weight of water is 2.2 lb/Liter, the weight of water that the jar could have held was then calculated and added to the weight of the empty jar, thus getting a reasonable estimate for the weight of the jar filled with water. (Although the dimensions given in Table 1 are metric, I have provided weights in pounds because it makes it easier to get an intuitive sense of what these weights would have meant to the users).

In this experiment, it is also important to consider how much of the total weight of a water filled jar is represented by the weight of the jar itself. We found that even with very well made thin walled jars, at least 15% of the total full jar weight was from the jar itself. A thicker walled, less well made vessel could represent over 25% of the load. If the source of water was some distance from the house, and water was carried once a day, and weight was a limiting factor in how much water could be carried each trip, then jar weight mattered. A thinner walled jar could result in a 10% increase in efficiency – that is, more water carried per trip. Such a difference would add up over time. In the summer, when water consumption would have been greatest and other activities competed for time and energy, making 10% fewer trips may have made procuring a well made, thin walled jar worth the effort.

Making a thin walled jar is about the most difficult potting task ancient people of the Southwest would have faced. Obtaining a well made, light, and strong jar would have been highly desirable from a practical, efficiency point of view, along with other possible social reasons.

THE ANALYSIS

Having obtained these weight values, it was possible to compare shapes and weights between different pottery types. We could be quite sure a dozen of the jars were for carrying water; these were the historic jars (an example is shown in Figure 2a) and the PIIII jars (Tularosa B/W, St. Johns Polychrome, and Jeddito B/O, an example is shown in Figure 2c). The historic jars weighed on average 35 lb including water and the jar itself. On average they would have held about 28.35 lb of water each. The PIIII jars were very similar, weighing on average 32.5 lb, water and jar, and holding 26.35 lb of water. Also, and perhaps more relevant, the largest historic jar would have weighed almost 49 lb fully laden, and the largest PIIII jar about 41 lbs.

Because of the fragmentary nature of many of the jars, it is easier at this point to discuss the weight of the water they could have carried, than their total weight. We know that jars that were surely used to carry water were quite heavy. The largest historic jar held 38.6 lbs of water and the largest PIIII jar held 34.4 lbs. That is, the largest jars that we have high confidence were used to carry water held around 35 lb of water (and approached 40 lb).
We can now turn to some other prehistoric vessels. A set of four Matsaki Polychrome jars averaged 21 lb of water and the largest held 24.1 lbs. The largest Jeddito B/Y jar held 37.8 lb of water, and the next largest held 27.9 lbs. The largest Sikyatki Polychrome jars were quite large. The largest could have held 40.3 lb of water, another held 38.7 lb, another 35.9 lb, and a fourth 30.8 lbs.

Sikyatki jars are noted for being very thin walled, so the question arose how the total weight of the different styles of jars compared. We were only able to get an accurate weight for three of the large Sikyatki jars. The one holding 38.7 lb of water weighed 10 lb, so had a combined weight of 48.7 lbs. The other two Sikyatki jars for which we could obtain weights were some of the smaller jars of this type, and their combined weights (water plus jar) were very close to 30 lb each. Thus, the very large Sikyatki jars were truly large and heavy.

However, as noted above, the heaviest Historic jar weighed 48.6 lb and the heaviest PIII jar 40.9 lb. Moreover, the largest Sikyatki jar, which held water weighing 40.3 lb, held only 2.5 lb more water than the largest Jeddito B/Y jar and only 3.9 lb more than the largest PIII jar. Even a slight difference in how full these jars might have been filled would bring them all into essentially the same weight range. That is, the laden weight of the largest Sikyatki jars is not appreciably larger than other water jars from the Southwest. The Sikyatki jars are not uniquely different in their size or laden weight.

It should be noted that one possible variable in the size of these jars is that the potters may have made jars that were slightly larger than they had anticipated. It must have been difficult to make jars with exactly the capacity one intended before production. Even if a jar were slightly larger than what is practical to carry when filled, it does not mean that the jar was ever filled to capacity as long as the appropriate amount of water reached its destination.

In summary, no class of prehistoric jar seems to be significantly larger than others, and no single jar appears to have been designed to be so large that it would not have functioned adequately as a water carrying jar.

A final note on jar weight and vessel thickness is in order. It turns out that the PIII jars, the historic jars and the Sikyatki Polychrome jars were quite similar when evaluated by the proportion of the laden jar weight that was the jar. The PIII jars had ratios of 0.16 and 0.22 jar weight to total laden weight, the historic had ratios had an identical range of 0.16 to 0.22, while the Sikyatki jars ranged from 0.15 to 0.26; interestingly the largest Sikyatki jar had a ratio of 0.21, and so it was not the thinnest walled vessel.

One additional and unexpected outcome of this work was a new understanding of the shape of the Sikyatki jars. The most efficient shape would have been like that of the rather spherical PIII jars. A sphere gives the greatest volume for the least surface (and therefore the least material). Thus, this shape should have had the lowest vessel weight to water weight of any shape. However, such jars needed thumb rests to be held in a stable fashion. Several of the prehistoric Hopi villages, especially those on Antelope Mesa, were on mesa tops and springs would have been far below, requiring trips up difficult trails carrying laden water jars. The late prehistoric period Hopi potters solved the carrying stability problem by a very different approach than the PIII potters. By flattening the profile of the jar, the edge of the maximal diameter forms a shelf like surface, which could be easily held. This was verified by actually placing jars on Jennifer Mimno’s head, (all while wearing latex gloves and placing an inert barrier between her hair and the bottom of the jar). This shape, sometimes casually referred to as the “flying saucer” shape, results in a jar with a low center of gravity that is easily and comfortably held on the head. While the lack of a more spherical shape...
was a drawback to this approach, it is a viable solution to the stability problem.

Jeddito B/Y jars are not quite as large as the later Sikyatki jars, and the one for which we have a jar to water weight ratio was quite thick walled. It would appear that there was an evolution toward a flatter shape and with more effort given to have lighter jars over time. Based on all this evidence, the change in shape in the Sikyatki jars does not appear to be a response to using the jars for activities other than transporting water (although it does not preclude other functions), but rather it seems to be a response to the need to carry large, heavy jars, sometimes up steep trails, with an ability to get a sure grip on the jar when needed.

In collecting this data another question arose. What appears to need an explanation is the large number of jars that are significantly smaller than the largest jar in each type. What were they used for? Were these smaller jars also for carrying water? Perhaps they were carried by young girls or very elderly women who could not carry heavy loads. Or were the small jars for ceremonial functions, to prepare liquid based foods, medicines and the like? Even though we were selecting the larger examples of each type to study, there was a Matsaki Polychrome jar in our sample that held only 15 lb of water, while yet another Matsaki jar held 50% more water; a PIII jar that held only 18.6 lb while another held almost double that amount. A Jeddito B/Y jar held only 17.5 lb, and a Sikyatki held 20.1 lbs, yet in each type there was another jar that held more than double that amount. These smaller jars were not little jars that held a gallon or less that might be expected to have very different functions than water jars. Instead the above examples seem just to be smaller versions of water jars, and were still big jars by other standards.

A final observation is obvious from Table 1: the Zuni area jars that were broadly contemporary with Hopi area jars were substantially smaller. This may have just been the result of small sample sizes, and should be checked with a larger sample. However, the largest Zuni area jar at 24.1 lb of water was only 60% of the weight of largest Sikyatki jar at 40.3 lb. This may reflect on the typical distance to water in the two areas. Late Zuni area sites like Hawikuh were located, not on mesa tops, but were very near domestic water sources, and so perhaps the need for really large jars to minimize the number of trips was not necessary.

CONCLUSIONS

Several conclusions can be drawn from this exercise. First, there does not seem to be a shift in function of Yellow Ware jars. While it may be the case that their use was extended to functions other than carrying water, they never grew so large that they could not have been used for water transport, and their shape was well adapted to carrying water over difficult trails. Although the shape of jars varied greatly among different Southwestern groups, the laden weights of their largest water carrying jars were quite similar. The notable exceptions are the jars from the late prehistoric period at Zuni, which appear to have been smaller than others. In addition, a large size range, ca. 100%, was noted between the larger and smaller jars that otherwise seem similar in shape, designs, and presumed function. Searching for the reasons for this significant size range would seem to be a fruitful line of investigation in the future.

Finally, these jars, when laden, were heavy. It is difficult to contemplate a woman probably weighing little more than 100 lb, carrying a laden water jar weighing 50 lb up to the top of Antelope Mesa along a steep and difficult trail. Not only were the women who made these vessels great potters and painters, they were tough. Putting the water back in the jars puts the daily lives of these women in a perspective that we do not usually consider. Artifacts need not be dry, sterile pieces of the past; we can put lives, or in this case, water back in them.
ENDNOTES

1 We only weighed jars that did not have plaster infilling and that were not missing large sections. However, almost all jars, except the historic ones, were imperfect in some way, and so weights are given only to the half pound as the values are probably not more accurate than that.

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Smith, W.

Smith, W., R. B. Woodbury and N. F. S. Woodbury
Introduction

In the latter half of the 1950s I was writing my dissertation in the anthropology department at the University of Arizona. The subject was the archaeology of a group of rock shelters on the east side of the Lukachukai Mountains in the extreme northeastern corner of Arizona. They are located on the contact between the Navajo and Wingate sandstones and typically were near a spring or intermittent watercourse.

Earl H. Morris, my father, had excavated these caves in 1931 for the Carnegie Institution of Washington. Other demands on his efforts resulted in their not being completely published before his passing in 1956, although two reports had appeared (Morris 1941, 1951). Emil W. Haury, a friend and colleague of my father, arranged to have the notes and collections deposited in the Arizona State Museum in Tucson where he was the director. At the time I was a graduate student in Anthropology at the University of Arizona, and Emil Haury suggested that I write up this research as my dissertation.

It was uniquely informative in several important ways. Many of the dozen caves had one or more Basketmaker pithouses aligned along the floors. Most of these had burned. As the roofs collapsed the earthen exterior coating had extinguished the fires preserving hundreds of perishable items. The conflagration of homes and belongings must have been a major tragedy for the Basketmaker people. However, it has proved to be a treasure trove for archaeologists. We are still learning from these tangible remains of Basketmaker life in the seventh and proceeding centuries A.D.

Just a partial listing of artifacts of wood includes farming implements, snare sticks, a dice set, fire hearths and drills, flutes, atlatl and dart fragments, and pieces of bows and arrows. Artifacts of leather included pouches, sandals, and a human scalp lock. There were more than 300 yucca fiber sandals elaborately ornamented with dyed and textured designs (Hays-Gilpin et al. 1998). Other yucca fiber artifacts included tump bands, female aprons, and colorfully decorated bags. Additional examples of finely woven items were made of human hair and dog fur. It is this last category with which this paper is concerned.

Adding to the significance of the Prayer Rock sites was the precise dating of pithouse construction. Dendrochronology applied to the four major posts that supported the roofs and the secondary smaller timbers that were the inside component of the walls indicated building dates ranging from A.D. 623 to A.D. 670. Other pithouses dated from A.D. 470 into the A.D. 500s, and are interpreted to be associated with the atlatl-using, pre-pottery Basketmaker II inhabitants. It is interesting to note that these tree-ring specimens were so numerous and tightly clustered that they were used to verify early radiocarbon dates during the development of C-14 in the 1950s. Later, in the 1980s these well dated, burned floors and hearths were used to extend the archaeomagnetic dating
curve back into the seventh century (Jeffrey Eighmy, personal communication 1986).

There were dozens of ceramic vessels. Most of these were plain gray with both polished or merely smoothed surfaces. A few exhibited rather simple black designs. Some earlier probably experimental vessels were thick and lumpy and had been molded on the inside of coiled baskets. Whether they were fired before they were baked in the house fires remains an open question. Dozens of metates, manos, cobble stones, projectile points, beads and other stone artifacts were in the assemblage.

ARCHAEOLOGICAL DATA BASE OF THE DOG HAIR SASHES

The artifacts classified in the fiber arts group were both numerous, diverse, often elaborately decorated, and well preserved. Among them were eight sashes made from dog hair. They were found in two caches, six specimens wrapped together of Pithouse 6 in Broken Flute Cave. Seven of them are illustrated in Figure 1. Most of the fibers were spun into very fine string and were from white dogs. Dark brown fur, also very finely spun, was made into two whole sashes and into decorative elements on three of the others. All except one of the sashes in Figure 1 were made of flat braids of differing widths and slightly differing lengths. All had fringes at the ends (Figure 2). One all brown sash had an olivella shell tied to each fringe. While all sashes were not made to precisely the same specifications in terms of length, width, and decoration, they were very similar in manufacturing details (Table 1, Figure 1). Four of them in particular look as though they were made to be a set (Figure 1, a-d, and see Figure 3). Interestingly one of these is brown. Some possible interpretations of the significance of this color switch are presented in the discussion below. However, it may be an unintentional, almost accidental choice of the weaver or the material available.

![Figure 1. Photograph of seven Basketmaker sashes of dog hair found in Prayer Rock rock shelters. (from Morris 1980:94, Figure 52)](image)

The inside-most specimen in Figure 1g is made of white elements in a 12 thread over-two, under-two square braid. The fringes are 3-4 cms in length. Each is wrapped at the base with very finely spun white and brown thread (Figure 4). This artifact wrapped the Obelisk Cave sashes together.

The two sashes (Figure 1, e-f) found in the cist under Pithouse 6 in Broken Flute cave are similar to those described above, but lack the fineness of thread and precision of manufacture. Both are about 130 cm long and 1.5 cm in width. There are short tassels at each end (see Figure 5).

A few other specimens made of the same colors of dog fur were recovered as well. These were small lengths of string and braid found separately in the excavations. A cache of unspun fur found in a
Table 1
Quantitative analysis of components of dog hair sashes from the Prayer Rock rock shelters.
(from Morris 1980:98, Table 7)

<table>
<thead>
<tr>
<th>Color of thread</th>
<th>a</th>
<th>b</th>
<th>c</th>
<th>d</th>
<th>e</th>
<th>f</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total length of sash</td>
<td>270 cm</td>
<td>270 cm</td>
<td>270 cm</td>
<td>270 cm</td>
<td>203 cm</td>
<td>147 cm</td>
</tr>
<tr>
<td>Total width of sash</td>
<td>7.3 cm</td>
<td>4.5 cm</td>
<td>6.5 cm</td>
<td>6.7 cm</td>
<td>2.2 cm</td>
<td>5.6 cm</td>
</tr>
<tr>
<td>Total number of threads</td>
<td>103</td>
<td>79</td>
<td>99</td>
<td>111–119</td>
<td>35</td>
<td>111</td>
</tr>
<tr>
<td>Length of a single thread</td>
<td>3.7 m</td>
<td>3.9 m</td>
<td>3.7 m</td>
<td>3.6 m</td>
<td>2.7 m</td>
<td>2.1 m</td>
</tr>
<tr>
<td>Total length of thread per sash</td>
<td>389 m</td>
<td>288 m</td>
<td>378 m</td>
<td>427 m</td>
<td>91 m</td>
<td>215 m</td>
</tr>
<tr>
<td>Number of threads per inch of weave</td>
<td>13</td>
<td>20</td>
<td>17</td>
<td>17</td>
<td>12</td>
<td>24</td>
</tr>
<tr>
<td>Length of plaited part of sash</td>
<td>170 cm</td>
<td>161 cm</td>
<td>170 cm</td>
<td>155 cm</td>
<td>103 cm</td>
<td>78 cm</td>
</tr>
<tr>
<td>Number of braided fringes per end of sash</td>
<td>13</td>
<td>10</td>
<td>12</td>
<td>14–15</td>
<td>8</td>
<td>14</td>
</tr>
<tr>
<td>Length of fringe elements</td>
<td>57 cm</td>
<td>50 cm</td>
<td>57 cm</td>
<td>49 cm</td>
<td>31 cm</td>
<td>27 cm</td>
</tr>
<tr>
<td>Total length of fringe elements per sash</td>
<td>11.3 m</td>
<td>9.2 m</td>
<td>11.2 m</td>
<td>12 m</td>
<td>9.2 m</td>
<td>9.4 m</td>
</tr>
<tr>
<td>Number of threads per braided fringe element</td>
<td>8 in 24</td>
<td>8 in 12</td>
<td>8 in 19</td>
<td>8 in 24</td>
<td>4 in 13</td>
<td>8 in all</td>
</tr>
<tr>
<td>Lashing method used on fringes, as shown in Fig. 57</td>
<td>25 of a 1 of i</td>
<td>14 of j 3 of g 3 of h</td>
<td>20 of a 1 of b 1 of c 1 of d 1 of e</td>
<td>26 of i 1 of j knotted instead of lashed</td>
<td>all of h</td>
<td></td>
</tr>
<tr>
<td>Average length of fringe</td>
<td>7.3 cm</td>
<td>20.3 cm</td>
<td>7.3 cm</td>
<td>14.7 cm</td>
<td>21.5 cm</td>
<td>4.0 cm</td>
</tr>
</tbody>
</table>

Small plain ware jar was probably collected to be spun into thread for yet another beautiful garment. There was some spun string in the same pot. Its occurrence in a ceramic vessel would reflect a Basketmaker III age, as probably would the cache of two found under the floor of Pithouse 6 in Broken Flute Cave. The other cache, of six sashes, was found in Obelisk Cave is probably Basketmaker II in age.

Most of the sashes were on display in the Museum in Mesa Verde National Park until a few years ago. More recently a single sash was to be seen in an exhibit case there. The other artifacts and the notes and analyses for them and the sashes are in the Arizona State Museum, University of Arizona, Tucson.

The dissertation with detailed descriptions and illustrations was completed in 1959 (Morris 1959b) and then was published (Morris 1980). Papers describing the 70± cm long wooden flutes with their remarkably preserved tonal qualities and other musical attributes have appeared separately (Bakkegard and Morris 1961; Morris 1959a). The manufacture, evolution, and decoration of the yucca fiber sandals were published in 1998 (Hays-Gilpin et al. 1998).
OCCURRENCE OF DOG HAIR SASHES IN OTHER SITES

An effort was made to identify other sites where artifacts made of dog hair have been found. In 1916-1917 Alfred Vincent Kidder and Samuel James Guernsey excavated in 15 rock shelters near Kayenta in northern Arizona. One of the sites is named White Dog Cave for the mummy of a canine located with the mummy of a human found in the site. This dog mummy was on display in the Peabody Museum, at Harvard University for many years. With it in the case was a blue ribbon awarded by the American Kennel Club at a nearby dog show for “Oldest Dog in the Show”. Additionally, the description of fur-wrapped yucca string to make fur blankets so often used for burial wrapping and in all likelihood for warmth by the living, included dog, rabbit, and buffalo. The padding and warp-end ties on tump bands are also described as dog or buffa-
The scarcity of buffalo remains from the Colorado Plateau make it seem more likely that this material was dog hair and or fur.

Additionally, two very narrow “tapes” were recovered from this cave and Cave 1 nearby. The description of the material and the weaving technique suggests that these are narrower versions of the sashes from Broken Flute and Obelisk Caves (Kidder and Guernsey 1919:173 and Figure 82).

A sash was recovered from a Basketmaker context in the Falls Creek North Shelter high up on the west side of the Animas River a few miles north of Durango, Colorado. Artifacts in a dry space, labeled the Burial Crevice, included a sash or belt very similar to the ones found in the Prayer Rock cave group. The excavations made by the Carnegie Institution of Washington and materials recovered by earlier efforts have been published (Morris and Burgh 1954). The sash resembles those described above in a number of respects. There are 52 finely spun strings plaited in a wide braid forming a sash 6 cm in width. Most of the material is white with a decorative zig-zag line made from two side-by-side brown threads running loosely from edge to edge. Only slightly more than 20 cm of the artifact were recovered, along with some scraps from the surrounding fill. Thus, the total length of this sash and the presence or absence of fringes cannot be determined. The material was originally identified as rabbit fur. It is here suggested that wild cottontail and jack rabbits have fur with very short elements and that only some of it is white. Snowshoe hares have longer hair that is seasonally white. However, it seems at least possible that a more accurate iden-
tification would be dog hair. In any case, the plaiting, design, age, and form (sash) are reasons for including it here.

Painted Cave is located on the west side of the northern Lukachukai mountains and not far from the Prayer Rock rock shelter group (Haury 1945). A specimen of “braided belting” made of brown and white fur was recovered. Haury considers it to be of Basketmaker II age with some alterations made by later Pueblo III inhabitants. Two 17-inch lengths of the same fur belt were sewn side by side with cotton thread. Additionally, cotton threads were sewn on the ends of the piece. The original belt would have been at least 86 cm long and 3 cm wide. The brown and white checkered design in this flat braid, as well as its length and narrow width, are reminiscent of the sash shown here as Figure 1e. However, the creation of the pattern was by a somewhat different method.

Martin et al. (1952) describe a sash of animal hair found around the waist of a mummy in Tularosa Cave near Reserve, New Mexico. It consisted of a bundle of strings twisted into a rope. The finished length was 166 cm. The described age of Pine Lawn or Pre-pottery phase matches the Basketmaker II-III age of the Prayer Rock sashes.

DISCUSSION

In the years that have passed since the dog hair sashes were written up in the 1950s, I have become better informed about fiber arts and about dog hair. Additionally, some innovative thinking about interpreting material remains has been developed. For that matter the archaeological data base itself, as well the number of archaeologists, greatly expanded.

I am tempted to add the names of several close dog companions to the list of references cited. In any case, dog hair is often produced continually and in abundance. While not all is long and soft enough to be spun into thread suitable to weave into fabric, much of it is. Dog hair and fur is much softer to manipulate than yucca fiber even when the vegetal fiber is soaked and pounded. A dedicated weaver with access to the right dogs would not lack raw materials.

Lynn Teague, utilizing Pueblo and other ethnographic information, usefully describes the spinning and weaving processes as being easily done around the more regular household schedule of women's duties such as meal preparation and child tending. When weaving and the attendant activities become economically significant, usually for trade, taxes, or other wealth accumulation, men become involved. Teague goes on to describe such labor intensive but pacific, repetitious activities of creation as often being in the province of women. Sometimes these individuals may be very old, or very young, or perhaps infirm (Teague 1998). Family, kin, and neighborhood associations are more likely to be within the work and creativity provinces of women (Hays-Gilpin, 1992; Hays-Gilpin et al. 1998).

Additionally, some dynamic thinking has been applied to Southwestern textile and other arts with respect to possible social implications (for example Hays-Gilpin 1992; Hays-Gilpin, et al.1998). Other references address this subject usefully from a broader perspective (for example Costin 1996; Maurer 1979; Watson and Kennedy 1991).

The Basketmaker people were no strangers to beautiful fiber arts. The twined sandals mentioned above found in the Prayer Rock caves and other sites in the Four Corners area are elaborately decorated. While a few specimens are made of flat braiding of wide and narrow yucca leaves or of hide, the vast majority were woven of finely spun yucca fiber string. Both warp and weft elements are interpreted to be manipulated in a loom-less or hand loom context. Decorations consist of geometric designs on the upper surfaces produced with threads dyed red, yellow, black, and blue-
green. Textured geometric designs were formed on the bottoms of the soles by elaborate wrapping of weft threads around the warps. There is no correlation between the upper colored designs and the tread designs. These textured bottoms would be of use on the sandstone surfaces so abundant in the area in the way that shoe and boot soles are in modern times.

However, the upper colorful designs while known to the wearer would only be visible to others when they were not being worn. It has been suggested that the upper colored patterns and the lower textured designs might identify the wearer to others whether they were seen as tracks perhaps on a trail, or if the footgear was left by the entry of a pithouse.

It has been further suggested that besides identity of the wearer, known patterns might indicate status or social affiliations, for example kinship affiliations such as clan, village or tribe (Hays-Gilpin et al. 1998).

It is suggested that the existence, decoration, and possible function of the dog hair sashes might reflect some similar interpretive possibilities. The elaborate creation of twined sandals must have been well established in Basketmaker II and III culture. The dog hair sashes, even in their rarity, are concurrent. Perhaps the inventive effort at weaving and decoration seen in such elaboration in the sandals opened the doors for the creator(s) of the sashes to work in their own media. Hays-Gilpin suggests that the social importance of sandal designs might reflect increasing group size with the resulting interpersonal interaction. In other words, as the group increases in size, more devices are needed to keep track of each other and behavioral reciprocity. Sandal decoration might be one method visible in the archaeological record. Sex, gender, and status might be indicated by attributes such as design and color in woven artifacts. Sex, gender and status studies have proliferated in recent years and are too numerous to permit an exhaustive review. The focus of these studies can be clarified by stating that sex is a biological fact while gender is a societal role that is a matter of recognition of individual and cultural choice. For example, Barbar (1994) refers to women gathering for textile manufacture in the way that “quilting bees” are held in modern times. Despite usually involving women, this is a gender related activity. Another reference addressing this situation is to be found in Watson and Kennedy (1991).

Additionally, a colleague raised an interesting possibility. The astounding amount of labor involved in collecting the fur it the first place, spinning it into very fine thread, and then plaiting the thread into at least eight sashes can be described as labor intensive from any point of view. It is possible that more than one individual was involved. If so they were certainly in very close communication with each other. It has been suggested that the weaving is likely to be done by women, and perhaps those who are very young or old or infirm enough to spend their days working quietly with dog hair rather than in more arduous pursuits such as gathering or gardening. In consideration of these matters a colleague asked if the weaver might have been blind or visually impaired? My initial reaction was skeptical. How could a visually impaired person create such fine products? It was explained to me that various kinds of essentially repetitious handwork were an important ingredient in programs teaching blind people to be creative and useful. This is a fascinating subject to contemplate. Together with other causes of relative immobility it is unlikely that archaeological evidence will provide the answers. However, in an optimistic mode, it is possible to imagine skeletal remains indicating severe crippling due to injury or arthritis being found being found in association with weaving tools and ideally materials. Perhaps the remains of such an individual have already been recovered (Morris 1948). In the case of the dog hair sashes someone kept the white and brown fur separate and for whatever reasons expended a great deal of skilled effort.
SUGGESTIONS FOR FUTURE RESEARCH

There would appear to be a number of directions for future research. Beginning with the archaeological database more examinations of perishable collections in museums might discover other specimens made of dog hair and fur. Utilizing modern analytical techniques some of the artifacts described as having “fur” or “animal hair” components might be accurately identified.

Perhaps other materials would be found that were made into sashes in Basketmaker times. Other fine weavings might emerge as foci for manufacturing processes and, perhaps, behavioral interpretation. Additionally, more sashes might be located. The bigger the sample the more likely it is to identify the desired norm of the product. With complete specimens described, tiny scraps might be identified. Even bits of string made from dog and perhaps other species could increase our knowledge of temporal and geographical distributions.

The sizes of individual specimens might suggest the purposes for which they were made. Other uses besides those mentioned here are certainly predictable. Items of clothing on burials would be particularly informative. Other fiber arts techniques, besides basic spinning and flat braiding, are also predictable utilizing this often plentifully available soft dog fur and hair. Perhaps other relatively rare materials might be identified.

Comparative studies of other prehistoric sashes wherever they occur would add perspective to the function(s) as well as the temporal and geographic occurrences. Ethnographic comparisons, particularly among the descendent Puebloan people, will add insight into who uses sashes, their significance, when and how they were used, and how they were made. Prehistoric kiva murals, rock art components, pre-commercial kachinas, and designs on ceramic vessels would be fruitful sources of information.

Hays-Gilpin (1979), quoting Maurer (1979:120), presents yet another potential direction for interpretation:

Often the act of making a garment was believed to be under the guidance and protection of sacred powers who taught people techniques such as weaving, or sent them inspiration for the concept of decorative forms and the perfection of their execution. In this system, the maker as well as the wearer of a garment might share in the spiritual power and blessings that it represented.

Comparative studies might be made with similarly woven artifacts made of other materials, including yucca leaves, human hair, other animal hair or fur, apocynum fiber, cotton, willow, other tules, and still other materials, would illuminate the distribution of the final products, the manufacture and, hopefully, suggest some non-material attributes. Patterned spatial and temporal distributions would suggest the presence or absence of cultural contact in prehistoric times.

Dog hair plaited into sashes is apparently quite rare. The question may be asked asked. Is it the utilization of dog hair or the making of sashes that is rare? Or, is it possibly both of these variables?

One wonders whether the use of dog fur did added power or status to the finished product and its wearer. Perhaps this material was used only because it was available. A consideration of domestication raises the question about whether wolf or coyote fur was ever spun?

It has been suggested that these caches of sashes might constitute items intended for trade. This would especially apply in so far as they do not constitute a “set” as has been suggested for the ones portrayed in Figure la-d. Or perhaps a “set” could be manufactured for trade as well. This might be local, within a community perhaps of several related sites, or for more distant destinations. How
relevant are the obvious differences visible among the members found in each cache?

Perhaps their creation is an early indication of wealth accumulation. Equally interesting and equally hard to prove are hypotheses that certain garments, paralleling those suggested for sandal designs, might reflect sex, gender, social status or at least identity of specific individuals.

The current dynamic status of hypothesis formulation about interpreting archaeological material culture creates a fertile field for further investigations of this fascinating subject.

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Morris, Elizabeth Ann


Morris, Earl H.


Morris, Earl H. and R. F. Burgh


Teague, Lynn S.


Watson, Patty Jo and Mary C, Kennedy

Paul Sidney Martin (1899 - 1974; Figure 1) stands as a pillar in the development of North American archaeological knowledge, method, and theory. During a career that spanned six decades, including 43 years as curator of North American Anthropology at The Field Museum in Chicago, Martin excavated more than 70 archaeological sites in Colorado, New Mexico, and Arizona, supervised six major archaeological surveys, collected more than 585,000 objects, and published more than 200 books, peer-reviewed articles, and popular contributions. Martin made many discoveries, trained generations of archaeologists, and helped develop a scientific archaeology, but closed a glorious career in Southwestern archaeology with a reflexive, if not melancholy, testimonial: “My guess is that there are many roads to the truth” (Martin 1974:27).

Martin’s scholarly publications with John Beach Rinaldo (1912 - 1999) and others in The Field Museum’s Fieldiana:Anthropology series are well known to archaeologists. Martin’s popular contributions are less well known, for they were published in obscure outlets like the Pan-American Traveler, Great Lakes Technocrat, and Empire Crown, in addition to The Field Museum’s membership newsletter. This paper examines Martin’s research on 32 Mogollon sites in New Mexico and Arizona between 1939 and 1955 (Figure 2) in light of the context provided by these popular contributions. Because they were intended for a radically different audience than his scholarly publications, the popular articles provided Martin with a non-threatening venue in which to cautiously test new conjectures about the prehistoric past and to declare, often vociferously, the relevance of archaeological research to the solution of the world’s problems.

Figure 1.
Paul Sidney Martin in 1948, reading a copy of his recently published archaeology textbook Indians Before Columbus (FM Neg. No. A92570).
BACKGROUND

Martin was a lifelong Chicagoan. Born November 20, 1899, Martin went to New Trier High School in Winnetka, Illinois, and earned his bachelor’s, master’s, and doctoral degrees at the University of Chicago. Though he conducted fieldwork in the Yucatan in the 1920s, and wanted to become a Mayanist, simultaneous cases of malaria, worms, and amoebic dysentery precluded his return to tropical climes. In 1928, Martin began a long fieldwork career in the American Southwest while working for the Colorado State Historical Society. He continued that research after accepting a curatorial position at The Field Museum in 1929. There he began a swift ascent to become Chief Curator in 1935, a post he held until his retirement in 1964. Martin remained in Chicago as curator emeritus until 1973 when for health reasons he moved to Tucson, Arizona, where he died 20 January 1974.

One remarkable aspect of Martin’s career is that he typically published site reports within one, two, or at most three years of the completion of excavations at any given site (see Table 1). This publication record stands as clear evidence that Martin was not hindered by the constraints of a university teaching schedule, and that he had able assistants working for him. However, Martin failed to publish reports on 27 of the 71 (41%) sites he excavated or tested, particularly those he excavated in Arizona later in his career and after Rinaldo left the Museum in 1963. To be fair, Martin’s excavations at some of the unpublished sites, particularly those in New Mexico, amount to little more
than archaeological testing, but included in the unpublished roster are some that are important in the history of archaeological method and theory, including the Vernon Site (see Longacre and Graves 1976), Hay Hollow Site (see Martin 1967; Plog 1974) and others. The Field Museum archives do contain documentation of various kinds for nearly all of the unpublished sites, so they are not completely lost to science, and certain collections remain available for analysis. Rinaldo (n.d.) did prepare a manuscript summarizing the excavations of some of the unpublished New Mexican sites; it is on file in The Field Museum archives and will be summarized below.

Table 1
Sites Excavated by the Field Museum of Natural History’s Southwest Archaeological Expedition, 1939 – 1955.

<table>
<thead>
<tr>
<th>Site Name</th>
<th>Excavation Year(s)</th>
<th>Artifacts (to nearest 10)</th>
<th>Source</th>
</tr>
</thead>
<tbody>
<tr>
<td>SU Site, NM</td>
<td>1939, 1941</td>
<td>34860</td>
<td>Martin 1940, Martin 1943</td>
</tr>
<tr>
<td>Wet Leggett Site, NM</td>
<td>1946</td>
<td></td>
<td>Martin 1947</td>
</tr>
<tr>
<td>Promontory Site, NM</td>
<td>1947</td>
<td>80</td>
<td>Martin, et al. 1949</td>
</tr>
<tr>
<td>Twin Bridges Site, NM</td>
<td>1947</td>
<td>450</td>
<td>Martin, et al. 1949</td>
</tr>
<tr>
<td>Oak Springs Pueblo, NM</td>
<td>1947</td>
<td>980</td>
<td>Martin, et al. 1949</td>
</tr>
<tr>
<td>Wet Leggett Pueblo, NM</td>
<td>1949</td>
<td>8220</td>
<td>Martin, et al. 1949, Rinaldo 1950a</td>
</tr>
<tr>
<td>South Leggett Pueblo, NM</td>
<td>1949</td>
<td>2350</td>
<td>Martin and Rinaldo 1950b</td>
</tr>
<tr>
<td>Three Pines Pueblo, NM</td>
<td>1949</td>
<td>1360</td>
<td>Martin and Rinaldo 1950b</td>
</tr>
<tr>
<td>Pine Lawn Camp, NM</td>
<td>1949</td>
<td>1410</td>
<td>Martin and Rinaldo 1950b</td>
</tr>
<tr>
<td>Tularosa Cave, NM</td>
<td>1950</td>
<td>51670</td>
<td>Unpublished; see Rinaldo n.d.</td>
</tr>
<tr>
<td>Apache Cave, NM</td>
<td>1950</td>
<td>330</td>
<td>Unpublished; see Rinaldo n.d.</td>
</tr>
<tr>
<td>Bull Basin Cave, NM</td>
<td>1950</td>
<td>20</td>
<td>Unpublished</td>
</tr>
<tr>
<td>Balke Site, NM</td>
<td>1950</td>
<td>80</td>
<td>Unpublished</td>
</tr>
<tr>
<td>Cordova Cave, NM</td>
<td>1951</td>
<td>15060</td>
<td>Martin, et al. 1952</td>
</tr>
<tr>
<td>Negrito Cliff Dwelling, NM</td>
<td>1951</td>
<td>300</td>
<td>Unpublished; see Rinaldo n.d.</td>
</tr>
<tr>
<td>Kiehne Pueblo, NM</td>
<td>1951</td>
<td>170</td>
<td>Unpublished</td>
</tr>
<tr>
<td>Sawmill Site, NM</td>
<td>1951, 1952</td>
<td>1200</td>
<td>unpublished</td>
</tr>
<tr>
<td>O Block Cave, NM</td>
<td>1952</td>
<td>6780</td>
<td>Martin, et al. 1954</td>
</tr>
<tr>
<td>Y Canyon Cave, NM</td>
<td>1952</td>
<td>650</td>
<td>Martin, et al. 1954</td>
</tr>
<tr>
<td>Hinkle Park Cliff Dwelling, NM</td>
<td>1952</td>
<td>3790</td>
<td>Martin, et al. 1954</td>
</tr>
<tr>
<td>Cosper Cliff Dwelling, AZ</td>
<td>1952</td>
<td>830</td>
<td>Martin, et al. 1954</td>
</tr>
<tr>
<td>Apache Creek Pueblo, NM</td>
<td>1954</td>
<td>990</td>
<td>Martin, et al. 1957</td>
</tr>
<tr>
<td>Valley View Pueblo, NM</td>
<td>1954</td>
<td>350</td>
<td>Martin, et al. 1957</td>
</tr>
<tr>
<td>Saddle Mountain Pueblo, NM</td>
<td>1955</td>
<td>280</td>
<td>Unpublished; see Rinaldo n.d.</td>
</tr>
<tr>
<td>Powerline Site, NM</td>
<td>1955</td>
<td>260</td>
<td>Unpublished; see Rinaldo n.d.</td>
</tr>
<tr>
<td>Delgar Site, NM</td>
<td>1955</td>
<td>510</td>
<td>Unpublished; see Rinaldo n.d.</td>
</tr>
<tr>
<td>Perry Lawson Site, NM</td>
<td>1955</td>
<td>40</td>
<td>Unpublished</td>
</tr>
<tr>
<td>Bar BO Site, NM</td>
<td>1955</td>
<td>30</td>
<td>Unpublished</td>
</tr>
<tr>
<td>Frisco Pueblo, NM</td>
<td>1955</td>
<td>40</td>
<td>Unpublished</td>
</tr>
</tbody>
</table>
Martin also did not catalog the vast majority of artifacts he collected. As a general rule, Martin catalogued only the artifacts that he, Rinaldo, or others analyzed and presented in the site reports. Though this strategy makes some sense, in that the published artifacts are the ones to which other scholars would likely demand access, the unpublish collection, numbering in the hundreds of thousands of objects, went uncatalogued, and the objects were therefore never recorded in The Museum’s institutional memory. Fortunately, this situation has recently been rectified. In 1997, The Field Museum’s Curator of North American Anthropology, Jonathan Haas, acquired funding from the National Science Foundation (Grant No. SBR-9710181) and hired the author to catalog and computerize the Martin Collection. A searchable database is now available on-line at The Museum’s website.

Martin’s archaeological research for The Field Museum can be divided into three general phases: 1) The early years (1929 - 1938) in southwestern Colorado; 2) The middle years (1939 - 1955) in New Mexico; and 3) The later years (1956 - 1972) in Arizona. Generally speaking, Martin was doing culture history in Colorado and New Mexico, whereas Arizona served as his laboratory for the New Archaeology. Though the current political division between Arizona and New Mexico has no relevance archaeologically, Martin’s research in New Mexico has coherence both historically and philosophically and is therefore a reasonable unit for consideration in the current volume. This paper briefly summarizes the basic archaeology of each site while examining the broader context of Martin’s research between 1939 and 1955.

Why did Martin begin working in New Mexico? His research at Lowry Ruin and other sites in Colorado did not occur in scholarly isolation. Though he was promoted to Chief Curator in 1935, he was still a young professional seeking to make a professional name for himself, and probably felt he could not do this by continuing to study Anasazi archaeology. In 1937, Martin published a glowing review of Emil Walter Haury’s (1936) book on the newly discovered Mogollon Culture of Southwestern New Mexico, and his enthusiasm was palpable:

The results of [Haury’s] work are so astonishing, so far reaching, and so unorthodox that the worth of this report and of the new data contained therein probably will not be understood or esteemed for some years...The hypotheses set forth in this excellent report will doubtless be scoffed at by many competent people...[but] I am willing to go along with Haury (Martin 1937:233-234).
Martin thus jumped headlong onto the Mogollon bandwagon, and in 1939 moved The Field Museum’s Southwest Archaeological Expedition south to excavate in the Pine Lawn Valley and surrounding regions of west-central New Mexico.

**MOGOLLON ARCHAEOLOGY AND THE RINALDO YEARS**

Martin's archaeological research in New Mexico (1939 - 1955) is roughly coincident with two important aspects of mid-century anthropology at The Field Museum: his tenure as Chief Curator (1935 - 1964) and Rinaldo's tenure (1938 - 1963) at The Field Museum.

Martin's research dominated Field Museum anthropology for the 27 years that he served as Chief Curator. His expeditions to the southwest constitute nearly half (20 of 44; see Nash and Feinman 2003:294-296) of all the expeditions conducted by Field Museum anthropologists between 1935 and 1964. Martin’s *Fieldiana: Anthropology* monographs constitute one-third of the *Fieldiana* volumes published during the same period (see Nash and Feinman 2003:301-305). Clearly, his research was the Departmental priority at the time.

John Rinaldo (Figure 3) joined Martin as a volunteer during the 1938 field season and continued in that capacity in Chicago in the fall, focusing on descriptive artifact analyses and report writing. In 1939, Martin hired him as an Associate in Southwestern Archaeology, thus formalizing a professional collaboration that would last for a quarter century. Rinaldo was promoted to Curator of Archaeology in 1950 and remained at the Museum until 1963, when Martin's conversion to the New Archaeology became too much for the conservative Rinaldo to bear. In Rinaldo, Martin found an able and willing assistant who handled with aplomb the many practical matters of archaeological field and laboratory work. With firm control of department resources and an able assistant at his side, Martin's Mogollon research began at the SU Site in 1939.

The SU Site, located seven miles west of Reserve, consisted of more than 30 pithouses and three surface structures, of which Martin and crew excavated 26 during the course of excavations in 1939, 1941, and 1946 (Martin 1940, 1943a, and 1947). Their goals were to help define the characteristics of the Mogollon culture and to examine the relationship between Mogollon and the “San Juan Core” (Martin 1940:7), from which Martin had fled.

Because excavations at the SU Site were helping to define a newly discovered prehistoric culture, which occupied pithouses rather than pueblos, Martin changed his excavation and recovery techniques. In contrast to Lowry Ruin, where plainware sherds were discarded with willful abandon, Martin collected plainware at the SU Site, especially because it represented some of the earliest pottery then known in the Southwest. In 1939, Martin collected more objects from eight pithouses than he had during four years digging 50 rooms at Lowry Ruin. The 1941 season at the SU Site was essentially a repeat of the 1939 season: ten pithouses excavated, 20,000 sherds and 750 formal stone and bone tools collected.

Because of World War II travel restrictions, Martin did not return to New Mexico until a third and final season at the SU Site in 1946 (Figure 4). In the meantime, Martin and Rinaldo published the 1941 site report (Martin 1943a), revamped exhibits, worked on collections, and wrote numerous popular articles. These contributions, particularly those penned in 1943 and 1944, provide insights on his thoughts about the state of humankind, of museum anthropology, and the role that anthropology can, or should, play in the grand theater of life. As such, they warrant further examination.

In “Housing, Food, Clothing, and Religious Practices of Prehistoric Indians” (Martin 1941a:1-
2), written for The Field Museum's membership, Martin offered a wide-ranging piece that used ethnographic analogy to interpret Mogollon archaeology while admonishing housewives to be thankful for the luxuries they currently enjoyed: “The ancient kitchen is but a far cry from the present day housewife’s labor-saving electrically equipped kitchen with shelves stocked with boxed cereals and canned goods, and a delivery boy bringing in a package of lamb chops” (1941a:1). How such commentary was received is not documented in the archives.

In “Why Dig Up Dead Indians?” Martin (1941b) made one of many statements regarding the relevance of archaeological research for the solution of the world’s problems. Note that, even at this early date, Martin is moving beyond culture history, the what, where, and when, to the study of culture process, of why and how:

If we can understand all of these facets of life in a relatively simple Indian culture, and if we can discover the whys and wherefores, then we are in a much stronger position for understanding and attacking the greater and more complex historical problems which must be solved if we are to attain real knowledge of man in the modern world (Martin 1941b:1).

Martin (1941c) also sought to educate the public about the nature and meaning of a “successful” expedition while addressing the ultimate goals of an anthropological archaeology:

Was the [SU Site] expedition a success? Emphatically, yes. Did we bring back any showy specimens? No...archaeology is not a search for specimens, and emphasis on them indicates a lack of perspective on “what it’s all about”...From the dust, the archaeologist cannot recover the language spoken by the villagers, the dress, the social customs, and a thousand and one other items that make up any culture. We can, however, by careful study and observation make reasoned and reasonable guesses about the past.
The United States government at the present time encourages isolated farmers to gather in or near centralized communities, where they may benefit from better social, psychological, and educational environments. To know whether such an experiment would be successful today, one must study the past. We are interested in showing that in spite of differences in time, climate, race, and geography, men possess certain fundamental urges which cause them to act more or less similarly in all times and in all places (Martin 1941c:4).

Two years later, at the height of World War II, Martin's universalist bent was again evident in a statement on race: "The racial problem is really a cultural, social, and economic one, rather than a purely biological one...Strictly speaking, it may be argued that there is only one real race, i.e. the Human race. (Martin 1943b:4-5; emphasis in original). In a more existential contribution entitled "Man, The Unknown", Martin (1943c) contrasted the conflicting goals of antiquarianism and archaeology and, through his evolutionist lens, was clearly affected by the terror of World War II:

Anthropologists seek to derive some general knowledge of human ways, not to collect miscellaneous and curious information...Raw data must be collected and studied in order to produce a significant synthesis of human history. Mere collecting of specimens and data is [sic] not enough; that is antiquarianism, not research, and is a sterile waste of time, effort and money.

We must know more about that puzzling and frightening creature, Man—about his desires and contradictory impulses which, on the one hand, cause him to make progress and to strive upwards and, on the other hand, to pull down and to destroy by wars and conquest much of the good he has achieved (Martin 1943b:6). Martin continued this pensive thread, and expounded on others, in a longer 1943 piece entitled "Why Dig Up The Dead!", which was published in Empire Crown, the employee newsletter for the Empire Box Corporation (Martin 1943d). He wrote bluntly:

We are living today in a very sick world. If civilization is to endure, we must push forward the study of man. We need to know the strength as well as the weaknesses of the human race and to understand the relationship of man to culture. We must know this because the tremendous machine which science has helped create may be directed by man for good as well as evil.

The present chaotic condition of the world is not new; it is only worse than before. Every major invention, from the time of the first stone axe or arrowhead down to the airplane and radio, though at first bringing great material benefit and comfort, has sooner or later created social or economic problems. Fire is useful for warmth and cooking but it may be used for destructive purposes. A stone arrowhead is useful for hunting animals, the skins of which may be used for clothing, and the meat for food; but an arrowhead may also be used for killing other men. And so it goes.

We must understand how the products of science affect the philosophy of life and the science of government. By studying the Basket Maker, the Mogollon, and other Indians we have an opportunity for the study of the rise and spread of a culture, the dominance of rulers and priests, civil wars and other rivalries, the effect of climate, the decline and eventual fall of a virile culture, and finally the effects of submission to a foreign military conquest.

If we can understand all of these facts of life in a relatively simple Indian culture, and if we can discover the whys and
wherefores, then we are in a much stronger position for understanding and attacking the greater and more complex historical problems which must be solved if we are to attain real knowledge of man in the modern world... Thus, digging up dead Indians has a very real significance and, if looked at in this way, it actually has possibilities which stagger the imagination (Martin 1943d:14).

Archaeology has yet to live up to such grandiose billing, but by 1945 Martin, as Chief Curator, was beginning to consider the possibility of conducting applied research to address the ethical and philosophical disconnect between technological progress and popular culture:

Everyone will recognize our remarkable technological achievements in the past one hundred years in manufacturing, weaving, metallurgy, agriculture, [and] transportation. But there has been no corresponding progress in our methods of understanding and handling people and in controlling our destinies. Our methods of dealing with them are Archaic... We hope to carry on research in Applied Anthropology in this Museum, and to present exhibits which deal with our subject (Martin 1945:4).

By the time Martin and Rinaldo returned to the field in 1946, they were changed men in a changed world. Rinaldo had served in the United States Army; Martin had tried to enlist but failed the physical examination. While remaining in Chicago, Martin interacted with cultural anthropologists on the faculty of the Department of Anthropology the University of Chicago. In the mid-1940s, Martin team-taught a museum studies course at the University with fellow curators Donald Collier and George Quimby. He also spent a great deal of time with cultural anthropologist Fred Eggan, who was working on Social Organization of the Western Pueblos (Eggan 1950), which proved quite influential to Martin’s scholarship.

Whereas Martin’s research to that time had been firmly grounded in culture history, the site report for the 1946 season (Martin 1947) contains one of his trademark “Conjectures” sections, which he used to push the interpretive envelope toward surprisingly modern processual research questions regarding prehistoric subsistence, social organization, and religion. The seeds of the New Archaeology of the 1960s may be found in the 1946 season report:

One of the main objectives in carrying on further excavation at the SU site was to gain more information as to how the former inhabitants of the village lived, how they grouped themselves socially, how they solved their subsistence problems, whether they had developed any religious concepts, and what their particular interests were (Martin 1947:287).

In 1947, Rinaldo conducted an archaeological survey of the region within a fifteen-mile radius of Reserve, ultimately documenting more than 100 sites. The SU Site had by then been tree-ring dated to the A.D. 400s and was accepted by scholars as the type-site for the Pine Lawn Phase (A.D. 200 – 550), so the goals of the survey were twofold: 1) To identify pre-pottery sites to learn about regional and cultural antecedents of the early Mogollon; 2) To find pottery-bearing sites in order to establish a more complete typological sequence for the region, especially for the post A.D. 500 period that had not yet received systematic attention (Martin et al. 1949:7).

After identifying several pre-pottery sites, Martin and Rinaldo, with help from geomorphologist Ernst Antevs, in 1947 excavated the Promontory and Wet Leggett Sites, the latter of which they returned to again the following year (Martin et al. 1949). Also in 1947, they excavated two pithouses at the Turkey Foot Ridge Site (Martin et al. 1949, 1950a), and several rooms at Oak Springs Pueblo (Martin et al. 1949).
The Wet Leggett Site

The Wet Leggett Site is a diffuse scatter of Cochise Culture (see Sayles and Antevs 1941) artifacts eight miles west of Reserve. Not to be confused with Wet Leggett Pueblo, which Martin excavated in 1949, the term “Site” is somewhat of a misnomer when applied to this manifestation, for artifacts were found eroding along a two-mile stretch of Wet Leggett arroyo at depths of two to five feet (Martin et al. 1949). Ultimately, they delineated two artifact concentrations. After trying without success to lay excavation grids over the difficult terrain at Locus A, they excavated test trenches at right angles to the arroyo bank, though the exact location of each was not tied to any datum and went unrecorded. At Locus B (Figure 5), provenience information was recorded to a greater level of detail, though precise location data are still lacking. Because of the difficulty inherent in excavating the site, they wisely decided to focus attention on later sites and to opportunistically visit the Wet Leggett Site after each rain, when new artifacts might be exposed in the gully.

The Promontory Site

The Promontory Site, an early pithouse village located on top of a mesa (no additional provenience data are available) was excavated to determine whether its entire occupation occurred during the Pine Lawn period (300 B.C. - A.D. 500) and whether temporal and spatial variations were observable during the Pine Lawn phase, though no such variation was found (Martin et al. 1949).

Twin Bridges Site

The Twin Bridges Site is a small village of four pit-houses located on the west side of the Pine Lawn Valley, west of New Mexico Highway 12 and about two miles south of the junction between it and U.S. Highway 180. Three of the pithouses are square or nearly so; the fourth is roughly circular and shallow. The site dates to the Three Circle Phase (A.D. 900 - 1000).
Oak Springs Pueblo

Oak Springs Pueblo (also once known as “Twin Bridges Pueblo”; LA 9725) is a small, L-shaped, seven-room pueblo located on the valley floor above Oak Springs arroyo. Martin and Rinaldo excavated the site in 1947 to examine problems “directly concerned with the origin and development of Mogollon culture” (Martin et al. 1949:17). The site dates to approximately A.D. 1050 (Bluhm 1957:70) squarely in the Reserve Phase (A.D. 1000 - 1100; Martin et al. 1949:221).

Turkey Foot Ridge Site

Located on a steep, narrow ridge west of New Mexico Highway 12 and less than two miles north of the junction of Saliz and Leggett Canyons in the Pine Lawn Valley, the Turkey Foot Ridge Site has 15 pithouses, most of which are rectangular. The ceramic assemblage indicated construction of most of the houses during the San Francisco phase (A.D. 700-900) with continued occupation into the Three Circle Phase (A.D. 900 - 1000). One house, though not directly dated, may relate to the early Georgetown Phase (A.D. 500 - 700). Excavations at the site were designed to “to fill in a postulated 400-year gap in the local sequence in the Pine Lawn Valley” (Martin et al. 1949:245). Martin and crew tested the Turkey Foot Ridge Site in 1947 and selected it for more detailed attention in 1948 (Figure 6; Martin and Rinaldo 1950a).

By the end of 1948, Martin had invested a lot of time and energy in examining the development of Mogollon culture in the Reserve region, but they had excavated only one relatively late, Reserve Phase (A.D. 1000 - 1100) pueblo—Oak Springs Pueblo—and at the time only one other was known to science—Starkweather Ruin, which had been excavated a decade earlier by Paul Nesbitt of the Logan Museum at Beloit College in Beloit, Wisconsin (Nesbitt 1938). They dedicated the 1949 excavation season to fieldwork on the following Reserve Phase sites they had previously identified: Wet Leggett Pueblo, South Leggett Pueblo, and Three Pines Pueblo. However, before conducting this fieldwork, Martin’s popular pen again turned existential. In a contribution enti-
tled "Peace from 4000 B.C. to A.D. 1000 in Indian Utopia", Martin wrote:

To a world threatened with the necessity of building underground to escape from atomic bombs, bacterial warfare, and other predicted horrors, the 1948 Archaeological Expedition to the Southwest brings news of Americans who in very early times gave up a free life of living under the stars to dig themselves in [at the Turkey Foot Ridge Site] for approximately 500 years, emerging to build themselves above the ground— all this without signs of chaos or warfare (Martin and Johnson 1948:3).

The comparison between pithouses and nuclear fallout shelters strains the bounds of credulity but indicates the degree of trepidation felt in a post-nuclear world. Again, Martin was attempting to demonstrate and maintain the relevance of his research to broader social and political issues.

**Wet Leggett Pueblo**

Wet Leggett Pueblo is located on a sloping peninsula bordered by Wet Leggett Canyon on the south and a small, unnamed arroyo on the north (Martin et al. 1950b: 413). The pueblo constitutes one of the earliest manifestations of surface architecture with stonewalls in the region; it is not rectilinear and does not have an associated kiva.

**Three Pines Pueblo**

Three Pines Pueblo was located just east of U.S. Highway 260 and “close to expedition headquarters” (Martin et al. 1950b:413). It consisted of two units of two masonry rooms each, and an associated pithouse.

**South Leggett Pueblo**

South Leggett Pueblo (LA 3563) is located just east of U.S. Highway 260, close to Leggett Canyon. It consists of a four-room pueblo with an associated but unattached masonry room that may, at one time, have been attached to the main room block. Nearby is a pithouse that Martin and Rinaldo (1950b:446-448) initially thought to be a kiva, but which did not yield artifacts and features that are characteristic of these specialized architectural forms.

Research on the three Reserve Phase (A.D. 1000 - 1100) sites was designed to address a number of questions. Martin and Rinaldo had come to recognize certain Anasazi influences in the Reserve Phase material, and sought to determine where those influences originated, whether actual immigrants were represented, whether kivas were associated with Reserve phase pueblos, if pithouses evolved into kivas, what social and political changes accompanied the Anasazi innovations, which Mogollon traits persisted and which were displaced, and if the advent of masonry rooms was abrupt or progressive (Martin and Rinaldo 1950b:415). Though they never answered these questions directly, the conjectures section of their report (Martin and Rinaldo 1950b:556-569; cf. Martin 1951a:50) offers the following astonishing inferences regarding the Chiracahua Stage of the Cochise Culture:

In the Southwest and adjacent areas, bilateral descent is characteristic of societies with a food-gathering economy, even after they have taken up agriculture. Hence the Indians of the Chiricahua stage [ca. 1500 B.C.] may have been organized as follows: a nuclear family (i.e. a married man and woman and their offspring) living in a house (because the house is physically small); politically independent endogamous bands ([Murdock 1949]:19, 85, 214); bilateral descent ([Murdock 1949]:212); matrilocal residence ([Murdock 1949]:204, 205, 213); no slavery and no social classes ([Murdock 1949]:88); monogamy, if both sexes contributed equally to support the household, or polygyny, if the women’s contribution was the larger ([Murdock 1949]:36) (Martin and Rinaldo 1950b:564-565).
Martin and Rinaldo (1950b:565-569) go on to offer similar inferences regarding the social organization characteristic of peoples during the subsequent Pine Lawn, Three Circle, and Reserve Phases. At this point, the scholarly impact of Eggan and George Peter Murdock are abundantly clear in Martin’s interpretation of prehistory.

By the end of 1949, the systematic excavation of pre-ceramic, pithouse, and pueblo sites had provided Martin with a sound spatial and material baseline on which to understand the development of Mogollon culture. Though ceramic crossdating and seriation had allowed him to establish a temporal sequence for sites in the region, he still did not yet have a stratigraphic sequence on which to check his conclusions. In 1950, Martin and Rinaldo turned their attention to a truly extraordinary site, Tularosa Cave.

Tularosa Cave

Tularosa Cave is the largest of a series of small caves in Tularosa Canyon, approximately one mile east of Aragon, New Mexico (Figure 7; Martin et al. 1952). Though Mogollon non-perishable ceramic and stone technologies were reasonably well understood, at that time only open air sites had been excavated and as a result perishable botanical and faunal remains from Mogollon sites were poorly known. The 2.8-meter thick deposit at Tularosa Cave, which spans some 1300 years, has always been protected from precipitation and ground water, and is characterized by an unprecedented degree of preservation. Excavations in 1950 yielded 38,000 maize cobs, more than 1700 pieces of string, netting, basketry, and wood, and 2600 well-preserved animal bone fragments. Martin and Rinaldo must also have received a boost in confidence when the stratigraphic sequence at Tularosa Cave confirmed the chronology they had established over the previous decade through seriation and crossdating. The site
report (Martin et al. 1952) for Tularosa Cave remains a classic in its detailed and collaborative presentation.

Cordova Cave

In 1951, Martin and Rinaldo excavated Cordova Cave, six miles south of Reserve. The archaeology of Cordova Cave is overshadowed by that of Tularosa Cave largely by right of first discovery. The sequence at Cordova Cave is not quite as long, but preservation is equally remarkable (Martin et al. 1952).

Martin and Rinaldo’s excavation of Tularosa and Cordova Caves contributed to a number of innovations in archaeological method and theory and allowed substantive additions to archaeological knowledge. Specimens from Tularosa Cave were some of the first objects to be radiocarbon dated (Martin 1951b:2). The maize cobs recovered at both sites allowed Martin to discuss the development of agriculture in the Southwest at a level of detail unmatched at other sites (see Cutler 1951). In a simple testament to the value of curated collections, researchers recently extracted ancient DNA from maize cobs Martin excavated at Tularosa Cave to help identify the date (ca. 4400 years ago) of earliest allelic selection by prehistoric farmers (Jaenicke-Deprés et al. 2003). They were able to discern three major changes in Mogollon material culture at the beginning of the Pine Lawn Phase (ca. 150 B.C.), the beginning of the San Francisco Phase (ca. A.D. 700), and the beginning of the Reserve Phase (ca. A.D. 1000; Martin et al. 1952:496). Though Martin was reluctant to identify a single cause for these shifts, he pointed to diffusion for the introduction of pottery and pueblo architecture. Martin identified an increased reliance on wild plants during the Georgetown Phase (A.D. 500-700), evidenced by a 50% drop in the amount of maize present when compared to the Pine Lawn period (300 B.C. - A.D. 500). He noted a diminished reliance on hunting after A.D. 700. These latter inferences, facilitated by the remarkable preservation at Tularosa Cave, mark a nascent cultural ecology in Martin’s conjectures. As before, Martin continued his search for evidence of prehistoric social organization in the record at Tularosa Cave, and in the site report used the term “culture process” for the first time (Martin et al. 1952:496).

Martin and Rinaldo still did not have a good handle on the later occupation of the Reserve region, however, and turned their attention to even later sites in caves and cliff dwellings. In 1952, they excavated five sites: O Block Cave, Y Canyon Cave, Hinkle Park Cliff Dwelling, Cosper Cliff Dwelling, and the Sawmill Site.

O Block Cave

O Block Cave, also known as Skunktail Cave (LA 59869), is located 17 miles south of Reserve on the San Francisco River. It is one of a series of caves that lie underneath massive rock bluffs that overlook a small valley 200 feet above and a third of a mile east of the San Francisco River. A large boulder-fall divided the cave into two areas, the lower of which contained a fairly deep midden deposit. Martin and crew trenched the cave, maintaining horizontal control through the use of two-meter squares, as they had done previously. Vertical control was maintained through the use of arbitrary 20cm levels; occupation debris occurred to a depth of 1.7 meters.

Y Canyon Cave

Y Canyon Cave, some 50 miles east of Reserve, is the largest cave in a conglomerate cliff on the north side of Y Canyon, a tributary that drains east from the site about two miles to the San Augustin Plains. Though Martin (Martin et al. 1954) mentioned test excavations, no ground plan or map of the site was ever recorded, much less published. Artifact analyses suggested that the cave was occupied during the “Pine Lawn through Reserve” phases, a span of over 1000 years from ca. 200 BC to A.D 1100. Apache sherds were
found on the surface, suggesting that the site had been visited, and possibly disturbed, during the historic period.

**Hinkle Park Cliff Dwelling**

Hinkle Park Cliff Dwelling, dating to A.D. 1100 - 1200, is located 30 miles west of Reserve, and 12 miles south of the Blue, Arizona, post office, on a divide between the Blue and San Francisco River drainages. Martin (Martin et al. 1954) noted that their excavation constituted one of the first careful examinations of a Mogollon cliff dwelling. There were nine rooms visible on the surface, two of which were two-story. A midden was present at the cave’s east end.

**Cosper Cliff Dwelling**

Cosper Cliff Dwelling is located in a small cave in east-central Arizona, some 50 miles east of Reserve, New Mexico, and 12 miles south of the Blue, Arizona, post office. It includes two rooms and a small midden area, all of which were excavated and yielded material culture similar to that of its Tularosa Phase neighbors, particularly Hinkle Park Cliff Dwelling.

**Sawmill Site**

After limited testing at the Sawmill Site in 1951, Martin and crew turned their attention to the site in 1952 and again in 1954 (Bluhm 1957). The Sawmill Site is an L-shaped, eight- to ten-room pueblo on Dry Leggett Arroyo near Reserve, also known as the Fox Farm Site (LA 9657; see Martin et al. 1952:9) it has an associated large, semi-subterranean kiva, on the basis of which the site was thought to date to the San Francisco Period, in the late A.D. 800s.

Elaine Bluhm served as field director at the Sawmill Site and, in a departure from previous practice she excavated the site in natural layers (e.g. “humus”, “fill”, and “floor”) rather than arbitrary 20cm levels. In a further departure from previous practice, the site report was single-authored by Bluhm (1957). With Rinaldo’s monograph on Foote Canyon Pueblo (Rinaldo 1959), and their monograph on Mogollon Pottery types (Rinaldo and Bluhm 1956), these are the only monograph-length Southwestern archaeology reports published under Martin’s tutelage at The Field Museum that do not bear his name. It is not clear why this is the case, for these clearly fill the bill as standard *Fieldiana*-style contributions were certainly produced under the Southwest Expedition research program.

**Higgins Flat Pueblo (LA 8682)**

Building on the 1952 season, the goal for the 1953 field season was to discover the youngest pueblo in the region; if possible, the one occupied just prior
to regional abandonment. Higgins Flat Pueblo seemed to fill the bill. The site is located three miles northwest of Reserve, on a small mesa on the southeast side of the San Francisco Valley. Discovered by Rinaldo during the survey in 1947, the site is 100 meters long, 75 meters wide, and forms a three-meter high mound; the north side of which is a 70 cm deep midden. Dating to A.D. 1175 - 1250, Higgins Flat Pueblo has two masonry room blocks, the larger with 25 rooms and the smaller with only six, and plazas and a Great Kiva lying between them. About 12 rooms and numerous test trenches were excavated in 1953 (Martin et al. 1956); the rectangular Great Kiva and a separate pithouse-kiva served as the foci in 1954 (Figure 8; Martin et al. 1957).

Also in 1954 Martina and crew excavated small portions of Apache Creek Pueblo and Valley View Pueblo, Tularosa Phase (A.D. 1100 - 1200) sites in the region.

Apache Creek Pueblo

Located on a terrace on the west side of the Tularosa River near the mouth of Apache Creek, one-half mile southeast of the Apache Creek Post Office, Apache Creek Pueblo consists of between 40 and 50 rooms, laid out in a generally rectangular shape made of blocks of 12 to 18 rooms (Martin et al. 1957). A plaza was found associated with room Block II, and a subterranean structure was evident to the west, but was not excavated. Remodeling was evident. The western segment of the site is several hundred feet from the river’s edge and is separated from the eastern group of rooms by a broad, grassy swale. The latter rooms hug the crest of the terrace overlooking the river bottom 10 to 20 feet below.

Valley View Pueblo

Valley View Pueblo (LA 3257) lies on a hill 200 feet above the Tularosa River some three miles east of Aragon, New Mexico. Consisting of between 25 and 35 rooms, the site was apparently built according to a rectangular plan around a central plaza, with additional plazas possibly located southwest and north of the pueblo. Martin and crew excavated two rooms at this heavily disturbed and pot-hunted site in the hopes that it was a “late site”, but they were “greatly surprised” because the ceramic assemblage indicated an earlier component was present on the hill and that the pueblo itself was Tularosa Phase and probably occupied before A.D. 1175 (Martin et al. 1957).

By the end of the 1954 field season, Martin and crew had conducted a systematic, 15-year long archaeological examination of west-central New Mexico. They had excavated, and better yet published, 20 sites in the region, ranging from pre-pottery sites through the latest known site in the area. Intra-site settlement pattern data, though they did not term them as such, came from extensive excavations in pithouse and pueblo sites. Temporal sequences at cave sites and cliff dwellings confirmed chronologies and provided a wealth of information about perishable technologies and resource utilization.

They still did not know, however, what happened to the Mogollon when the region was abandoned. They therefore followed a hunch and turned their attention northwest, into Arizona. In 1955, Rinaldo led the first of nearly twenty succeeding years of Southwest Archaeological Expedition excavations in Arizona, beginning at Foote Canyon Pueblo (Rinaldo 1959).

Foote Canyon Pueblo

Located, three miles south of the Blue River post office in Blue River Canyon, Arizona, Foote Canyon Pueblo consists of several large room blocks, in the largest of which Rinaldo excavated approximately a dozen rooms, as well as several underlying pithouses and portions of the plaza. Crossdating the pottery sequence, Rinaldo determined site occupation to have occurred between A.D. 1245 and 1325, possibly as late as 1350, thus corroborating the pottery sequences found to the
east, in the Pine Lawn, Tularosa, and San Francisco valleys of New Mexico (Rinaldo 1959:196).

While Rinaldo was away in Arizona, Martin and crew tested six more sites in New Mexico in 1955, though these sites were never published. With six previously excavated but unpublished sites that Martin and Rinaldo tested in 1949, 1950, and 1951, these dozen sites constitute a significant lacuna in Martin's record and therefore deserve attention here.

The Unpublished Sites 1939 - 1955

Excavation records, including room forms, sherd tabulations, and field notebooks, as well as accession records, catalog cards, photographs, and other documents, exist for some, but not all, of the unpublished sites listed in Table 1. Sometime in the mid- to late-1950s, probably in 1956, Rinaldo summarized the excavations at Apache Cave, the Delgar Site, Negrito Cliff Dwelling, the Pine Lawn Camp Pithouse, the Powerline Site, and Saddle Mountain Pueblo in a paper titled these “Notes on Minor Excavations in the Reserve Area, New Mexico” (Rinaldo n.d.). About the same time, he also wrote a brief description of the Perry Lawson Site, though for reasons that remain unclear it was not included in the manuscript. Descriptions below are culled from The Field Museum archives and Rinaldo's (n.d.) text. The sites are described in the order in which they were excavated (see Table 1).

Pine Lawn Camp Pithouse

The Pine Lawn Camp Pithouse is located on a small mesa just west of the buildings that comprised camp headquarters for the Chicago Natural History Museum expeditions to the Southwest from 1939 to 1955. Rinaldo (n.d.:31) notes that the pithouse “was probably part of the same village as the pithouse situated three meters north-

east of Three Pines Pueblo” (Rinaldo n.d.:31; see Martin and Rinaldo 1950b). It was excavated in 1949 as part of a determined effort to find whether kivas were associated with Reserve Phase (A.D. 1000 - 1100) pueblos. The Field Museum curates 177 sherds from Pithouse A, which constitutes only a small fraction of the 1597 sherds (1044 Alma Plain, 35 Alma Rough, and 518 San Francisco Red, Saliz variety) that were excavated (Rinaldo n.d.:30). It is not clear how the curated sample was generated.

Apache Cave

Apache Cave, also known as Hood Station Cave, was excavated in 1950. Located in the southwest wall of Starkweather Box Canyon, about five miles northwest of Reserve, Apache Cave is a small rock shelter with Mogollon and Apache sherds on the surface and an associated pictograph panel nearby. No architecture was evident. Martin and Rinaldo sought, through excavation, to determine the relationship between the Mogollon and Apache occupations. The Field Museum curates eight chipped stone tools, 149 faunal remains, six manos, and 161 sherds.

Bull Basin Cave

One small room at Bull Basin Cave was excavated in 1950, though few written data are available. A sherd tabulation sheet indicates the vast majority of sherds were Alma Plain, though a few corrugated sherds were recovered as well. The Field Museum curates four sherds and 12 faunal remains from this enigmatic site.

Balke Site

The Balke Site is also problematic. Location information indicates only that it is located on the Blue River in New Mexico. The Field Museum curates 41 artifacts from an apparent pithouse at
the site, which is also referred to as “Balke Earth Dam” site. Rinaldo (n.d.:1) suggests the site dates to the Pine Lawn Phase (300 B.C. - A.D. 500).

**Negrito Cliff Dwelling**

Excavated in 1951, the Negrito Cliff Dwelling includes at least two masonry rooms located at the confluence of a small creek and the San Francisco River near Reserve. The Field Museum curates 283 artifacts, including three pieces of adobe, one awl, ca. 100 botanical remains, six pieces of charcoal, one chipped stone tool, 39 faunal remains, one mano, one projectile point, and 145 sherds from the site. A plainware ceramic type that is not Mogollon and that Rinaldo deemed Apache dominates (N = 67) the assemblage. The second most frequent type (N = 23) is Reserve Smudged, and there are 11 sherds of Reserve Indented Corrugated, two Tularosa Fillet Rim sherds, one Alma Plain and one San Francisco Red sherd. According to Rinaldo (n.d.) these sherd types, as well as the two different styles of pictographs at the site, indicate a Tularosa Phase (A.D. 1100 - 1200) occupation prior to the historic Apache occupation.

**Kiehne Pueblo**

Excavated in 1951, Kiehne “Pueblo” (sic?) is located somewhere in west-central New Mexico. There are simply no other geographic data available. The subsite provenience information suggests that Martin and crew excavated a test trench at least 12m long, (in places) 6m wide and (in places) 60cm deep. The excavation strategy reflected in these provenience data suggest that the site is in a rockshelter or cave. There is no evidence for architectural features in the subsite data. The Field Museum curates two awls, a bone tube, a knapping tool, an effigy form, a bracelet, 52 faunal remains, and 150 sherds from this site.

**Saddle Mountain Pueblo**

Saddle Mountain Pueblo is a small nine-room masonry structure on the west end of a ridge about two hundred yards east of the junction of the road between Saliz Canyon and Blue, Arizona, and the logging road from Frisco Divide to the Saddle Mountain Lookout. Four of nine rooms were excavated in 1955; the Field Museum curates one ax, fourteen pieces of charcoal, 11 faunal remains, one hammerstone, and 241 sherds from the site. Rinaldo believes that the site is transitional between the Reserve (A.D. 1000 - 1100) and Tularosa (A.D. 1100 - 1200) Phases.

**Powerline Site**

The Powerline Site has multiple components, including a small six- to eight-room Reserve Phase pueblo and a Pine Lawn Phase pithouse village. It is located on a hill overlooking the broad valley south of the SU Site and about a quarter of a mile south of the intersection of the Reserve – Springerville and Silver City – Springville highways. One pueblo room and part of a pithouse were excavated. The Field Museum curates 333 artifacts from this site, including four undated tree-ring specimens, one piece of bone, 249 sherds, and 79 pieces of chipped stone.

**Delgar Site**

The Delgar Site (aka Hough Survey Site 102 (see Hough 1907), Danson Survey Site 83 (see Danson 1957)) is the largest site ever examined by The Field Museum’s expedition to the Southwest. It consists of three large room blocks, one of which contains over 100 rooms, and covers six acres. Located 1.5 miles below Joseph, on a terrace above the Tularosa River, “the object of the test excavations [in 1955] was to identify on the basis of modern taxonomy some of the pottery types found, and thus to place the ruin roughly in its temporal position (Rinaldo n.d.:22).” Provenience information on the artifacts curated at the Field Museum indicates that at least 11 test trenches were excavated across the site, but it is not clear where these were placed, how they were excavated, or why they were placed where they were. Given the paucity of artifacts (N = 544)
recovered from relatively extensive excavations at this rich site, it appears that Martin and Rinaldo were highly selective in what they kept as a result of their excavations.

Perry Lawson Site

The Perry Lawson Site is a large, ca. 50-room pueblo, consisting of a rectangular block of rooms grouped around a plaza or courtyard, which they thought might be a Great Kiva. It is located due west of the Apache Creek – Quemado Road (Highway 60), near the junction of the road and the trail into Perry Lawson campgrounds in the Apache National Forest. Ten test pits of various sizes were placed arbitrarily across the site in 1955. Numerous excavation records and about a dozen photographs are on file at The Field Museum. Given the size of the Perry Lawson Site and the goals of excavation, it is curious that only 38 artifacts (35 sherds, two chipped stone tools, and a bone awl) are curated by The Field Museum.

[Bar] BO Site

The [Bar] BO Site, excavated in 1955, is problematic. The site name seems to stem from a brand in which a bar is drawn over the capitalized letters “BO.” The site consists of at least one pithouse/kiva and one pueblo (?) room, though no other descriptions have been found in the archives. The Field Museum curates 18 sherds from the pueblo room and 13 sherds from the pithouse. No excavation records have been found.

Frisco Pueblo

Frisco Pueblo is a small pueblo consisting of at least two rooms. Excavated in 1955, it is located somewhere in west-central New Mexico; no additional location data are recorded. The Field Museum curates 38 sherds from two separate locations: Room 1 Fill (31 sherds) and Room 2 Trench or Fill (seven sherds).

In light of Martin and Rinaldo’s otherwise exemplary publication record, these twelve sites constitute comparatively minor lacunae in his record. Perhaps more distressing, however, is the fact that some of the sites Martin excavated, both published and unpublished, have never been registered with the State of New Mexico, though efforts are underway to do so. The objects from all of the sites considered in this paper are now cataloged and available; interested researchers should contact the Department of Anthropology at The Field Museum.
CONCLUSION

In November 1955, Martin again outlined for a popular audience the goals of his research, which were to determine how the Mogollon developed, why and how they changed, and why their culture might have declined and died, stating flatly that these questions “have a direct bearing on our daily lives because their answers may help us to prevent our own civilization from withering” (Martin 1955:7). His optimism seems in retrospect naive and rather unsophisticated, but Martin knew that one way to enhance his ability to attract scarce resources was to argue for the relevance of his work to modern problems. In the 1960s, Martin used such optimism to win one of the first National Science Foundation grants awarded for archaeology and to develop the New Archaeology, but that is a story for another time.

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Recent studies (e.g., Clark 2002; Doolittle and Neely 2004a; Neely 2001) have disclosed several forms of agricultural technology and methodology employed by the prehistoric occupants of the Safford Valley. This paper is devoted to the consideration of another: foothill or bajada agriculture as it appears in Lefthand Canyon. This system incorporated the use of canals, some of which were rock-bordered, to convey waters from springs, rainfall and snowmelt runoff from the mountains to agricultural gardens and fields, some of which were rock-bordered or rock-terraced, and associated habitation sites. Reservoirs and direct precipitation dry-farmed fields were also incorporated. The foothill systems of the Safford Valley appear to constitute some of the best preserved complex water management systems in the American Southwest.

The agricultural system in Lefthand Canyon (Figure 1, Locus 3) is one of the most thoroughly studied locations in the Safford Valley. It has been found to provide excellent undisturbed preservation, substantial time-depth, unusual variety and complexity, and well-defined association with habitation and activity sites. These characteristics provide an unique opportunity to study an agricultural system superbly adapted to the available resources and to gain insights into the interrelated economic, socio-political, and religious systems. Survey has shown Lefthand Canyon to be the locus of one of at

Figure 1.
A schematic map of the seven habitation and agricultural areas recorded in the foothills on the northern face of the Pinaleno Mountains. The seven areas are: 1, Taylor Canyon; 2, Sand Wash - Middle Wash; 3, Lefthand Canyon; 4, Ash Creek Canyon; 5, Frye Creek Canyon; 6, Marijilda Canyon; and 7, Jacob Canyon.
least seven similar water management and irrigation systems located in the northern foothills (bajada) of the Pinaleño Mountains on the south side of the Safford Valley (Figure 1). Although this paper focuses on Lefthand Canyon, findings in other parts of the Safford Valley and elsewhere will be mentioned to put the foothill system into perspective.

HISTORICAL BACKGROUND

The first published reference to the foothill water management and irrigation features and systems of the Safford Valley was by Adoph F. Bandelier (1892). Subsequent investigators (e.g., Fewkes 1904; Hough 1907; Russell 1908; Sauer and Brand 1930; Stewart 1939, 1940a,b; Touhy 1960; Woodbury 1959) have also noted the presence of aspects of agricultural technology in the valley. However, few devoted much time to their study and none recognized the extent and complexity of the systems. My research on these systems began in 1994 when I fortuitously found the Lefthand Canyon system while visiting excavations at the Goat Hill site (Woodson 1995, 1999). Several sessions of fieldwork followed in which I was aided by S. Christopher Caran, Joseph S. Crary, William E. Doolittle, Jerry Howard and members of his “SWAT” team, Everett J. Murphy, Lee Dewester, J. Wesley Jernigan, Jennifer R. Rinker, Sharon F. Urban, Samuel M. Wilson, and students from the Anthropology Department, University of Texas at Austin.

ENVIRONMENTAL SETTING

Lefthand Canyon is located near the western end of the Safford Valley. The survey area for this study encompasses about 10 sq km in the northern foothills of the Pinaleño Mountains (Figure 1, Locus 3). The area is within the Basin and Range Physiographic Zone of Arizona (Wilson 1962: Figure 13), and is situated on a remnant terrace.

Figure 2.

A map of Lefthand Canyon, showing locations of the habitation sites, canals, and agricultural fields. Note that “A” and “B” match the same letters found on Figure 3, and serve to locate the agricultural canals and fields from the south segment of the canyon on this map.
Lefthand Canyon drainage courses in a south-to-north direction through the center of the survey area. The survey area ranges in elevation from approximately 1,158 m (3,800 ft) to about 960 m (3,150 ft) where Lefthand Canyon joins Cottonwood Wash. It is in the Chihuahuan Desert subdivision of the Lower Sonoran Life-zone (Lowe 1964: 15-20), with a Southwestern Desertsrub type biotic/vegetation community. The creosotebush biotic community dominates (Lowe 1964: 20-24). A riparian association characterizes Lefthand Canyon (Lowe 1964: 60-63). Warm temperatures allow a growing season that averages between 200 to 250 days in length (Bronitsky and Merritt 1986:21; Sellers and Hill 1974:422). The valley averages about 25.4 cm (10 inches) of precipitation annually (Sellers and Hill 1974: 7-8), but almost half falls during the months of July and August. The evapo-transpiration rate for the area is over nine times greater than the rainfall.

THE LEFHAND CANYON SYSTEM

The Lefthand Canyon settlement and agricultural system extends in a linear fashion for a distance of nearly five km along both sides of the intermittent, at times perennial, drainage. However, a closer inspection of these systems reveals that there are significant differences that divide them into two linear segments. About a third of the way down the canyon (Figure 2) the system takes on a new aspect, one that is characterized by several canals that do not connect with the up-stream (southern) canals and by the presence of a greater number of habitation sites. Several of these sites date earlier than the Goat Hill site and all present very different site plans and settlement patterns. A description of the two segments follows, and the significance of the differences is considered.

The Southern Segment of the Canyon

The southern segment of Lefthand Canyon is distinguished by three prominent features: a) the Goat Hill site (AZ.CC:1:28 [ASM]), b) the canal and terrace system that trends down the east side of the canyon, and c) the offtake of a canal that courses immediately west of the canyon.

The Goat Hill Site. This is the only habitation site found in this segment of the canyon. This site is thusfar unique in the Safford Valley, although it is suspected that similar sites may be present in areas not yet well surveyed. This 35 room masonry pueblo, situated atop an artificially leveled butte, has a central plaza and a D-shaped kiva. It was constructed and occupied during the Classic period from about A.D. 1275 to 1325. The construction of this site has been attributed to northeastern Arizona Kayenta-Tusayan immigrants into the Safford Valley (Woodson 1995, 1999).

Canals, Gardens, and Fields on the East Side of the Canyon. About 250 m to the west-northwest of the Goat Hill site lies the offtake of the primary canal that supplied water to irrigate a linear system of agricultural fields on the east side of the canyon. The majority of the canals and smaller ditches found in this segment of the canyon (Figures 3, 4 and 5) were rock-bordered, and several small test excavations indicated that most were probably rock-lined as well. Linear borders and grids of rock, as well as rock-faced terraces, defined “improved” irrigated agricultural fields and gardens. These improved canals, fields, and gardens reflect a high energy cost and investment.

In addition, a few areas that were devoid of linear borders of rock and rock-faced terraces were tentatively listed as “unimproved” fields and gardens on the basis of their topography, associated artifacts, and locations in relation to canals and site. The majority of these gardens and fields appear to have been irrigated.
The Canals. In this segment of Lefthand Canyon the drainage channel has cut deeply, and there is currently no perceptible floodplain present. The canal system begins as a small, shallow, broadly U-shaped channel in the earth that branches from a cutbank on the east side of Lefthand Canyon at about 3.5 m above the present floor of the canyon. This channel courses down slope for some 50 m and then becomes more clearly visible as two closely spaced parallel alignments of unshaped slabs, cobbles, and small boulders of local rock (Figure 4). In places these alignments are clearly discernible; in others they are not. The course of the channel, which averages about 65 cm in width, is seen to be rather sinuous, perhaps to better follow the lay of the land and maintain a channel grade of approximately 1.5 to 2%. Occasionally, an alignment of cobbles was

Figure 3.
A map of the rock-bordered canals and rock-faced terraced fields located just west and below the Goat Hill site in the south segment of Lefthand Canyon.

Figure 4.
A well-defined portion of the rock-bordered canal located just west and below the Goat Hill site in the south segment of Lefthand Canyon.
constructed at right-angles across the canal channel, but only obstructing the lower part of the channel. These partial blockages of the channel may have acted to slow water flow (cf. Lindsay 1961:183-184), but often mark the presence of canal offtakes. The use of rock to border one or both of the walls of the canals is functional. As found in other areas (e.g., Fish and Fish 1984:Figure 4; Lindsay 1961), it provides channel wall stability in the loose, sandy soils characterizing the area. Most of the earth-walled canals have segments of their courses rock-bordered in locations where the soils are either particularly loose and sandy or in places where a washout of the canal walls was likely.

The canal continued northward down the terrace along the tops of several finger-like ridges. The canal branches into several rock-bordered, slightly smaller, secondary and tertiary canals/ditches as it courses down-slope. Water could be turned out from the primary canal directly into gardens and fields through small sluices or gates in the canal walls, or into the smaller ditches that branch from the canal. A total of nearly 1,230 m of canals and ditches was mapped in this system. The primary canals in this system were about 1,050 m in length (Figure 3). The remaining canals/ditches mapped, totaling approximately 180 m, represent the secondary and tertiary branchings extending to the fields from the primary canal.

Gardens and Fields. Following definitions found in the literature (e.g., Doolittle 2000:82-85), and based primarily on size and proximity to habitation sites, it is evident that both gardens and fields were present and canal irrigated. Both “improv-
ed" gardens and fields (those with rock constructions such as linear borders, grids, terrace walls, and check dams) and “unimproved” gardens and fields (those lacking any rock improvements) are present in the southern canyon segment.

**“Improved” Gardens and Fields.**

About 100 m north of the primary canal offtake from the Lefthand Canyon drainage, a section of the primary canal and associated features that formed a complex garden was chosen for more detailed mapping (Figure 5). This area, is similar to the small irrigated gardens at Hopi (Maxwell and Anschuetz 1992:Figure 3.2). The larger rock-bordered garden “plots” are clearly discernable and range from about 4.5 sq m to 40.5 sq m in area, with an average area of about 15.9 sq m. Downslope, north of this detail mapping area, the rock-bordered and terraced areas increase in size and become fields, with a maximum size of about 18 by 20 m (360 sq m in area). Small turn-out sluices or “gates” were found in the alignments of rocks bordering the canal. These gates would allow controlled amounts of water to be diverted from the main canal into smaller canals, directly into garden and field areas, and into smaller stone-bordered areas, that, for want of a better term, I have called “planters.” The patterning of the gates was such that they were present most frequently at the up-slope corner of an adjacent plot or planter. These gates could be easily closed or opened by the insertion or removal of rocks and mud.

Figure 6.

A schematic map of 22 of the rock-faced terraced fields located in one of the “erosion” channels in the southern portion of Lefthand Canyon. See Figures 7 and 8 for photographs of portions of this terraced field system, and Figures 9 and 10 for photographs of “splash pads.”

The “planters” illustrated in Figure 5 consist of rock-bordered areas that vary in form and range in area from approximately 0.7 sq m to 4.2 sq m. They occur adjacent to rock-bordered canals and smaller offtake ditches. In many, but not all, cases they were connected with rock-bordered canals or
ditches by a “gate.” Their use was most likely as smaller irrigated garden areas. These are similar to the planting “basins” recorded in the gardens at Hopi (Hack 1942:36-37; Maxwell and Anschuetz 1992:Figure 3.2) and the “waffle” gardens at Zuni (Bohrer 1960; Doolittle 2000:97-98; Forde 1931; Stewart 1940b).

It seems possible that some of these features, the ones without a gate, may have been designed to hold water. In such a case, the empounding of a small amount of water, when the canal was dry or being used for other purposes, could have served to provide a source from which the agriculturalist could dip, with a gourd or a small ceramic vessel, to apply water directly to the base of wilting plants. This form of labor-intensive hand-watering has been documented ethnographically in the American Southwest among the Zuni (Doolittle 2000:98; Ladd 1979:497, Figure 12) and the Akimel O’odham (Castetter and Bell 1942:160), as well as in the Valley of Oaxaca in southern Mexico (Kirkby 1973:117-119) and in Guatemala (Wilken 1987:178-193). Prehistoric use of hand watering has been suggested for the Tonto Basin (Welch 1994:106) and the Valley of Oaxaca (Neely et al. 1990: 146-150). About 50 of these planters present in this segment of the canyon.

Further down canyon (north) from the gardens, and interspaced between some of the ditch-bearing finger-like ridges of the canyon’s terraced surface, are what appear to be erosional channels. These channels, some relatively narrow and deep and some quite broad and shallow, have been cleared of rocks. The rocks apparently had been thrown to either side of the channels and also used to modify the channel into a series of leveled areas that proceed in a stair-step fashion down slope (Figures 6 and 7). This was accomplished through the construction of dry-laid linear borders and terrace walls of unmodified cobbles and boulders at nearly right-angles to the channel of the drainage to form a series of adjacent “improved” fields. Varying from one to at least five rocks in height, at least 170 borders and terraces are pre-
sent in this system. These provided approximately 40,000 sq m of field area for cultivation.

One set of 96 mapped rock-faced terraces was found to extend about 575 m. However, side channels diverging from and joining the terrace system were also seen to have terrace walls; adding about 100 m to the complex. The terraced areas (the effective area of cultivation) comprising the southernmost 22 fields in this complex (Figure 6) extend about 473 m in length, range from about 12 sq m to nearly 124 sq m in area, have an average field area of nearly 55 sq m, and provide a total cultivable field area of about 1,200 sq m (about 0.12 hectare).

Thirty-five of the terrace walls in this complex (12 are shown in Figure 6) were found to have a small cobble-filled or cobble-outlined area located down-slope from, and immediately in front of, the lowest portion of the wall (Figures 6, 8, 9, and 10). These features may have served as a type of "splash-pad" to prevent the erosion produced by water flowing over the terrace wall onto the terrace below. Excavation will be necessary to determine if the small earth-filled rectilinear or curvilinear areas bordered by rocks may have formed a small pool that served a similar function and possibly also produced a type of "seepage" field (Hack 1942:34).

Christopher Caran, a professional geologist specializing in Holocene geomorphology and hydrology, examined these field areas. Caran noted that a great deal of human effort was expended over the entire surface of the terrace to remove the naturally occurring cobbles and boulders from the field areas, to transport them to the areas bordering the fields, and to construct the terracing walls. In addition, Caran's examination disclosed a rather surprising observation. This well-preserved stone-faced terraced field system suggested to him that the entire channel, which was initially assumed to be a natural erosional feature, was largely a product of landscape modification by the agriculturalists. In addition to systematically removing the rocks, he considers it probable that humans, rather than natural erosional processes, produced the channel in which the terraces were constructed. He also observed that the sandy soils forming the fields in this channel might have been purposely brought in from elsewhere to provide a choice growing environment for whatever was cultivated therein.

Large rock-bordered features formed the down-slope end of many of the canals and ditches.
These features, circular in form and ranging in size from about 3 m to around 8 m in diameter, appear to be analogous to the O'odham tail-water gardens or “second gardens” (Crosswhite 1981:64; Rea 1979; Welch 1994:108, Table 5.4). A variation on this theme, employing rock-bordered grids at the terminus of rock diversion walls, has been reported by Rogers (1970) for the Upper Little Colorado region southeast of Young, Arizona. In Lefthand Canyon, most of these canal terminus gardens currently contain large mesquite trees. It seems probable that these rock-bordered areas were used as “planters” for mesquite trees and other plants to supplement the diet.

Crescent-shaped and circular features, usually constructed of boulders and ranging from about one to five m in diameter and one to three courses high, were found in both irrigated and dry-farmed fields. The crescents are oriented with their open end facing up-slope, were probably yet another form of water and soil catchment feature, and were found in both improved and unimproved fields. Circular features were found attached or adjacent to canals, but others occurred isolated in field areas. Like the rock-bor-
dered features discussed immediately above, these crescents and circles of rock also are frequently found to have mesquite trees growing in them. These features have been found in the Upper Little Colorado region southeast of Young, Arizona (Rodgers 1970).

Non-irrigated improved fields were found in several locations in this segment of the canyon. Perhaps the best example was found in the small intermittent drainage that begins at the toe and northern edge of Goat Hill butte and courses around the east side of the butte. In this drainage the agriculturalists constructed a series of about 40 dry-laid rock checkdams and rock-faced terraces at right-angles to the channel. These features range from about two to 15 m in length and from one to five courses in height. In addition, several linear borders of rock, ranging from about 10 to 40 m in length and one course high, were constructed along the up-slope side of the drainage, following the contours of the land and paralleling the channel.

"Unimproved" Fields. Although most of the fields found in the southern segment of the canyon were found to be "improved", a few level areas probably served as "unimproved" fields. While some fields of this type were identifiable due to the presence of offtakes from rock-bordered canals leading past these areas, the absence of the rock water management features and thinner sherd scatters made determination of their shapes and sizes highly speculative. Even more acute difficulties occurred in the identification of fields that received their moisture directly from precipitation.

The Canal Immediately West of the Canyon. A cleverly designed canal has its offtake about 450 m to the southwest of the Goat Hill Site (Figure 2). This canal carries water from Lefthand Canyon about 650 m to the northwest where it intersects a natural drainage coursing northward from the upper foothills of the Pinaleño Mountains. From this point of intersection the canal carries both the water from Lefthand Canyon and runoff from the natural drainage. From the offtake northward, the channel exhibits modifications such as the placement of dam-like constructions across the channel and the rock armor ing of cutbank areas as it progresses northward to empty into Lamb Tank. Another human-modified drainage flows from Lamb Tank and courses northeast to drain into the lower reaches of Lefthand Canyon. The total length of this canal is about 4,450 m. Improved and unimproved agricultural fields border both sides of this canal.

Reservoirs. Ceramics found in the vicinity of Lamb Tank, and other tanks along the north face of the Pinaleño Mountains in the Safford Valley, suggest the use of reservoirs as part of foothill canal irrigation agriculture during the time of deteriorating climatic conditions at the end of the Late Formative and beginning of the Early Classic periods (Rose 1994:357-358; Van West and Altschul 1994:400-403). Prehistoric wells and reservoirs are found elsewhere in the Southwest, dating from ca. 10,000 BP to A.D. 1400 (e.g., Draper 2004; Evans 1951; Wheat 1951). While this type of feature is traditionally thought of as a domestic water resource, it also may have served for irrigation. Surveys (Neely 1995a; Neely and Crary 1998) suggest that at least some of these reservoirs were originally natural marshes or cienegas (see Doolittle 1997) that were modified to become larger water impoundment areas. These impoundments evidently functioned as did the Papago reservoirs recorded in southern Arizona and northwestern Sonora by Castetter and Bell (1942) and in Iran by Bonine (1982). Castetter and Bell (1942:169-170) describe the Desert Papago balsa as an embanked reservoir, from which the water is allowed to enter a ditch through a gate into a cultivated field. They state that it is possible to raise a crop from the water of a single filling of the balsa. Bonine (1982:154) reports that such pond-like features, called estakhr sarkh and functioning in Central Iran in both
ancient and modern times, serve villages when water is not present in sufficient flow or quantity for agricultural irrigation. The water is impounded and collected until enough is present for irrigation; then it is drained by canals taking the water to the fields to complete the irrigation process. The two examples noted above illustrate the precise procedure currently conducted with Lebanon Reservoirs #1 and #2, just south of the city of Safford. It is probable that the modern Lebanon Reservoirs are also refurbished and enlarged prehistoric reservoirs.

The Northern Segment of the Canyon

The Sites. The sites in the northern portion of Lefthand canyon (Figure 2) are more numerous, have greater lengths of occupation, have a different site layout (plan), and have a different settlement pattern than found in the southern segment of the canyon. A total of 18 habitation sites, and several activity sites (e.g., roasting pits, bedrock metates and mortars), have been recorded by survey. Of the 18 habitation sites, five (AZ.CC:1:11, 43, 52, 53, and 56 [ASM]) have been subjected to professional test excavations (Jernigan, personal communication, 1997; Rinker 1998; Rinker and Neely 1998), but all have had varying amounts of looting. Seventeen of these sites are relatively small, consisting of an estimated two to twenty pithouses, pitrooms, and/or masonry surface rooms, depending on their period(s) of occupation. These sites range in area from about 4.5 sq m for a field house to about 5,800 sq m for a pithouse village. The Spear Ranch Site (AZ.CC:1:11 [ASM]) is in a separate category as the largest site in the entire canyon; it is estimated to have about a hundred rooms and an estimated area of about 10,000 sq m. The ten sites on the west side of the drainage and eight on the east form a continuous line of habitations paralleling the course of the drainage. Most are located on the first terrace above the floodplain, providing a secure location from floods while giving close access to water and the irrigated gardens and fields. The sites were recorded as separate sites and are assigned 18 individual ASM numbers. However, their juxtaposition would suggest that they probably should be considered as one large site, or possibly two, with each side of the Lefthand Canyon drainage forming a separate linear settlement pattern.

The Canals. The primary canals on both sides of the drainage have offtakes that are clearly separate from the canals in the south canyon segment. These are primarily earth-walled canals, but large portions of their channels were bordered with rock. The total length of all of the canals recorded in this canyon segment is between about 8,000 and 12,100 m. Canals were constructed to supply water to fields located in three distinct locations: the floodplain of the canyon, the first terrace above the floodplain, and the higher terraces. In addition to supplying water for the agricultural fields, it is evident that the canals also supplied water for domestic purposes and household gardens.

Canals on the West Side of the Canyon. One primary canal with several branches was recorded on the west side of the canyon (Figure 2). Canal grade on the west side ranges from about 1.2% to about 1.8% (Rinker 1998). A study of the canal fill indicates that the canal was well designed and evidently functioned efficiently (Caraan, personal communication, 1997). A late 19th/early 20th-century re-excavation of the northern-most portion of this canal (Ross Bryce, personal communication, 1997) increased the grade to about 2.5% and resulted in a rapid flow and rapid sedimentation (Caraan, personal communication, 1997).

The total traceable length of the canals mapped on the west side of the lower canyon is about 5,396 m. Except for a few relatively short areas, where erosion or alluviation has occurred to obliterate or hide the channel, this canal is clearly visible. It took water from the west side of the drainage of Lefthand Canyon at an elevation of about 1,060 m (about 3,480 ft), it coursed along
the first terrace above the floodplain, and ultimately emptied back into the drainage of Lefthand Canyon at an elevation of approximately 978 m (about 3210 ft). This canal had a shallow U-shaped cross-section that ranged from about 30 cm to 1.0 m in width, and averaged about 50 - 60 cm wide at ground level. In places, a low-mounded spoil bank is visible to one or both sides of the channel. In several locations, including the floodplain of Lefthand Canyon, sections of the channel are clearly rock-bordered. This canal system apparently provided waters to agricultural fields as well as to habitation sites situated along its course for domestic uses and gardens.

At a point about 510 m from its point of origin, a ditch from a higher terrace to the northwest joins this canal (Figure 2). This ditch is clearly visible for the 175 m it courses along the side of the higher terrace up to the top, where it branches west and north and is traceable for another 30 m before it fades from view. This side channel is thought to augment the canal’s water flow with runoff from the catchment area of the higher terrace.

Almost immediately after its offtake from the drainage, the primary canal had small offtakes to the east that supplied water to rock-bordered fields on the floodplain and the first terrace. About 700 m from its head the canal exhibits major branchings. The primary canal continues its course at the rear of the first terrace, at the toe of the second terrace, to supply water to habitations, gardens, and improved and unimproved fields on the first terrace. One set of branchings turns west to water mostly unimproved fields on the upper terraces while another set of branchings turns east to water improved and unimproved fields on the floodplain.

Ceramics and flaked stone artifacts were found along the primary canal, and the density of the artifact scatter increased in the vicinity of habitation sites. Agricultural fields adjacent to this canal also had a relatively dense surface scatter of ceramic and lithic materials. Large numbers of fragmentary grinding stone artifacts (i.e., manos, metates, mortars, and pestles) were found along both sides (but mostly on the east side) of the canal from a point about half way down the canal to where it terminates. Thirty-seven fragments of grinding stones were found along this 1,100 m segment of the canal. E. J. Murphy (Safford
Archaeological Site Steward) commented that about ten complete and nearly complete grinding stones that he had seen along this canal in the early 1990s were now missing, presumably removed by local collectors.

**Canals on the East Side of the Canyon.** At least one primary canal, with several branches, was clearly visible on the east side of the canyon. This canal was constructed to course west of the habitation sites and along the edge of the first terrace above the floodplain. The total length of this canal is approximately 2,607 m. However, there is some evidence that a branch of this canal coursed at the toe of the second terrace above the floodplain and east of the habitation sites (Figure 2). Like the canal supplying Lamb Tank, this canal appears to have used a natural drainage channel for part of its course. It is about 4,142 m in length, but has been indicated by a dashed line on Figure 2 because of poor definition. If the latter feature was a canal, this would modify the total canal length for the east side of the drainage in this segment of the canyon to about 6,750 m.

**Irrigated Fields and Gardens.** The fields and gardens in this segment of the canyon also have a linear distribution down-slope and paralleling the drainage. In contrast with the southern segment of the canyon, the northern fields and gardens were more difficult to distinguish because they had fewer improvements (i.e., rock linear borders, terrace walls, etc.). However, many of these fields were recognizable in survey, especially on the west side of the canyon, because of well-defined turnouts (Figures 2 and 11) in the primary canals and the presence of relatively dense scatters of ceramics and artifacts. The latter suggests that household garbage was used as mulch and fertilizer (see Donkin 1979:2; Neely et al. 1990:134-135; Roberts and Barrett 1984; Stewart and Donnelly 1943:42-43; Wilken 1969:231; Wilkinson 1982). In addition, several fields were clearly evident on aerial photographs (e.g., 9-29-78 BLM 24CN AZ-78AC 1-8-69). Four of the best defined fields, representative for this portion of canyon, were approximately 43 by 54 m (2,322 sq m), 64 by 93 m (5,952 sq m), 93 by 122 m (11,346 sq m), and 100 by 143 m (14,300 sq m) in size. The estimated maximum total area involved in irrigation agriculture in this west part of the north segment of the canyon is about 38 hectares.

From the offtakes in the primary canal, small secondary canals course toward the east through the areas suspected to be remnants of unimproved prehistoric fields. What appear to be even smaller tertiary "field ditches", or perhaps furrows, branch from these secondary canals. The field ditches or furrows course in what appear to be a sinuous pattern through the fields, crossing the contours of the gentle slope of the ground surface only enough to provide a slight slope to promote the gravity flow of the water through the channels. The field ditches or furrows course from one side of the plot to the other in a back-and-forth meandering fashion until they reach the lowest corner of the field. There they appear to empty onto areas that probably served as floodplain fields further down-slope, and ultimately to flow eastward into the Lefthand Canyon drainage. It should be noted that the above description of the fields is more of a vague perception than a set of well-defined, observable characteristics. It would be highly desirable to scrape from 5 to 10 cm of the topsoil of portions of one or more of these fields to verify and augment the description provided above.

Because these cultivated areas were situated between the linear arrangement of habitation sites (Figure 2), there was a question as to whether some of these areas should be classified as "fields" or as "gardens" (Turner and Sanders 1992:265-266). Most were classified as "fields" because of their large area and because smaller plots were discovered in even closer proximity to some of the sites. Site AZ.CC:1:52 (ASM), on the west side of the canyon, provided an excellent example of a garden. This site occupies a small rincon on the first terrace above the floodplain. A small rock-
bordered ditch branches from the primary canal that follows the contours just above and west of the site. This ditch courses down onto the rincon’s living surface to supply water to a small garden indicated by rocks forming linear bordered and gridded areas (Rinker 1998: Figures 3.11, 3.12). The area of this garden is at least 130 sq m, but, because this area had been disturbed by looting, it probably was originally larger.

As the eastern most canals recorded on the west side of the drainage (Figure 2) lead to the floodplain, irrigated floodplain fields were evidently also present. In a few areas, remnants of rock-bordered fields were seen. Their vagueness is due to the natural erosional and alluvial processes that characterize all floodplains. The apparent alignments on the floodplain in these areas probably represent features analogous to the “channel-bottom weir terraces” reported by Doolittle (1988:48-50). If so, they could represent a combination of irrigation, floodplain, and floodwater field technology. Generally similar canals and fields were found in the Safford Valley by Clark (2002) on the floodplain of the Gila River, elsewhere in the Southwest (e.g., Neely 1993, 1995b), and in several locations in Mexico (e.g., Doolittle 1988; Neely et al. 2005; Woodbury and Neely 1972).

Dry-Farming/Runoff Agriculture. Recognition of dry-farming or runoff agriculture for the entire canyon is based largely on the presence of habitation site location, specific artifact types, and water-management features, all of which lend insights as to the possible or probable presence of fields. The present sample suggests that a broad range of field sizes (from about 100 sq m to over 10,000 sq m) were used. In addition, a range of topographic and geographic locations was used, and in specific topographic situations the field size range and the use of certain water-management features appear to be positively correlated. While dry-farmed fields in the Safford Valley mostly date from the Late Formative and Classic periods, the tradition can be traced back into the Early Formative period. The numerous dry-farmed fields in the Lefthand Canyon area mirror those found throughout the Safford Valley, and indicate that they played an integral role in the subsistence and economic systems.

In Lefthand Canyon, as throughout the Safford Valley, the Early Classic period (A.D. 1150 - 1300) communities were often surrounded by extensive dry-farming systems that consisted of rockpiles, stone grids, linear borders, check dams, and terraces (Gilman and Sherman 1975; Neely 2004; Neely and Doolittle 2004; Seymour and Euler 1990; Seymour et al. 1997a). These systems were evidently used in a symbiotic relationship with the expanding irrigated agricultural systems in marginal non-riverine settings such as in Lefthand Canyon. The characteristics of the Early Classic dry-farming fields appear much the same as those fields dating to the Late Formative period. It seems likely that many of these fields would have gone out of use because of the severe lack of precipitation during the “Great Drought” years between A.D. 1275 and 1299 (Dean and Robinson 1982:Figures 8.3 and 8.6). However, the Late Classic period (A.D. 1300-1450) witnessed the final expansion of large upland dry-farming systems (Neely 1997a, b; Neely and Rinker 1997). The upland settlements in the Safford Valley, including those in Lefthand Canyon, were evidently abandoned by about A.D. 1385, as indicated by the absence of key diagnostic ceramic types at any of the sites recorded.

Some (e.g., Bradfield 1971:18; Glassow 1980:45) have opined that dry-farming was probably a minor contributor to the subsistence base in the Southwest. However, recent studies in the Safford Valley (Doolittle and Neely 2004) and the Tucson area (Fish et al. 1992) have shown dry-farming to play a significant role in supplying food and fiber to the economy. For example, about 10 km to the south-southeast of Lefthand Canyon, the rock-bordered grid system (gridded fields) located north of the present community of Bryce, Arizona (Doolittle and Neely 2004) forms the largest dry-farming sys-
tem yet recorded in the Safford Valley. The system was constructed within an area of approximately 6.0 sq km, and consists of 15 to 33 (depending on how one interrelates the gridded areas) irregularly juxtaposed, but separate, areas of rock-bordered grids. They comprise a total gridded area of about 822,000 sq m (82.2 hectares or 203 acres). It is estimated that a maximum of seven percent (about 57,500 sq m or 5.75 hectares) of those fields were constructed beginning early in the Late Formative period (Neely and Doolittle 2004). The remaining fields were constructed in the subsequent Classic period.

A number of studies (e.g., Fish et al. 1992; Doolittle and Neely 2004; Seymour et al. 1997b), have convincingly shown that many, if not most, of the dry-farmed fields that depended solely or predominantly on direct precipitation for moisture were dedicated to the cultivation of agave for the production of mescal, and very probably other products (cf., Parsons and Parsons 1990). It seems highly probable that the dry-farmed fields in the Lefthand Canyon area served a similar purpose. A feature frequently found adjacent to the dry-farmed fields is the roasting pit. The excavation of these features will undoubtedly provide insights into the crops grown in the associated fields as well as dates when the fields were most likely cultivated. Radiocarbon samples recovered from a roasting pit in Lefthand Canyon were dated to A.D. 1185-1267 (TX-9258) and A.D. 1374-1452 (TX-9259).

The Southern Segment of the Canyon

Based on proximity, the canals and fields in the southern segment of the canyon most probably resulted from the efforts of the inhabitants of the Goat Hill site. This site, the only site recorded in this segment of the canyon, has been dated by excavated archaeomagnetic and radiocarbon assays, as well as pottery, to the period of A.D. 1275 to 1325 (Woodson 1999). This chronological placement is supported by the few ceramic sherds seen on the nearby garden and field surfaces during survey.

The Northern Segment of the Canyon

Unlike its southern counterparts, many sherds were found associated with the canals, gardens, and fields of the northern segment. The water management and irrigation agriculture in Lefthand Canyon may be seen as a system that has been augmented through time. The northern portion of the canyon was first used late in the Early Formative period, perhaps as early as ca. A.D. 700-750. A few sherds of the earliest ceramics, Cerros/Three Circle Red-on-white and Pinaleno Red-on-brown, were found at two multi-component sites on the west side of the drainage (Figure 2). It is not known if the canal associated with these two sites dates this early. However, it is evident that the major occupations were during the subsequent Late Formative and Classic periods, and persisted as late as A.D. 1385. Five of the sites, all on the east side of the drainage (Figure 2), date only to the Late Formative period (ca. A.D. 800 to 1100), and four sites date only to the Classic period (ca. A.D. 1100 to 1450). The remaining nine sites are multi-component, with two sites on the west side of the canyon having the few sherds dating to late in the Early Formative period.

The dating of the large Spear Ranch site (AZ. CC:1:11 [ASM]) is not well established, but test excavations there by Eastern Arizona College recovered ceramics that spanned the period of ca.
A.D. 900 to 1400 (Jernigan, personal communication 1995). Assessment of the surface ceramics, as well as a review of the ceramic types reported by Jernigan (personal communication 1995) and Woodbury (1959), supports the ca. A.D. 900 date for the founding of the site, but the decoration and vessel forms of Gila Polychromes found suggest a terminal date of ca. A.D. 1385. This would indicate that the Spear Ranch Site was occupied before the Goat Hill site, and had sustained its occupation after the Goat Hill site was abandoned.

OBSERVATIONS

The Foothill Settlement Pattern

Although the focus of this paper is on Lefthand Canyon, a reconnaissance of the northern foothills of the Pinaleño Mountains revealed that foothill systems had been constructed along the entire northern face of the mountains, evidently wherever springs and drainage catchments of sufficient size were present. The Taylor Canyon system (AZ.CC:1:70 [ASM]; Neely and Rinker 1997), about 5 km west of Lefthand Canyon, was the system recorded furthest to the west (Figure 1, Locus 1). The Jacobson Canyon system (Neely and Crary 1998), about 26 km southeast of Lefthand Canyon, was the system recorded furthest east (Figure 1, Locus 7). In scale, the Taylor Canyon canal was the smallest and least complex system found, while the Marijilda Canyon system (Neely and Crary 1998), associated with the Marijilda site (AZ.CC:5:6 [ASM]) and located some 19 km southeast of Lefthand Canyon, was the largest and most complex system recorded (Figure 1, Locus 6). Evidence from the seven systems varies greatly in quantity and quality. This distribution is verified and expanded by the observations of Bandelier (1892), Hough (1907), and Sauer and Brand (1930). In addition, Bandelier's (1892:414) reference to yet another canal with branches lying in a similar topographic situation about “12 miles” southeast of Globe, Arizona also implies that the systems reported herein are not unique to the Safford area.

The foothill canals found begin at elevations as high as 1,311 m (about 4300 ft) and extend northward to elevations as low as 845 m (about 2770 ft). The heads of many of the systems are represented by the presence of a spring or the most likely offtake point from a spring-fed stream, as well as augmentation by runoff from precipitation and snow-melt from much higher elevations. The terminus of the systems has been placed at the furthest point downslope that the system can be traced, although it is likely that some of the systems recorded were more extensive than indicated by surface evidence visible today. While the seven systems shown on Figure 1 do not extend northward to the Gila River floodplain, findings at the Bandelier site (AZ.CC:1:7 [ASM]) indicate that this site, situated at the edge of the first terrace above the floodplain, received water from a canal coming from the foothills of the Pinaleño Mountains (Bandelier 1892:410; Neely and Rinker 1997). The documented foothill canal systems range from about 600 m to 12.5 km in length. The Marijilda Canyon system is the largest recorded; with a watershed collection area of at least 28.3 sq km (USGS 1998), and an additional 15.5 sq km of watershed probably augmenting that system from the adjacent Deadman and Rincon Canyons (USGS 1998).

Considering its juxtaposition and contemporaneity with other agricultural and settlement patterns now known for the Safford Valley (e.g., Clark 2002; Neely 2004; Neely and Doolittle 2004), it is unlikely that the foothill pattern existed as an isolate, completely separate from other contemporary manifestations. The combined use of the foothill and other agricultural and settlement manifestations may at least partially be explained as a superbly adaptive solution to a deficit of available moisture (Neely 2004). This pattern is seen as a part of the intricately interconnected use of multiple microenvironmental zones for the production of a varied agricultural crop and to protect the agriculturalists from the vagaries of unpredictable precipitation (Neely 2004; Neely and Doolittle 2004).
A major advantage of the foothill systems was a frequent availability of water for the entire year. Interviews with local inhabitants as well as BLM and Forest Service personnel indicate that snowmelt is present and springs function on the north side of the Pinaleño Mountains frequently nine to 10 months of the year into the first week or two of July, when the monsoonal rains start. This insured year-round water availability for domestic use and probably enough water for irrigation farming to permit several well-planned crops during the 200 plus day growing season this area usually enjoys (Bronitsky and Merritt 1986:21, 24; Sellers and Hill 1974:422). It probably was this water availability characteristic that made Lefthand Canyon, and the other foothill systems, habitable and very desirable, during the “Great Drought” years between AD 1275 and 1299 (Dean and Robinson 1982:Figures 8.3 and 8.6). The foothills would have been a lightly occupied location for habitation for the migrants from the four-corners region who arrived in the Safford Valley at this time.

A Highland-Lowland Pattern. The use of the foothills vis-à-vis the Gila River floodplain may represent a form of highland-lowland or infield-outfield (Wolf 1966:21) land use. Similar highland-lowland patterns are documented for both ancient and modern cultures throughout the world (e.g., Fish et al. 1992; Kirkby 1973; Özfirat and Helwing 2004; Vogt 1969). There also appears to be a highland-lowland pattern present during the Classic period along the Gila River in the Casa Grande region (Crown 1987), and during the Reserve phase (ca. A.D. 900 to 1100) in the San Francisco River drainage between the lowland WS Ranch Site (LA 3099) near Alma, New Mexico and the highland sites of Devils Park to the east in the Gila National Forest (Accola 1981; Neely, personal observation, 1980; Peterson 1988). Pool (1985) has suggested that the prehistoric Mogollon practiced a semi-sedentary annual round of subsistence procurement generally similar to that documented for the Apache. While the latter pattern suggested for the Mogollon is not as similar to the pattern seen in the Safford Valley as those from south-central Arizona (Fish et al. 1992) and west-central New Mexico (Neely, personal observation, 1980; Nelson and Anyon 1996), it does suggest a similarity in the subsistence/economic pattern that utilized several environmental zones. Interestingly, interviews with Safford Valley agriculturalists and ranchers document a similar historic and present day highland-lowland pattern of families using lands on the floodplain for farming as well as in the foothills for cattle grazing.

However, because of the presence of springs and large runoff areas, the Pinaleño Mountain foothills comprise a special form of highland-lowland relationship with the Gila River floodplain. This is a relationship evidently not present in the nearby San Pedro Valley (Patricia Cook, Jeffery Clark, Henry Wallace, e-mail correspondence, 2004) or Tonto Basin (Van West and Altschul 1994:362). At this stage of study, the uniqueness of the Safford Valley foothills seems to be due to physiographic and hydrological differences rather than cultural differences.

Origins of the Foothill Agricultural System

The origins of the foothill agricultural systems in the Lefthand Canyon are not clearly discernable. However, the survey and test excavations provide some insights.

The Southern Segment of the Canyon. The rock-bordered canals characterizing the southern segment of Lefthand Canyon are relatively unusual features and do not have many analogs. Those that are known suggest influences from the north. Similar rock-bordered irrigation systems have been found in the Verde Valley (Fish and Fish 1984) dating to ca. A.D. 1200-1350, in the Upper Little Colorado region near Springerville, Arizona (Fred Plog, personal communication, 1978) dating to ca. A.D. 1000-1300, and in the Glen Canyon area of southeastern Utah (Lindsay 1961).
dating to ca. A.D. 1050-1250. Considering the forgoing in relation to Woodson's (1999) findings at the nearby Goat Hill site, it seems probable that the agricultural fields and canals in the southern segment of Lefthand Canyon represent a construction effort by migrant Anasazi peoples. This probability is reinforced by the recovery of only Maverick Mountain Black-on-red and Polychrome sherds among the canals and fields.

The Northern Segment of the Canyon. The canals and fields in the northern segment of Lefthand Canyon have a longer and different history from those to the south. There, the surface ceramics and associated habitation and activity sites suggest a local development, probably with Hohokam influences, beginning several centuries before the construction of the Goat Hill site. It is not known whether the residents of the northern segment influenced the agricultural constructions of the southern segment occupants when they occupied the canyon, or if certain aspects of the southern agricultural constructions (e.g., the rock-bordering of canals) were subsequently adopted by the northern occupants through influences from the southern group.

The Question of CCC Origins. One of my colleagues, who has worked in the Safford Valley for many years and knows the archaeology of that area extremely well, has doubts regarding the prehistoric origin of some of the Lefthand Canyon terraces, and more specifically the stone-lined or stone-outlined "splashpads." He contends that the splashpads and their associated terrace walls are, in fact, erosion prevention features constructed by the Civilian Conservation Corps (CCC) early in the 20th century. He supports this contention with the discovery of similar features constructed in proximity to the Sanchez CCC Camp, located above the north bank of the Gila River some 6.0 km to the northeast of Safford, Arizona. I too have found similar features associated with CCC constructions at site AZ. CC:1:69, the Taylor Canyon CCC Site.

In respect of my colleague's opinion, I investigated this possibility thoroughly during my fieldwork. I have found a number of lines of circumstantial evidence that point to their prehistoric origins. Allow me to briefly enumerate these lines of evidence.

First, these features are found on privately owned (patented) land, including the Spear Ranch. Interviews with elderly members of the Lamb and Smith families, past and present owners of the Spear Ranch since circa. 1880, revealed that they have no knowledge of the CCC having done any work on their property. Discussions with several local historians and life-long residents of the area (Mr. Lee Krider [Pima, Arizona - 86 years of age], Mrs. Mary Long [Pima, Arizona - about 75 years of age], and Mr. Frank Quinn [Safford, Arizona - 90+ years of age], indicate that their perception of the CCC mandate did not include modification of the landscape on private lands. Further investigation of this perceived CCC mandate is being made in correspondence with the CCC Museum at Jefferson Barracks near St. Louis, Missouri.

Second, the splash pads occur, sometimes in a rather scattered or inconsistent distribution, amidst the prehistoric fields and canal systems. The question then arises as to why would the CCC come into an area and modify portions of the existing prehistoric systems? In addition, S. Christopher Caran, a professional geologist specializing in Holocene geomorphology and hydrology, examined the field area just below and west of Goat Hill. His findings were that the ground surface of the canal and field area was stable, and that minimal erosion had taken place since the canal and terraced fields were abandoned. Among the indicators of this stability is the extremely fine preservation of the surface details of the canal and the fields. Again a question, why would the CCC construct erosion prevention features on a stable land surface?

Third, a possible explanation to this quandry was found in January of 2004, when Doolittle and Neely last visited the Safford Valley. Just north of
the Safford Airport and west of the Sanchez CCC Camp we found what we interpret to be CCC constructions overlapping prehistoric features at slightly different angles. Our interpretation of these overlappings are that the CCC was duplicating and enlarging the original prehistoric features and systems of water and soil management. Thus, the similarities of check dams, linear borders, and splash pads found at prehistoric sites and at CCC sites are by no means fortuitous. The recognition of the prehistoric features and systems as efficient erosion preventing technology by the CCC apparently resulted in their replication by the members of that government agency. Such an eventuality seems entirely possible in the light of the reuse of the prehistoric canals by the historic agriculturalists of the Safford Valley (Neely 2004:30).

**Socio-Political Complexity in Lefthand Canyon**

Following the doctrine proposed by Stark (1993), regarding the collection of empirical data as they apply to the development of models and theoretical frameworks, it is necessary to provide tangible evidence for the generation of models. The subsistence system, with its focus on agriculture and its associated technology, is an important variable in reconstructing the processes of cultural change documented by the archaeology of the Safford Valley. However, in spite of the importance of climatic conditions and the subsistence system, the socio-political system of the prehistoric inhabitants must be considered as primary in directing the processes of change as documented.

Recognizing the problems of neoevolutionary terminology (Yoffee 1993), the level of socio-political organization implied by the subsistence and settlement patterns, the nature and size of the sites and associated canals and fields, suggests an organization that would conform to that which Sahlins (1961, 1968, 1972), Schneider (1965), Service (1962), and others have labeled as “tribal.” This would entail relatively small egalitarian groups such as lineages or clans (e.g., Levy 1992:8; Schlegel 1992:381) having tenure to agricultural fields and infrastructure in two or more microenvironmental zones (e.g., the Pinaleño Mountain foothills and the Gila River floodplain).

A model based on the ethnohistoric O’odham has been presented for the prehistoric social organization of the Safford Valley area (Neely 1997b), and appears to be applicable in this case as well. In summary, this model proposes that even the most complex and extensive communities and public works of the Safford Valley were constructed and used by peoples organized at a “tribal” level. The model postulates social, political, and religious organization changes, as well as modification in agricultural technology, to accommodate and adapt to stresses occurring between ca. A.D. 750-1400 that resulted from climatic degradation, population increases, and possible intercommunity conflict. The model proposes the early presence of an essentially egalitarian society organized by kinship and employing “Chanayov’s rule” (Sahlins 1972:87), an optimizing strategy to “store” labor potential in reserve to be used in emergencies to insure the survival of the household. As stresses build though time, more complex kinship-based organizational integrating mechanisms and religious organizations are adopted. These mechanisms for organization and integration operated as a horizontal form of stratification similar to the “sequential” or “modular” hierarchy described by Johnson (1982, 1989). The development of a horizontal form of socio-political organization would not require major reformulations of the system existing since the Early Formative period. It is proposed that the O’odham-based model briefly summarized above potentially could account for the construction of all of the habitation sites, including the large Spear Ranch site, as well as all of the associated canals, gardens, and fields.
CONCLUSION

The excellent preservation in Lefthand Canyon has provided insights into the foothill water management and irrigation system as another form of agricultural maximization employed by the ancient inhabitants of the Safford Valley. Its extensive use in environmentally propitious locations along the northern face of the Pinaleño Mountains indicates that the foothill system played an important role as part of the adaptive process in confronting the problems presented by variations in available moisture and probable socio-political pressures resulting from an increasing population of local and ethnically diverse immigrant peoples (Neely 1997b, 2004; Neely and Doolittle 2004).

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INTRODUCTION

For nearly 300 years, the Palace of the Governors served as the civilian and military center of Spanish, Mexican, and American governments in New Mexico. Established at the same time, the Palace of the Governors and Santa Fe functioned as integrative places on the edge of a vast frontier where cultures met and were transformed. Historical studies of the Palace of the Governors and its buildings and grounds have relied on civilian, ecclesiastic, and military documents that focus more on people, possessions, transactions, rather than the places that people lived and kept possessions and where events and transactions transpired. Recent historical/anthropological research has provided many new insights into sociocultural and historical processes during the Spanish Colonial period in New Mexico and the American Southwest (Brooks 2002; Frank 2000; Gutierrez 2002; Knaut 1995; and Weber 1992 for example). In contrast, little new historical research has been generated on the architectural and land use patterns of the Palace of the Governors since the early 1970s (Shishkin 1972; C. T. Snow 1974).

More recently, historical research and archaeological investigation have focused on the Santa Fe Plaza, of which the Palace of the Governors is an integral part (Tigges 1990a). Reconstructions of the Spanish Colonial plaza and adjacent buildings and grounds have been derived from town or presidio building specifications, but largely remain untested formulations, because of the slow flow of new historical documents or maps and interpretive limitations of spatially and temporally disparate archaeological data. The precise layout and location of the Palace of the Governors and the Plaza is speculative for the early Spanish Colonial period (1609-1680) (Hordes 1990; Pratt 1990; C. T. Snow 1990; 1993; Tigges 1990a). Excavations within the main Palace building documented a highly flexible architectural pattern with room layout and dimensions and indoor and outdoor space changing often. While it appears that in the late 1700s the Palace building became more fixed, dramatic renovations in the 1909-1913 erased many of the traces of how and when this final transformation occurred (Snow 1974, 1993; Seifert 1979a, 1979b).

INTEGRATING ARCHAEOLOGY WITH HISTORY

In advance of the construction of a New Mexico Museum of History on the property north of the Palace of the Governors, the Office of Archaeological Studies and the Palace of the Governors collaborated on a sixteen month archaeological investigation (Figure 1). Excavation within a 450 sq m area exposed mixed and stratified cultural deposits that contained more than 700,000 artifacts spanning 900 years of occupation and encased 160 architectural features, facilities, and discrete deposits from the historic period after 1609. Among numerous research
goals the project aspired to evaluate documentary and cartographic evidence of changing architectural and land-use patterns during Spanish, Native American, Mexican, and American occupations from 1609 to 1912 (Post 2002).

Defining the limits of the Palace of the Governors and grounds is difficult, since they changed through the years and limits are not well-described in the historical documents. For this study, I use the Joseph de Urrutia "Villa of Santa Fe map" of 1766 depicted in the Moorhead presidio study (1975). Using Moorhead's definition of a toesa, which was equivalent to two m, the project area composes a 5 percent sample of the Palace grounds.

Figure 1.
Palace of the Governors (USGS 7.5' Santa Fe Quadrangle, 1977).
excluding the main Palace building. Located in the north-central portion of the grounds, the project area was expected to provide a limited, but important perspective on how all the grounds may have been organized during the Spanish Colonial period.1

While the excavation encountered archaeological deposits and features from 1609 to the 1930s, this paper focuses on the Spanish Colonial period from 1609 to 1821, including the twelve-year Pueblo Revolt or Great Rebellion of 1680. The dynamic architectural and spatial history of the Palace is for the first time viewed from systematic excavation of the Palace grounds, details of which are rarely provided in historical accounts and descriptions. The excavation results are compared with previous projects conducted within the main Palace of the Governors building and the historical and cartographic record for the various periods for which site structure data are available.

EARLY SPANISH COLONIAL PERIOD (1609-1680)

In 1609, Governor Pedro de Peralta moved the first Spanish Colonial settlement from San Juan de los Caballeros to a low plains surface setting bounded by a seasonal drainage system on the north, a perennial river and tributary stream on the south and a swamp or cienega on the east. Located near water, arable land, and distant from, but centrally located to, the Keres, Towa, Tano, and Tewa pueblos, he presumably chose unoccupied land on which to establish the Casas Reales and surrounding town following the town building 1573 Ordenanza de Descubrimiento (Twitchell 1925; Hordes 1990; C. T. Snow 1990; Pratt 1990). The ordinance prescribed a Casas Reales formed by a defensible quadrangle of military, civilian and religious buildings. Just how closely Governor Pedro de Peralta followed the ordinance is unknown. Surviving documents suggest some semblance of a grid pattern to the town layout (Hordes 1990). There are no descriptions of the Casas Reales that include the size and number of buildings, orientation of the plaza(s), or interaction between daily activities and the built spaces. Early Santa Fe historian Ralph E. Twitchell (1925) describes a Palace and grounds that were 122 m (400 ft) east to west and 245 m (800 ft) north to south with a wall enclosing a large patio to the north. More recently, scholars contend that a larger plaza extended from its current west limit of Lincoln Avenue to Cathedral Place, which fronts the current St. Francis Cathedral, lines up with modern day Prince Plaza on Palace Avenue (D. H. Snow 1990, 1991, 1998; C. T, Snow 1990, 1993). This larger plaza also extended south to modern Water St., where the entrance to the Casa Reales was in the southeast corner (C. T. Snow 1993). This alternate view suggests that a smaller courtyard or patio extended north from the governor's residence, rather than the large plaza suggested by Twitchell.

Limited excavations in properties within or near the Casas Reales regularly yield pits filled with artifacts from the pre-Revolt period (C. T. Snow 1991; D. H. Snow 2003; Wiseman 1988). Oxidized adobe in these deposits may remain from the buildings burned during the Pueblo Revolt and later demolished by post-Reconquest rebuilding of the civilian, military, and religious buildings and facilities. However, no pre-Revolt period architectural remains have been identified outside the Palace of the Governors. These excavation results do not support either Palace/plaza location hypothesis.

The 1974-1975 excavations below the floors of Rooms, 5, 7, and 8 at the Palace of the Governors by Cordelia T. Snow provide the strongest evidence for the current Palace occupying space that was part of the Casas Reales or adjacent residences.
Excavation uncovered a series of stratified surfaces and floors in association with cobble foundations. The lowest and earliest surface in the west end of the building reflects temporary or non-residential use between 1610 and 1640 based on associated pottery (Seifert 1979a). This extra-mural or informal use was replaced by well-made and ornate adobe floored buildings as inferred from cobble foundations capped by one or two courses of adobe bricks built between 1640 and 1680 (Seifert 1979a: 136; Snow 1974:15-16). Corner fireplaces further indicated that these were living spaces. Unfortunately, complete room dimensions are unknown and their relationship to other parts of the Casas Reales is unclear. A massive Spanish Colonial foundation divides Rooms 5 and 7. C. T. Snow interpreted the lower portion of this foundation as a seventeenth century foundation (1993:16). Other room excavations have exposed a similar foundation suggesting that it spanned a 76 to 91 m
length (250 to 300 feet) within the early Palace (Alexander 1965; Seifert 1979b: 210-219).

Our excavations documented a highly flexible use pattern early in the Spanish Colonial period. We uncovered two processing/storage pits, a metallurgical processing pit, and a series of overlapping refuse-filled borrow pits at the lowest and earliest Spanish Colonial level (Figure 3). All features were capped or filled with deposits bearing seventeenth-century pottery and domesticated animal bone. The processing/storage, borrow/refuse, and metallurgical processing pits were excavated into a natural alluvial deposit. The borrow/refuse pits clustered in the southeast corner of the excavation. The metallurgical pit was isolated from the other pit features along the southern edge of the excavation, and the two processing/storage pits were separated from the other features by 10 to 14 m. Use of near space for borrowing raw material for construction suggests that activities and associated space were not well-defined; a pattern that might coincide with early settlement and building construction. Samples of this natural stratum will be compared with materials from early architectural contexts to assess if exploitation of a suitable plaster or adobe source may have preceded use as a garden, corral, or workspace.

Just as major changes in space use are evident inside the Palace of the Governors between 1640 and 1680, our excavation exposed a substantial three-course, three row cobble foundation embedded in a seventeenth-century sheet midden deposit. This 12 m long east-west foundation lacked corners, appearing as a free-floating wall segment (Figure 3). The abrupt foundation termini suggest rapid and aggressive demolition. Currently, this foundation is the only Early Spanish Colonial structural component found north of the Palace and it may be contemporaneous with the residence found in Room 5. The only known historical document that might relate is a request in the 1661 residencia of Governor Bernardo Lopez de Mendizabal for payment of 40 pesos to an Indian man named Juan Chamiso who completed various construction projects. For one of the projects, he built a structure with four rooms and hallway adjacent to the Casas Reales (AGN Tierras 3268, ff207-08v translated in C. T. Snow 1993:21). Location of the Casas Reales garden is speculative. The 1766 Urrutia map shows gardens and orchards north of the Palace, which could reflect original land divisions prescribed by the 1573 Ordenanzas (C. T. Snow 1990:61-62). Pollen samples from contexts associated with the foundation may provide some evidence of an early garden. Currently, the foundation is interpreted as part of an enclosing wall.

PUEBLO REVOLT (1680-1692)

In August of 1680, the Pueblo Revolt forced the abandonment of New Mexico and resulted in the destruction of towns, haciendas, missions, and all written documents. Without going over well-covered historical (Kessell 1979; Hackett and Shelby 1942; Knaut 1995) and more recently archaeological ground (Preucel 2002) suffice to say that in Santa Fe, the churches and many residences were destroyed and the Casas Reales were modified into an apparent four-story walled pueblo with two plazas with kivas (Hackett and Shelby 1940; Kessell 1979). The location of the pueblo that housed more than 1,000 Tano speakers including Popé, one of the primary leaders of the rebellion, is a virtual mystery. We “know” that it was somewhere within the central core of downtown Santa Fe, which has remained relatively constant since 1609. The various small-scale and unsystematic excavations in the downtown core have yielded no supporting information. To the north and east of the current Palace of the Governors there was a swamp that covered 16 acres (Tigges 1990b:84)
Figure 4. 
Estimated location of the cienega during the 16th century (adapted from Tigges 1990).

(Figure 4). Swamp deposits have been encountered by archaeological excavations northeast of the project area (Post 1995; Schaafsma 1982) strongly suggesting that in the 1600s and 1700s this active wetland restricted northward urban expansion. Our excavation has documented an occupation level formed between 1650 and 1720. This surface is at an elevation of 0.50 to 0.70 m below the floor elevations for the Pueblo Revolt rooms unearthed in 1974. This elevation difference is topographic and geomorphological evidence that a narrow lomita separating the Santa Fe River and Arroyo Mascaras floodplain was chosen for the Casas Reales location. Therefore, landscape evidence also supports a hypothesis that the bulk of the Casas Reales and Revolt pueblo occupied drier and better drained land to the south, rather than to the north of the Palace of the Governors.

The 1974-1975 excavation uncovered four Pueblo-style rooms, a complex of large, overlapping storage pits filled with domestic and subsistence refuse that arguably had mixed Spanish and Pueblo origins, and the Native American skeletal remains of 4 children and 2 adults (C. T. Snow 1974; Seifert 1979a). The stratigraphic, architectural, and mortuary evidence strongly support a Pueblo Revolt occupation. However, the grandiose four-story buildings described by Governor Diego de Vargas cannot be extrapolated from these humble rooms. The large storage facilities and burials point to a plaza north of the rooms. With the Pueblo Revolt pueblo/remodeled
Casas Reales demolished and replaced in 1697, it is possible that primarily subsurface features survived. Why the four rooms within the Palace survived wholesale demolition has yet to be adequately addressed, let alone explained.

Isolating evidence of the twelve-year Revolt period within the excavation area was expected to be difficult. Extending the Pueblo Revolt pueblo north from the current Palace places the project area within roomblocks or a plaza (Arnold 1984). If the Pueblo Revolt pueblo was south and east, then the project area occupies extramural space (C. T. Snow 1990). Nine multipurpose storage, processing and refuse pits contain Pueblo-made pottery with manufacture dates spanning 1650 to 1720. Pit forms and evidence of intensive reuse compare favorably with the Pueblo Revolt pits found during the 1974-1975 excavation. The range of artifacts, faunal remains, and archaeobotanical remains was also similar, except when present, the archaeobotanical remains were charred. Activity areas were suggested by the pit distribution with five features clustered in the northeast, three clustered in the southwest, and two clustered in the northwest quadrants of the excavation area (Figure 3). Chronometric dates from archaeomagnetic, thermoluminescence, and dendrochronological samples may provide finer temporal resolution. Although dating for these pits is uncertain, it is clear they are important links to the time and events surrounding and, perhaps, including the Pueblo Revolt. What is clear from the excavation data is that after these pits are filled or capped, use of space changes dramatically in the eighteenth century.

MIDDLE AND LATE SPANISH COLONIAL PERIOD (1692-1821)

The eighteenth century was a time of rapid change at the Palace of the Governors. Historical documents indicate that the Palace of the Governors and military quarters were rebuilt or remodeled at least five times from 1697 to 1791 (Arnold 1984; Moorhead 1974, 1975; Shishkin 1972; C. T. Snow 1993). Descriptions are brief and usually lack specific architectural details (C. T. Snow 1974; 1993). Governor Pedro Rodríguez Cubero replaced Governor Diego de Vargas, who had been living in the Pueblo Revolt remodeled Palace, in 1697 and demolished and replaced it with either six high and six low rooms. Upon his return in 1703, De Vargas criticized the new buildings as not worth “one-tenth” the former Casas Reales (Spanish Archives of New Mexico Series [SANM] II:94a, New Mexico State Record Center and Archives, Santa Fe). Five years later, Governor José Chacón slated the Casas Reales for demolition, but there is no subsequent documentary evidence that demolition and reconstruction occurred.

In 1716 Governor Felix Martinez describes the poor condition of the Palace proper, but provides the first and only details about the grounds to the north. Only two rooms within the Palace were serviceable with nine buttresses supporting the building’s crumbling walls. Behind the Palace through a zaguan (covered passageway), Governor Martinez describes two courtyards, one enclosed by a two story coachhouse and the other by the quarters for the bodyguard. The precise location of the zaguan is not specified and neither are the dimensions of the enclosing buildings. Presumably the buildings were adobe following the conventional architectural style and construction methods (Moorhead 1975). From this description, we know that at one time the Palace had one or two enclosed patios bounded by one- and two-story adobe buildings (SANM II: 253).

From 1715 to 1760, the Palace of the Governors and grounds were renovated to some extent three more times (Shishkin 1972; Snow 1974; Hordes 1990). By 1766, the map of Santa Fe drawn by Sub-Lieutenant Joseph de Urrutia, a member of the Marques de Rubí military inspection of all presidios in New Spain, shows the Palace of the
Governors as a monolithic structure with a large enclosed courtyard (Figure 5). The 1716 buildings are not shown and must have been demolished. Even with the construction of the expanded presidio between 1789 and 1791, there is no cartographic evidence of change in project area (Figure 6). Two maps drawn by U.S. military engineers in 1846 (Figure 7) and 1847 (Figure 8) also depict roughly the same oversized courtyard. Descriptions from the 1760s until the arrival of the U.S. military in 1846 consistently criticize the condition, size, and appearance of the Palace of the Governors (Adams 1954: 46-47; Adams and Chavez 1956:40). During the Mexican period, there are brief mentions of barracks, kitchens and ovens, and barracks occupying the grounds to the north (Read 1927:92-93; Davis 1857:169-170).

This fragmentary historical record combined with a 1967 sketch map of cobble foundations capped by adobe bricks exposed in utility trenches was the context for examining eighteenth and nineteenth architectural and land use patterns. Based

![Image of map](image_url)

**Figure 5.** 1766 map of Santa Fe by Joseph Urrutia (original is at Museum, copy obtained from the Fray Agelico Chavez Library and Photo Archive in Santa Fe).

**Figure 6.** Plan of the Presidio of Santa Fe, 1791 (from Spanish Government in New Mexico by Marc Simmons, 1968).
on the descriptions and maps, building foundations, free-standing wall foundations, and evidence of cultivation were expected, but the location, nature, and condition of these physical traces remained unknown. Our excavation revealed a record that is compelling, enlightening, and indicative of a land use pattern that is more dynamic than suggested by the historical evidence. We have uncovered and documented cobble foundations from an extensive complex of buildings, one, less substantial, but unique circular structure foundation, an irrigation ditch, and an unmarked cemetery with other lesser architectural and maintenance and production features. From superpositioning and stratigraphy, I can offer an initial sequence of construction, use, and abandonment for the structures and features. Absolute dates are not available yet, but relative dates are inferred from the associated Pueblo-made ceramics (Frank and Harlow 1974; Warren 1977; McKenna and Miles 1991) and Mexican majolica (Goggin 1968; F. C. and R. H. Lister 1974).
The earliest 1700s features are the irrigation ditch and the six burials, which constitute an unmarked cemetery (Figure 9). Irrigation ditches or acequias shown on the 1766 Urrutia map are the major conveyances that distributed water throughout the city and surrounding area (D. H. Snow 1990). Laterals and other minor ditches are not shown. An east-to-west ditch runs along the north limit of the current Palace courtyard (Snow 1993:10), a north-south ditch was unearthed inside Room 8 within the main Palace building (Seifert 1979a: 195), and an east-west ditch was exposed along the northern Museum property limit (Post 1995).

Unlike, the known ditches, the east-west ditch uncovered by our excavation was lined with upright cobbles. Measuring 30 cm wide, 20 cm deep, it is not as deep or wide as the others suggesting that it was an internal gardening ditch rather than a supply ditch. Its presence conforms to the 1766 Urrutia map depiction that this space was garden or orchard, although the ditch or lateral is not shown on the map. Early to middle eighteenth century Pueblo-made pottery (polished red wares, micaceous utility wares, and Tewa Polychrome vessel fragments) partly filled the ditch indicating it was decommissioned before Urrutia’s map. A poured adobe layer that served as an apron in front of Structure 3 capped the ditch.

With the filling of the ditch, this area may have continued as a garden or orchard, but it also acquired status as a small-scale cemetery (Figure 9). Two partly intact skeletons in their original interments and skeletal elements of three other individuals recovered from redeposited context indicate that up to five burials occupied this space prior to the construction of Structure 3. The two partly intact Native American burials, an adult female and an 8 to 10 year old child were lying on their backs with crossed legs and arms and heads facing west conforming to Catholic practice. These individuals were placed in a parallel alignment only 1.3 m apart. The child was buried in a
shallow grave above the ditch allowing for the inference that the cemetery post-dated the irrigation feature. A sixth burial was interred along and below the southernmost foundation that is beneath the Palace Patio office building (Structure 5). This adult Native American female was also lying on her back with arms and legs crossed, but her head was oriented to the east. Context indicates this interment pre-dates the other burials, which along with the 50 individuals recovered from a mass grave within the current Palace courtyard in 1884 indicate an intermittent, long term, and spatially extensive pattern of Spanish Colonial cemetery use during the late 1600s and 1700s (Shishkin 1972:42; C. T. Snow 1993:7). Burial contexts within the Palace proper are associated with the Pueblo Revolt occupation (Alexander 1965; C. T. Snow 1974; Seifert 1979a). Currently, there is no documentary evidence describing the practice of burying either christianized or unbaptized Native Americans within the Palace grounds or within civilian or military spaces during the Spanish Colonial period.

After 1766, there are rapid and dramatic changes to the project area. Cobble foundations from as many as five, abutting buildings stretched at least 41 m from the east limit of our excavation to Lincoln Avenue (Figure 9). The five buildings are represented by three east-west foundations, one of which was under the current north wall of the Palace Patio office building and the other two at 6 m intervals to the north. One postulated building may be an enclosed outdoor space (Structure 2). The other four buildings covered 490 sq m (5,314 sq ft). The foundation widths range from 0.70 to 1.3 m and 0.40 to 0.70 m deep. They were laid in shallow trenches with the majority of the foundation serving as a raised footing. Cobbles in the largest foundation weighed up to 80 kilograms. Internal foundations tended to be less substantial with the 1.3 m wide foundation exceeding the specification for a two-story building (SANM II, Roll 11, Frames 199-202). Two small foundation segments were capped with adobes ranging in size 45 to 60 cm long and 30 to 40 cm wide with 3 to 6 cm wide mortar joints. Each building was about 5 m wide with room widths only available from Structure 1 ranging from 6.5 to 7.8 m wide. The 5 m width is well within the range specified for presidio barracks, while the 6.5 to 7.8 m lengths are slightly more generous than the 6 m specification (Moorhead 1975:167).

All major foundations corners abutted suggesting that the long east-west 41 m plus foundations were laid first and then the room divisions inserted. Multiple floors within the structures combined with other remodeling evidence indicate that the building functions changed during their 75 to 100 year lifespan. Structure 1, Room 3 and Structure 4, Room 1 had corner fireplace bases in their southeast and northeast corners respectively, suggesting they were living or heated workspaces. Structures 3 and 5 lacked intramural room partitions suggesting that these were storage spaces or stables. Based on very similar foundation construction and the consistent pattern of north-south interior foundations abutting east-west foundations, Structures 1, 3, 4, and 5 were built at the same time.

Structure 3 provided the most information on architectural change because its outline was fully defined by excavation. The sequence of alterations to the north foundation may reflect changing requirements for access to and control of the interior space and contents. Initially, the north wall of Structure 3 had a 12 m wide opening with two, single-course cobble pilaster foundations attached to the east and west termini. No intervening posts or supports were identified and associated with the initial construction. This lack of a secure or substantial doorway could reflect its initial use for storage of low value or non-portable supplies and equipment that required limited protection from the elements and were unlikely to escape or be taken from inside the building. The building use apparently changed when two free-
standing single-course cobble pilaster bases were inserted into the twelve m gap dividing the stretch into 3 to 4 m spaces that could be spanned by three large doors or gates. Partitioning of the opening suggests the need for more security, confinement, or protection for the buildings contents or occupants (animal or human). Finally, the north side of Structure 3 was fully enclosed with the north gate pilasters integrated into a 0.90 cm wide foundation that was less substantial than the existing building foundations. At the same time, Structure 3 was divided into two 2 m wide stalls with the southern stall built floor reinforce by a three cobble deep sub-floor that created a dense, impenetrable base. The lack of abundant organic debris within Structure 3 argues against its use for livestock, unless it was cleaned before demolition. The full enclosure of Structure 3 and the narrow width of the rooms suggest a change to storage of small, portable items, supplies, or animals that required security and a more controllable interior environment.

Clearly, Structure 3 defies generic or simple classification, as does the use of this space throughout the Middle and Late Spanish Colonial period. Structural and internal changes reflect changing spatial needs as formerly open garden or cemetery space was converted to enclosed architectural space. However, as activities within the space changed, the boundaries of the space appeared to remain constant. The irrigation ditch defined the initial northern boundary of this multipurpose space. Once the ditch was covered, the north wall or limit of Structure 3 was placed on top of it. As Structure 3 changed, the north limit of the building remained fixed. The irrigation ditch and building wall divided the space into to distinct areas. The area north of Structure 3 yielded no permanent architecture, graves, irrigation features or formally enclosed spaces. Instead, it housed larger thermal features, a circular, partly subterranean and enclosed impermanent structure or facility with associated butchering debris and artifacts, and a large deep pit filled with piñon and juniper charcoal. These various features were capped by dense, but mixed layers of domestic and subsistence refuse dating from the eighteenth and nineteenth centuries. These distinct spatial and structural, and assumed functional differences will be an important focus of future study and analysis.

By 1846, the Palace and surrounding buildings suffered from serious neglect. Territorial governors complained about the safety, sanitation, and comfort allowed by their new residence and office. Small-scale renovations were conducted. The space to the north is briefly referred as gardens and kitchens, but little detail is provided. The collection of maps showing the Palace and grounds show ruins and open-spaces, but no buildings that can be securely correlated to the ones found during the excavation. Between 1867 and 1869 large-scale renovations at the Palace shortened it at both ends and removed and replaced dilapidated buildings that surrounded the courtyard (Shishkin 1972). Until 2002, the buildings and space north of the Palace lapsed into historical and archaeological obscurity.

CONCLUSIONS

Results of the 2002-2004 excavations behind the Palace of the Governors have far outstripped the initial expectations. The documents provide almost no indication of the range of architecture or activities. Maps show a place frozen in time as open-space. The archaeological evidence of features, architecture, and their associations reflect a dynamic, flexible, and planned use of the Palace grounds. This dynamic record spans more than 300 years of Palace history with the Spanish Colonial period well represented.

The early seventeenth century historical record details the establishment, operations, and competition between secular and ecclesiastic institutions. In contrast, the archaeological record reflects existing environmental setting at the initial settlement, subsequent land use and modification, and sustaining activities. The land use pattern and sustaining activities start small-scale and
become more formalized and substantial leading up to the Pueblo Revolt in 1680.

Conditions leading up to the Pueblo Revolt, the structural changes associated with the eradication of all Spanish things at and during the years of the Pueblo Revolt, and the rapid and radical changes that succeeded the Reconquest are not well represented in the excavation record. The array of pits and facilities uncovered by excavation need more reliable dating and detailed analyses. The large pit facilities rapidly filled with subsistence and domestic debris, either by Revolt or Reconquest participants are similar to the those found within the Palace suggesting that an extensive activity area or plaza extended to the north. The lack of foundations or other architecture in the excavation area is strong evidence that the Pueblo Revolt pueblo was located to the south or east. How the four small rooms within the Palace escaped the wholesale demolition of 1697 is a mystery.

Much of the Middle and Late Spanish Colonial period archaeological record inside the Palace of the Governors has been virtually destroyed by radical renovations. The Palace grounds have fared better, although surrounding spaces have been subject to urbanization and museum-building. In the 1700s, irrigated gardens shared space with an informal cemetery. These important open-space features were supplanted by substantial, but flexible architecture with processing and open-space activities pushed further north through time. The need for larger-scale open-space was replaced by a focus on secure, enclosed space for living, working, and storage.

As this project proceeds, analysis and interpretation will focus on integrating archaeology and history into a more holistic view of the Palace of the Governors. Moving between historical description, cartographic scale and archaeologically defined buildings, features, and deposits, will lead to richer interpretations as we seek to understand the Palace in the context of its multivocal pasts.

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ENDNOTES

1 Some scholars have suggested 3.19 acres for the Palace grounds based on lot sizes specified in the 1573 Ordenanzas (C. T. Snow 1990:60). Using the 1766 Urrutia map at the two meter per toesa scale suggested by Max Moorhead (1975:148) a 2.67 acre area is calculated. Some scholars prefer a 1.68 meter per toesa scale for the Urrutia map, which would yield a 1.91 acre area (D. H. Snow 1990:102; Pratt 1990:44). Using a 350 ft estimate for the length of the Palace in the 1850s (Arnold 1989:140), a 3.09 acre area can be estimated suggesting that Arnold employed a 2.1 m per toesa scale. When these different measures are applied to the middle to late Spanish Colonial Palace building, a length ranging from 275 to 350 ft is obtained. Suffice to say that this situation does not get clearer after 1846. This fact can be illustrated by an official description of the Palace in 1869 describing it as 247 ft long following removal of various dilapidated rooms from the east and west ends. This shortening of the Palace brought it to its current length. The 2000 survey plat for the Palace shows it as 270 ft long. Where
did additional 23 ft come from? The point of this lengthy and not necessarily complete summary of building dimensions and area measures is that at different points in time, scholars are not only unsure of the layout and composition of the Palace grounds, but they cannot even be sure of the main buildings size or the extent of the associated grounds. So any area calculations that I suggest are open to addition interpretation.

2 How and if these buildings relate to the 1766 Urrutia map is unclear. One building is shown north of the northwest corner of the Palace on the 1766 Urrutia map. Its southeast corner is joined by a free-standing wall that separated the courtyard from the gardens and orchards. Temporal and functional association between the irrigation ditch and the gardens and orchards is probable. However, the preliminary excavation results do not indicate if one the five building foundations are from the building depicted on the map. While not impossible, it is unlikely that Urrutia would omit a large complex of outbuildings within the military grounds. Therefore, the current assumption is that the buildings post-date 1766. Having stated this, it should be said that the subsequent Spanish and American military maps fail to show buildings in this location as well. Reasons for the omission are not known from the documents, except that the Palace and its associated buildings were often described as in poor repair or ruins. It is possible that these buildings were in such poor shape that they were not considered important, if they could not house livestock or people. Regardless of chronological considerations, these buildings indicate that there were significant changes to the Palace grounds during the middle and late 1700s.

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INTRODUCTION

Historian France V. Scholes believed that life in New Mexico after the arrival of Spaniards in 1598 was one of unrelenting hardship, without “any considerable degree of social refinement.” Yet, if life in Colonial New Mexico was as grim as Scholes and others would have us believe, how do archaeologists and historians explain the presence of majolica, Chinese porcelains and other “exotic” wares recovered from colonial sites? Further, what does the presence of those items in archaeological sites tell us about the individuals who once owned and used such ceramics?

For nearly a century archaeologists who worked in seventeenth through early-nineteenth century mission and domestic sites in New Mexico have recovered fragments of Mexican and Spanish majolica, and Chinese porcelains from their excavations. Initially viewed as curiosities by early archaeologists, those sherds, and the rare, but occasional restorable vessels they also recovered, were routinely dismissed as having little bearing on understanding or elucidating life in colonial New Mexico other than to suggest that such items belonged only to the wealthy, or elite, of the colony. Subsequently, archaeologists realized that the fragments of majolica and porcelain were more than curiosities and could be used to place the sites from which they were recovered in chronological order. As a result, the presence of majolica became a tool that could be used to aid the archaeologist in placing sites in time, but there it has remained.

While it is a truism that “sherds cannot be made to talk,” the very presence of certain artifacts such as majolica and Chinese porcelains in archaeological sites must indicate something more than the fact that the former occupants had owned specific types of ceramic vessels that had been transported long distances to the places they were ultimately used and broken. In other words, what does the presence of majolica and Chinese porcelains on a site or sites mean? Aside from the fact that the presence of majolica and porcelain sherds can be interpreted as identifying a site as having been occupied by Spaniards, those of Spanish descent, or those who had acquired or adopted Spanish tastes, the same sherds also can be interpreted as
coming from complete vessels used by individuals who exemplified a realization and knowledge of social order and polite behavior. In short, vessels of Mexican and Spanish majolica and Chinese porcelains were a means by which the colonists maintained their ties to polite society, the homes and manners of family—the culture of which they were a part.

In other colonial situations such as those along the Eastern Seaboard of the United States, we may derive knowledge of certain sets of behavior from the books of deportment that were published in contemporary European societies during the seventeenth through early nineteenth centuries, contemporary portraits and paintings, and diaries. But we have no books of deportment for colonial New Mexico; neither do we have extant contemporary portraits until the mid-nineteenth century. On the other hand, in Mexico and Spain, we have numerous portraits of individuals, and also a series of paintings that depicted the caste system and that were also used as devices to illustrate behavior and possessions inherent to members of individual castes (Sáiz 1988). We also possess a number of colonial wills and inventories that include mention of majolica and Chinese porcelain, in addition to a legacy of dichos, proverbs or folk sayings, and frequently cuentos and corridos, songs, accumulated by descendants of the early colonists of the province as reminders of correct and preferred behavior (Weigle and White 1989). Finally, we will look at the contents of several colonial wills and inventories to see if we can determine how majolica and Chinese porcelain may have been used in New Mexico. Far from being curiosities or chronological markers, sherds of majolica and Chinese porcelains are not only the tangible evidence (the objects) of the possessions used by the colonists, but also tangible evidence of the knowledge and familiarity with mannerly behavior and polite society in New Mexico.

**MAJOLICA IN NEW MEXICO**

Majolica (also mayólica, maiolica) is synonymous with faience and delftware as all are soft-paste earthenwares with lead-tin glazes. Majolica, known in Spanish-speaking countries as loza, loza fina, loza blanca and since the seventeenth century, talavera, after the town of Talavera de la Reina in Spain, was first brought to the New World by Columbus, and began to be manufactured in Mexico City as early as 1540 (Gámez Martínez 2003:231; Gavin 2003:1-2). Vessels of majolica arrived in New Mexico with the privately funded expedition of Juan de Oñate and continued to be transported to the province until the mid-nineteenth century. Majolica has been recovered from both mission and domestic sites from Taos south to El Paso del Norte, and from the former Hopi missions and Awatovi on the west, east beyond Pecos and the Saline Pueblos (ARMS files, Laboratory of Anthropology, Museum of New Mexico, Santa Fe; Plowden 1958; D. H. Snow 1965). Although majolica, Chinese porcelain and similar exotic sherds rarely account for more than two or three percent of the total number of ceramics recovered on a given site, the sherds are ubiquitous on colonial sites.

Majolica was considered to be so important in the daily lives of the friars during the seventeenth century, prior to the Pueblo Revolt of 1680, that beginning fifty years earlier in 1630, each friar-priest at each mission was to receive “one box of loza de Puebla” as part of the goods intended “for the infirmary” every three years (Scholes 1930:101). Attempts to determine how many vessels might be included in a box of majolica have proved enlightening to say the least. To begin, common sense tells us that it is considerably easier to transport majolica and Chinese porcelains, indeed any easily broken ceramics, in a box. However, since we have no data available to estimate the number of vessels in a box of majolica transported over land, I used figures provided
for sea transport with some modification. According to (Gavin 1993:6-7) shipping records for the “1592 and 1593 flotas [from Spain] to Cuba, Santo Domingo, Honduras, and Mexico” included:

11 large 100-pound marked boxes of 102 dozen pieces of blue mayólica made in Sevilla; 15 dozen pieces of white mayólica made in Sevilla, all plates and porringers; 4 100-pound marked boxes that carry 36 dozen pieces of blue mayólica [also] made in Sevilla; 3 boxes with 150 dozen pieces of blue mayólica.

Based on the foregoing, I assumed that the average box of majolica sent to New Mexico weighed no more than 100 pounds and contained perhaps nine dozen vessels. Since the contract for the mission supply service to New Mexico provided delivery on the average of once every three years, let us say that sixteen deliveries were made to the individual missions during the period between 1630 and 1680. Further, let us say that at any given time during this period there were thirty active missions in New Mexico, which means that approximately 480 boxes of loza de Puebla, or approximately 51,840 individual majolica vessels, were transported to the province prior to the Pueblo Revolt! Because more than 51,000 pieces of majolica appears to be excessive, I divided the contents of a box between missions based on a conversation with Patricia Fournier some years ago (personal communication 1992). During that conversation Fournier suggested that the average shipment to each mission transported to New Mexico may have contained no more than three-dozen pieces of majolica. If that were the case then the shipment to three missions could be combined and transported in a 100-pound box, in which case approximately 17,280 majolica vessels were transported to the colony before August of 1680. Even if each box held only four to six vessels of majolica and weighed considerably less than 100 pounds, we are still talking about a total of nearly two thousand to nearly three thousand vessels of loza de Puebla that were transported to New Mexico during the fifty-year period. Although the mission supply service ostensibly provided majolica for use in the mission infirmaries alone, Fournier (personal communication 1992), also suggested, and I agree with her, that in all likelihood once the friars at any given mission had replaced any vessels that had been broken since the previous delivery, those same friars turned around and sold any surplus vessels to the colonists as a means for raising funds for their mission endeavors. Speculation aside, even though each mission received a box of loza every three years between 1630 and 1680, exactly how many majolica vessels were actually present at the missions or adjacent pueblos remains a matter of conjecture, as does the number of vessels procured and used by the colonists.

A. V. Kidder (1932), Nels Nelson (1914), Frederick Webb Hodge (1937: Smith, Woodbury and Woodbury 1966) and Jesse Nusbaum were among the early archaeologists who worked at historic Spanish mission and sites in New Mexico during the first quarter of the twentieth century. Somewhat myopic in their views, the fact of the matter is that neither Nelson nor Kidder, in particular, were interested in majolica and missions so much as they were interested in working from the known to the unknown in studies of Puebloan speakers, stratigraphy, and seriation, or developing a means by which indigenous ceramics could be used to date the pueblos in which the missions had been constructed (Kidder 1932: 2, 5, 11; Nelson 1914: 9). Although Kidder and Nelson were aware of the historical backgrounds of the missions associated with the sites in which they worked, the mission churches and associated conventos were not germane to their larger studies. As a result, while Kidder and Nelson recovered and saved no more than several dozen sherds of loza and Chinese porcelain from their investigations, those sherds and the rare entire or largely restorable vessels that were recovered were con-
sidered nothing more than curiosities, and were usually identified in simple terms as "china," "crockery," or on occasion, "European porcelain" (Kidder 1932: 308; Nelson 1914).

Another early archaeologist, Jesse Nusbaum, worked at the Palace of the Governors between 1909 and 1911, and then in 1915 in the mission and convento complex at Pecos in order to restore those sites and structures for another reason entirely, tourism. Whether Nusbaum identified any majolica or Chinese porcelains from his restoration at the Palace of the Governors between 1909 and 1911 and Pecos Mission in 1915 is unknown. Knowing from personal experience that excavations at the Palace of the Governors in Santa Fe since the early 1970s have produced hundreds of sherds of both majolica and porcelain leads me to believe that any such artifacts that Nusbaum recovered from that site were discarded or, more than likely, never collected. In any case, no majolica or porcelain identified as having come from those excavations is known in either the collections at the Palace or in the Laboratory of Anthropology. Given the fact that Nusbaum is known to have recovered the exotic, life-size ceramic head of a cat with a mouse in its mouth from his work in the convento at Pecos, and now in the collections of the Laboratory of Anthropology, one cannot help but wonder what he missed there. Since none of those archaeologists screened the fill from their excavations—the fill was normally hauled out of the way to another location by the wheelbarrow load, or as in the case of early work at the Palace of the Governors, by the cartload—we have no idea of the number of sherds that were not collected or the number of vessels those sherds may have represented. Finally, and perhaps most important of all, as Karyn deDufour (2003) has shown in her recently completed thesis, no archaeologist has ever systematically excavated a mission midden in New Mexico.

Frederick Webb Hodge, on the other hand, not only excavated virtually the entire mission complex at Hawikuh but also was clearly interested in and knowledgeable about the history of the site (Hodge 1937). However, due to circumstances Hodge was unable to integrate the material analysis of Spanish goods with his historical research. Some years after Hodge's death in 1957, Watson Smith and Richard and Nathalie Woodbury (1966:xv) noted that:

Still to be done are studies of the... Spanish materials. The latter is a major effort in itself, and of such unique character that it has been thought better left to a publication in its own right. It is hoped that some scholar experienced in the XVI century Spanish material culture may find this a challenge.

To the best of my knowledge, the Spanish materials have yet to be analyzed.

Beginning with the second quarter of the twentieth century attitudes began to change about Spanish and Mexican majolica and Chinese porcelain. Between 1935 and 1939 J. O. Brew and others excavated much of the former mission site of Awatovi during which 104 fragments of majolica and Chinese porcelain were recovered (Montgomery et al. 1949: 94-95). Although Arthur Woodward of the Los Angeles County Museum of History, Science, and Art analyzed the materials, the authors decided:

Since specific identification of these wares is based largely on color and texture, since most of the specimens found are so tiny, and since they have all been classified as types well known to students of colonial Mexican ceramics, it has been decided that collotype illustrations of them would serve no useful purpose. Nor does the expense of a color plate seem justified (Montgomery et al. 1949: 95; emphasis added).
As a result, small sherd size notwithstanding, we have no knowledge of any vessel forms or design types that had been recovered during the excavation.

Majolica as an Aid to Seriation

While a student in the 1930s, John M. Goggin (1968) began to look at majolica as something other than a curiosity. Based on work he had done in the Southeastern part of the United States, and using both European and New World paintings in which majolica was depicted and through the study of historical documents, Goggin realized that it was possible to identify different types of majolica that could be used to date the sites from which it had been recovered. In order to test his hypotheses and study as many collections as possible, Goggin traveled to excavations throughout the Southeast, the Southwest, the West Indies and parts of Latin America beginning in 1949. While in New Mexico, Goggin met E. Boyd, Florence Ellis and others and borrowed materials from collections here to illustrate his seminal book, Spanish Majolica in the New World (1968).

Shortly after Goggin’s visit to New Mexico, William Plowden, a student of E. Boyd’s published the first chronology of majolica in New Mexico (Plowden 1958). An article written by David H. Snow, also a student of E. Boyd’s, followed Plowden’s paper. Snow enlarged upon the types Plowden had recognized, and refined the chronological order of majolica recovered from New Mexican sites (Snow 1965). Others who followed Snow and Plowden included Rex Gerald (1968) of the El Paso Museum, and Florence and Robert Lister (1969; 1982; 1987) with their unparalleled publications on the production and history of majolica in both the Old and New Worlds. With the publication of those papers in El Palacio, Historical Archaeology, and elsewhere, the occurrence of majolica in a Spanish Colonial site in New Mexico was raised from that of a curiosity to an important temporal marker, a means by which it was used to date the sites from which it had been recovered. Subsequently historic archaeologists working in New Mexico and elsewhere have used the presence or absence of specific types of majolica, more or less successfully, to chronologically date the sites in which it has been found.

Because of their utility as chronological markers, sherds of majolica have been avidly collected since the 1960s, and, as a result, have been recovered from seventeenth through early nineteenth century colonial sites throughout New Mexico. Archaeological sites from which loza has been recovered include not only purely Spanish sites but also Genizaro sites such as Abiquiu, Santa Rosa de Lima, Las Huertas and others. In other words, loza or majolica, while not the most common ceramics recovered from seventeenth through early nineteenth century sites, is ubiquitous at Colonial sites throughout New Mexico.

Manners and Polite Behavior

Although archaeologists may view the sherds of the thousands of majolica tiles and vessels that were laboriously transported tremendous distances from their place of manufacture, over caminos reales or by sea, as either curiosities or as a means by which to date the sites from which they have been found, most assuredly the individuals who originally owned and used those vessels or tiles did not view them in the same way. What did the possession of even one or two majolica vessels represent to single individuals, or boxes of loza de Puebla mean to the brothers in a far-off mission? What types of behavior required the use of majolica instead of indigenous ceramics; in other words, what was the meaning of majolica?

As Lorinda Goodwin (1999:4) states in her delightful book, An Archaeology of Manners, “No
one has ever excavated a curtsy. No field archaeologist has ever recovered a wine health toast, a proposal of marriage, or a gracious nod of recognition in church." While potsherds cannot be made to speak that does not mean that the archaeologist cannot interpret behavior from the potsherds and artifacts recovered from his excavations. Behavior, particularly polite behavior, the ability to get along with one's family, friends and the world at large, is part of the cultural baggage carried around by everyone—this was as true 2,000 years ago as it is today. Part and parcel of that cultural baggage carried to new settlements are the personal possessions—clothing and household goods—one's belongings, necessary to enable one to carry out polite behavior in the most comfortable manner possible.

Since historical archaeology, unlike prehistorical archaeology deals with written records along with excavated material culture, it has the whole of culture as its purview. The study of mannerly behavior then is simply a facet of the larger picture (Goodwin 1999:6).

Goodwin wrote Archaeology of Manners using as her subject several families who belonged to the merchant elite of Colonial Massachusetts where she was fortunate to have diaries, collections of letters and portraits of her subjects. Goodwin (1999:13) believes that, “the heart of mannerly behavior is communication: manners are best understood as an opportunity for agency and action...manners allowed the expression of individuality while preserving the means of communication between individuals and groups.” Goodwin also notes (1999:13), “Manners are reflective of more than superficially polite encounters” where rules are “flexibly applied and subject to cultural interpretation.” Unfortunately, in New Mexico we do not have those same primary resources available to us where in cases the individuals whose dictated wills and inventories survive were, in some cases, functionally illiterate. Equally important, only in exceptional cases in New Mexico is it possible to positively identify Spanish Colonial domestic sites such as LA 20000 with specific individuals, unlike Goodwin's identification of sites and the individuals who had occupied them in Massachusetts. Where Goodwin dealt with the mannerly behavior of the Turner family who had occupied the site of the House of the Seven Gables that inspired Nathaniel Hawthorne (Goodwin 1999:80), in New Mexico we must depend upon the objects or artifacts recovered from such sites as LA 20000 and 4955 to illustrate mannerly behavior.

In view of the lack of positive correlation of sites with individuals in New Mexico and lack of such primary documents as diaries, it has been necessary to look elsewhere for examples of mannerly behavior and so the question becomes: what if any artifacts recovered from colonial sites represent mannerly behavior? Aside from the frequent mention of tools, wearing apparel and the like, two of the items most commonly mentioned in contemporary wills and inventories were chocolate and sugar. Equally important then as now, chocolate, sweetened with sugar, was more than likely served in cups or small bowls on specific occasions (Figure 1).

CHOCOLATE

One of the best, if not the very best, of the New World's gifts to the Old, chocolate, the seeds of the cacao tree, *Theobroma cacao*, was almost universally disliked by the Spanish conquerors of Mexico (Coe and Coe 1996; Dalby 2000; Foster and Cordell 1992; Schivelbusch 1992). As late as 1575, Girolamo Benzoni, wrote in his *History of the New World*, that chocolate, "seemed more a drink for pigs, than a drink for humanity" but then went on to say, "the taste is somewhat bitter [however], it satisfies and refreshes the body, but does not inebriate" (Coe and Coe 1996:108-109; Pierce 2003:248). More recently, Wolfgang Schivelbusch
(1992: 85-87) described coffee as the fashionable drink of the Protestant ethic, England, Holland, and France, while he placed chocolate together with Catholicism, southern Europe, Spain, and Italy. Although slow to catch on, chocolate became the drink of choice in Mexico by the second half of the sixteenth century. Because it was believed that one could consume liquids and not break religious fasts, initially chocolate became a clerical fasting drink, or the “drink of priests” (Schivelbusch 1992: 91). During the Inquisition trials of New Mexico in the 1660s, Governor López de Mendizábal and his wife, Doña Theresa, reported drinking chocolate in the Palace of Governors during Holy Week (Pierce 2003:250). Consumption of chocolate during Mass was later adopted by well to do parishioners, particularly women, throughout the Catholic world.

The custom of drinking chocolate may have arrived in New Mexico with Juan de Oñate if the sherds of Chinese porcelain and both Spanish and Mexican majolica recovered from test excavations at San Gabriel are any indication. However, it should be pointed out that chocolate is not mentioned in any of the inventories for either the original Oñate entrada or for the reinforcements that arrived in 1601. Neither is chocolate listed in subsequent mission supply inventories. On the other hand, by mid-century we have documentary evidence that chocolate was consumed at least on occasion and possibly with some frequency. In one instance, Fray Alonso de Posada reported from the convento at Pecos in September of 1663 that he offered chocolate to then Governor Diego de Peñalosa who had ridden to Pecos to arrest the Franciscan (Kessell 1979:199-200).

Judging from both available wills and inventories and the archaeological record, the consumption of chocolate became even more popular during the eighteenth and early nineteenth centuries. The
1704 will of Diego de Vargas provides us with examples of the importance of chocolate. In his will Vargas asks that chocolate in his personal possession be used to pay the friar who officiated at his funeral (Kessell et al. 2002:231). He specified that the chocolate, “from my personal supply will come to about 9 arrobas [225 lbs.] in two big baskets.” Elsewhere in the inventory of Vargas’s stores in the Palace of the Governors, were a “case of ordinary chocolate and 2 cases of sugar” in addition to “39 copper chocolate pots” and 8 dozen chocolate beaters” (Kessell et al. 2002:236, 242), all ostensibly for resale.

Other wills and inventories also mention both loza and loza de Puebla and Chinese porcelains. One will in particular, that of Juana Lujan in 1762, mentions chocolate, sugar, “a chocolate pot and a mixing jug for chocolate, a chocolate cup from Michoacán, and three cups for chocolate from China.” Also in Juana’s collection of goods were two cups and two saucers from China, a saltcellar from Puebla, six cups from Puebla, three warming cups from China, three plates from Puebla and one of porcelain (Ahlborn: 1990:341-355). Finally, the inventory of the estate of Lugarda Quintana included a broken pitcher, perhaps a chocolate pot, from “Alcorzar.” Almost certainly the pitcher had been manufactured in the royal ceramics factory in Alcora, Spain and from there transported to New Mexico (Pierce and Snow 1999:79).

OBJECTS SUPPORTING IDEAS DISCUSSION AND CONCLUSIONS

From the foregoing we have seen that as many as several thousand entire pieces of loza were transported to New Mexico during the seventeenth century alone. Quite obviously, those who owned and used the entire pieces did not view them as either curiosities or the means by which to date the sites in which they were living. Those interpretations are those of the archaeologists who worked on the sites whether they were missions or domestic sites.

Because France Scholes dealt only with historical documents and because he knew that New Mexico was a frontier settlement of the Spanish empire, Scholes believed that Spaniards living in a colonial situation were uniformly rude, uncouth and without “any considerable degree of social refinement.” However, Scholes neglected to take into account the cultural background of those early Spanish settlers in New Mexico. While the majority of the settlers were descended from second and third generation Mexican conquerors, others arrived in New Mexico directly from Spain. Both groups considered themselves “Spanish” whether they were in fact, Españoles, peninsulares, criollos or members of other castas. With settlement of New Mexico beginning with Juan de Oñate, the majolica transported over the Camino Real became a symbol of the cultural identity, the “Spanishness” of those who settled here.

Loza has been recovered from sites in New Mexico that range the gamut from mission to domestic occupations from seventeenth through nineteenth century in date. The majolica brought to New Mexico was augmented by transportation of Chinese porcelains, chocolate, sugar and styles of clothing that further emphasized that identity as opposed to the indigenous peoples of New Mexico. In other words it appears that majolica was not so much as luxury as a necessity to many of the colonists. But necessity does provide an explanation for behavior.

As we have seen during the seventeenth century Fray Alonso Posada offered chocolate to Governor Penalosa who was intent on arresting the friar. While we don’t know for a fact that Posada served the governor his chocolate in either a majolica or Chinese porcelain cup, chances are that it was served in one or the other type of vessel. Quite obviously Juana Lujan also served
chocolate in vessels of either loza de Puebla or porcelain. Again, we can only assume the chocolate was being served as a gesture of hospitality. To whom, when and why that hospitality was extended is unknown.

In a manner of speaking, ownership of loza de Pueblo, Chinese porcelains, fine clothing, and expensive foodstuffs represented means by which the colonist could “thumb his nose,” albeit politely, at the vicissitudes of frontier life and means by which to reaffirm cultural ties to Spain. Possession of such goods on the frontier of New Mexico also provided a means by which the owners could exhibit to others, be they family, friend or foe, their knowledge of and familiarity with what constituted proper behavior in Mexico and Spain. Thus when the colonists fled to El Paso del Norte in 1680 after the Pueblo Revolt, many complained of their nakedness, both literally and figuratively. The colonists had lost not only their lands but, equally important, all of their possessions, so their complaints of nakedness reflected the loss of their identity as Spaniards. The objects and life-style that had marked them as Spanish had been destroyed. Ironically, that this same loss of identity had occurred to the pueblos with the missionization of New Mexico beginning in 1598 was beyond the comprehension of those who fled the Pueblo uprising in mid-August of 1680.

After the Pueblo Revolt, when Diego de Vargas reclaimed New Mexico for Spain, those who had accompanied Vargas and those who came after attempted to recreate their former lives here. Once again as in the seventeenth century, majolica, Chinese porcelains, glassware and articles of fine wearing apparel were transported over the Camino Real to New Mexico for use of the inhabitants of the colony. Consumption of chocolate and sugar became more widespread. In New Mexico majolica continued to be symbolic of the Spanishness of the colonists whether they were actually of Spanish descent or not. Loza was a reflection of the knowledge, the customs and traditions, of proper behavior and polite society on the frontier. It was a signal to all that the Spaniards who lived in New Mexico were not the uncouth, rude barbarians they would be characterized as by historians and others in twentieth century.

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am pleased to be honoring two good friends and valued colleagues, particularly because it has afforded me the opportunity to trace the webbed lines linking a set of personal relationships, a "bold" hypothesis, and a process for interpreting Chacoan prehistory. Dick Woodbury played a part in that process.

Clyde Kluckhohn's strong personal and professional ties to the Southwest, especially his interest in the Navajo, are well known. In Dick Woodbury's (1961) summary of Kluckhohn's professional career, he makes special note of Kluckhohn's early interest in archaeology. That interest was honed from 1932 to 1934 when he served as an Assistant Professor of Anthropology at the University of New Mexico. In August, 1937 Kluckhohn returned to UNM from his teaching post at Harvard University to direct the Chaco archaeological field session. His work during that brief month subsequently had an impact on both Dick Woodbury and Gordon Vivian, and, in turn, through my father has influenced my thinking about Chaco and its place in the Puebloan World.

Chaco field work in 1937 was focused primarily on the small house site of Bc 51 in the Casa Rinconada complex of small sites. Limited work also was carried out at nearby Bc 50, which had been partially excavated in 1936 by the field school directed by Donald Brand. After the month's excavation at Bc 51 was completed, Kluckhohn assumed responsibility for the final report, which was published in 1939 (Kluckhohn and Reiter).

To insure rapid publication, Kluckhohn enlisted the help of his co-editor, Paul Reiter, and the field school teaching staff including Florence Hawley and Donovan Senter. He also engaged the assistance of several graduate students who were assigned the task of analyzing and describing various classes of artifacts recovered from the site. Using the greater Southwest as a base, the students then prepared distributional studies of each artifact class. Dick Woodbury was assigned "Ground and Pecked Stone Artifacts (Other than Arrow-shaft Tools)." Dick writes (personal communication 2004) that his participation went back "to the lucky accident of my being assigned to Clyde Kluckhohn as my tutor (at Harvard) when I was an undergraduate." When Kluckhohn farmed out the artifact classes Woodbury was assigned ground stone which "I was pleased to have ... and it became my first publication (Woodbury, personal communication 2004). Though Dick was quite familiar with ground stone analysis, having worked on the Awatovi materials for J. O. Brew, his Chaco task was complicated by the weight of the tools. An editorial note (Kluckhohn and Reiter 1939:58) in the Bc 50-51 report indicates that "there were not funds available to ship the heavy implements to Cambridge, and hence Mr. Woodbury had to work from the field catalog." Despite this handicap, Dick's analysis was thorough and served for many years as the best source on ground stone artifacts from
Chacoan small house sites. He concluded that ground stone from Bc 51 essentially resembled that from two other excavated small sites, Bc 50 (Brand et al. 1937) and Leyit Kin (Dutton 1938). He also noted a number of similar artifacts from the Chetro Kelt and Aztec West Ruin, both Chacoan great houses.

Dick's contribution and other chapters in the Bc 50-51 final report provided Kluckhohn with a solid base for examining another Chaco phenomenon (though he did not identify it as such) in his "Discussion" chapter of the site report. In this case, he observed that on the basis of the Pecos Classification, particularly the use of architectural criteria for assigning sites to temporal periods, contemporaneous sites in Chaco Canyon would be classified as both Pueblo II (small house sites) and Pueblo III (great houses). Similarly, when Roberts' (1935) classificatory terminology was employed, Kluckhohn (Kluckhohn and Reiter 1939:157) noted that "during a certain time interval, the cultures on the north side of Chaco Canyon were Great Pueblo, the at least partially, contemporary ones about a mile away on the south side were Developmental Pueblo." Dick paid special attention to this critique of the Pecos and Roberts classifications in the obituary he wrote for Kluckhohn (Woodbury 1961). He pointed out that Kluckhohn's evaluation of Southwestern classificatory schemes had "stimulated many archaeologists to examine more carefully the bases on which their chronological schemes were built" (Woodbury 1961:408).

Gordon Vivian was also influenced by Kluckhohn's "Discussion" chapter, but not precisely with respect to the evaluation of the Pecos and Roberts classifications. Dad was not a student of Kluckhohn's, but he was at the University of New Mexico working for Edgar Hewett during Kluckhohn's tenure in the Department of Anthropology. Their contact increased in the summer of 1937 when Kluckhohn directed the work at Bc 51 and our family was in the canyon as Dad prepared to launch the Ruins Stabilization Unit. Though Kluckhohn was in Chaco for only a short period that summer, there was sufficient time for Dad to almost wreck their friendship. Kluckhohn carried with him a set of cookware that he had put together years earlier during several long horseback trips into the Navajo country that he described in To the Foot of the Rainbow (Kluckhohn 1927) and Beyond the Rainbow (Kluckhohn 1933). At some point that August Dad discovered the blackened and food (particularly oatmeal) encrusted pans, pots and utensils and as a favor to Kluckhohn thoroughly cleaned them — failing to realize that Kluckhohn had spent numerous summers perfecting their appearance.

I do not know when Dad's thinking about Chacoan prehistory was first influenced by Kluckhohn's "Discussion" chapter. Unlike a number of his contemporaries, however, Dad was less interested in the terminological problems and far more intrigued by Kluckhohn's attempt to resolve the Pueblo II – Pueblo III Chaco conundrum. Kluckhohn noted that there were at least two possible solutions to this problem if both small house sites and great houses were contemporaneous — and all the evidence suggested that for the most part they were. The first solution, which Kluckhohn noted had been proposed previously (Hawley 1937), interpreted great house and small house architectural variability as the reflection of conservative and progressive social units in the canyon — with great houses representing the more progressive element. Kluckhohn (Kluckhohn and Reiter 1939:153), on the other hand, proposed "rather boldly, an alternative hypothesis." Briefly, he argued that the cultural traditions of occupants of the two site types in Chaco were not precisely the same and proposed that small house populations may have migrated into the canyon bringing a different "cultural heritage" and possibly even "a different language from the dwellers, in say, Pueblo Bonito" (Kluckhohn and Reiter 1939:159). He bolstered his hypothesis by citing "the Laguna group at Isleta, the Tewa on First
Mesa, (and) the plausibility of Keresan-speakers in the Jeddito Valley...” (Kluckhohn and Reiter 1939:159).

Though recent evidence (Windes 2004) suggests that the roles may have been reversed with great house dwellers entering Chaco Canyon to find already established small house populations, the possibility of at least two Puebloan groups practicing somewhat different cultural traditions while residing as neighbors in Chaco Canyon appealed to Dad. He briefly outlined his thoughts on “Parallel Developments” in the Three-C site report (Vivian 1965) and then more thoroughly dealt with them in the Kin Kletso report (Vivian and Mathews 1965). In the Kin Kletso discussion Dad and Tom Mathews expanded upon Kluckhohn’s hypothesis by contrasting nine Hosta Butte Phase (small house sites) and Bonito Phase (great houses) cultural traits. But they also added an additional phase, the McElmo, which they believed represented a late intrusive unit into the canyon from the northern San Juan Basin. Finally, they argued that some of the variability characterizing Chacoan culture reflected an early but continuing divergence of Chacoan culture from the mainstream of what they termed the “Northern Pueblo continuum” (Vivian and Mathews 1965:115).

I, like my father, have accepted the premise that Chacoans took an early and different cultural trajectory from many of their neighbors and, like him, I have remained committed to the belief that Chacoan peoples were Puebloans trying to make a living in a Puebloan way in a somewhat unique and often harsh environment. In a time of new and divergent views about the evolution of Chacoan culture, I acknowledge the potential professional folly of citing material written four decades ago by a relative. However, I believe Dad’s final statements on Chacoan “cultural values” are as germane today as they were in 1965.

He (Vivian and Mathews 1965) argued that “Northern Pueblo” peoples had evolved from a Desert Culture base for several millennia in a rather direct “slant” or “form” ultimately becoming the Rio Grande pueblos at the beginning of the historic period. During this time Northern Pueblo culture was “not appreciably altered by external influences but integrated only those that were consistent with its ultimate direction” (Vivian and Mathews 1965:114). Dad noted that this meant that “the Northern Pueblos were living in comparatively small independent groups with little or no interpueblo activity, that one group of Pueblos was not held in subjugation by another, and that there were no dominant Pueblo groups based upon military alliances” (Vivian and Mathews 1965:114). He then placed developments in Chaco within this Northern Pueblo context.

The developments in the Chaco in the 11th and the early part of the 12th centuries were not in the direct line of the Northern Pueblo continuum as it was exposed at the beginning of the historic period. The continuation of the direction taken by the Chaco group would have carried it even farther out of the stream of development that culminated in the Rio Grande. The distinctive traits that we have so often emphasized—great kivas 50 to 60 feet in diameter, great kivas serving more than one community, tri-walled structures, tower kivas up to three stories in height, interpueblo water collecting systems—all imply a growing measure of specialization, social control, and interpueblo control. The elaboration of these institutions with ever-increasing control, specialization, and centralized authority was simply not compatible with the ‘slant’ or ‘form’ that directed the destiny of the Desert Culture-Basket Maker-Rio Grande continuum. The cultural genes that survived in this continuum were those con-
cerned with strong conservatism, with a lack of social control, with rejection of centralized authority, and with a ritual controlled by small participating groups. In this light then, the highest developments in the Chaco were cultural experiments or deviations that failed as they strayed from the main course of Northern Pueblo history. (Vivian and Mathews 1965:115).

Dad did not identify his "Northern Pueblos" specifically, but reading between his lines it is clear that he considered them to be peoples of the greater Chaco and Mesa Verde regions who eventually shifted into the Rio Grande drainage. He notes,

[that during]...this movement the most elaborate of the ceremonial aspects and the higher levels of social control were lost, and that emphasis was regained or redirected toward the subsistence efforts of small units of population. It shows on the one hand the losses experienced during the breakup and dispersion of well integrated communities and the stability or persistence of the local Rio Grande traditions in the face of intrusions. (Vivian and Mathews 1965:114)

Though I (Vivian 1990) have hypothesized a Tanoan-like system of dual organization for Chacoan great house populations, there have been few attempts to implicitly infer Chacoan forms of sociopolitical organization from ethnographic data on the historic and contemporary Rio Grande pueblos. I have never had a problem drawing upon such data for helping to interpret the past, though in doing so some colleagues have placed me squarely in a "Puebloan box." At the same time, I have never rejected the need for a wider analytic base and subscribe to the "comparative archaeology" defined by Cordell et al. (1994). And, I note with some satisfaction that within the past decade, Southwestern archaeologists in various publications (e.g. Gumerman and Gell-Mann 1994; Cordell, Judge, and Piper 2001) have begun to reevaluate the use of puebloan analogs, a practice soundly rejected in the 1980s (e.g. Upham 1987).

Jerry Levy (1994:234), for example, when discussing strategies for reconstructing puebloan archaeological cultures asked "...what sort of ethnographic analog is better suited for the task, for it is a given that there is no interpretation of archaeological data without the use of an ethnographic analogy of some sort." He (Levy 1994:234) cautioned that "a random selection of ethnographic cases" that appeared to suit a researcher's purpose was "perhaps even more dangerous than ... the simple application of contemporary Pueblo data to prehistoric sites." Florence Hawley Ellis (1983) made a similar argument in an elegant evaluation of Chacoan interpretive schemes, and Dozier (1970) raised Levy's same concerns more than two decades earlier.

The repeated arguments made against using Pueblo analogs include intense acculturation following more than 400 years of Euroamerican contact and a presumed or postulated "simplification" of previously more complex societies. Chacoan culture in particular is often cited as having reached levels of sociopolitical complexity far exceeding that of historic pueblos. Moreover, Chaco as a cultural system is often cited as not only complex but possibly unique. For example, Saitta (1997:21) has commented that "Chaco's trajectory was different from that of ancient states and civilizations and possibly even exceptional on a global scale..."

Yoffee (2001:70) describes this claimed uniqueness as "...the singularity of Chaco in Southwestern prehistory...," a singularity he believes was matched by cultural plurality expressed in architectural diversity and "presumed large amounts of linguistic and ethnic variety ... in the Chaco system." Despite his acceptance of
this potential singularity, which he believes was expressed as a “rituality,” Yoffee is less convinced of politically and economically stratified complexity in the Southwest — including Chaco. Noting that “…the Southwest has clearly joined in the archaeological trend toward ‘complexity inflation,’” (Yoffee 1994:345), he points out (Yoffee 1994:352) that “there are all sorts and kinds of complexity” with degrees of differentiation and stratification. To support his point, he cites the Hopi who “…by rejecting trends towards political and economic power and dominion, … achieve their own richly complex ceremonial and intellectual behavior within a kinship system in which contests over status and leadership are resolved in the Hopi way” (Yoffee 1994:353).

Yoffee’s emphasis on kinship-based complexity and his Western Pueblo example reinforce Ware’s (2001) admonition that Southwestern archaeologists (and occasional Mesopotamianists) who utilize Puebloan analogs would be better served if they became more familiar with Eastern Pueblo (i.e. Rio Grande) ethnographies. Ware contrasts Western Pueblo kinship-focused organization with Eastern Pueblos that were and are “…governed by hierarchies of priests – members of exclusive sodalities who exercise authority over the ritual and, in many communities, the mundane aspects of everyday life” (Ware 2001:83). Ware makes the important point that despite the essentially sovereign political and economic nature of each pueblo, there was “…a high level of connectedness among the ceremonial elites of different pueblos, and these connections appear to have always had important social, political, and economic ramifications” (Ware 2001:83).

Sodality-based governance among Eastern Pueblos is thought (Dozier 1960, Ortiz 1965, 1969) to have deep roots. Ware (2001:83) notes that “If sodalities emerged as centralized polities much before the thirteenth century, then Chaco, as well as other expressions of Eastern Anasazi complexity, may well have been shaped by the process.” One is reminded of Gordon Vivian’s “Northern Pueblo continuum.” If this was the case, Chaco may not have been as singular, unique or unusual as some propose. Ware (2001:82) concurs, observing that “The surest way of making something appear strange is to detach it from its historical context, and in my judgement, this is precisely what happens when we ‘singularize’ Chaco.”

But we also should be cautious about emphasizing the ritual aspects of Chacoan organization. Yoffee (2001:67) clarifies that his term ‘rituality’ is not intended to substitute a mode of cultural integration in place of what others have seen as a political integration.” Nonetheless, the mere force of the term underscores the increasing tendency of archaeologists to describe Chaco Canyon as a ritual-focused ceremonial center or a “location of high devotional expression” (Renfrew 2001). Within this center power was vested in religious specialists practicing their craft in the “ritual landscapes” (Stein and Lekson 1992) of Chacoan great houses. Pueblo Bonito, the building, has recently been described as “an occult engine powered by the cycles of the cosmos” (Stein and Lekson 1992). And Marshall (2003) believes it was a “monumental architectural sacrifice,” having been purposefully placed below a gigantic slab of cliff face which, according to Marshall, the Chacoan builders knew would eventually fall and crush the structure.

These scholars argue that new directions are necessary for understanding the emergence and growth of Chacoan social and political organization inasmuch as existing “ecofunctionalist” and “ecologic and economic” models have not provided satisfactory explanations. This may be so, but substituting ritual for ecology and economy does not bring us any closer to understanding Chaco. Ware (2001) has pointed us in one potential direction for a better comprehension of things Chacoan, but he also emphasizes the ritual aspects of sodalities in Eastern Pueblo organization.
believe those aspects should and must be considered within the broader matrix of Eastern Pueblo dual organization.

Tewa dual division is the most highly evolved of this organizational form and one of the best described (Dozier 1960, Ortiz 1965, 1969). Members of all Tewa pueblos belong to one of two moieties, each of which “is the mirror image of the other in organization and function” (Dozier 1960:150). The core of each moiety is the moiety “association,” or “sodality,” or “society.” Responsibility for all village governmental and ceremonial affairs are alternated seasonally (fall and spring equinox) between the chief or head priest of each association. Not all adults in a pueblo join a moiety association, and membership is restricted by moiety. Each moiety society is assisted by several other subordinate sodalities or associations who are responsible for special functions including curing, hunting, and war. Membership in these sodalities, unlike moiety associations, cuts across both moieties.

One of the most critical features of Tewa dual organization is the degree to which steps are taken to reconcile the reciprocal and hierarchical aspects of dualism. Continued reciprocal exchange of personnel and goods—primarily foodstuffs—between the two moieties during the year is guaranteed by insuring that the hierarchical status of each moiety is temporary as a result of seasonal changes in leadership. Moreover, alternating control of ritual is assured through an annual process of selection of deities impersonated in Tewa rituals. Thus, through a process of seasonal and annual rotating, sequential but temporally asymmetrical exchanges of duties and responsibilities, it is “almost impossible for either moiety to attain a clear-cut ritual or political supremacy,” so that “the moieties can never really be uniformly divided on a major structural issue” (Ortiz 1969:91). Other measures are taken to insure harmonious relations between the moieties, thereby benefiting the entire pueblo. But the critical point is that these processes are intertwined in all segments of Tewa society—not just ritual.

Economic considerations are especially vital. Among the primary functions of moiety associations are the organization and direction of planting and harvesting, the supervision of construction and annual cleaning of irrigation ditches, and the coordination of communal hunts that are carried out by the hunt society. The moieties themselves directly reflect annual changes in subsistence activities; the Summer moiety chief controls agriculture, whereas the Winter chief is responsible for non-agricultural activities, primarily hunting. Ritual is organized around the economic cycles and includes a parallel series of subsistence-related activities including food redistribution.

Both Ware (2001) and Yoffee (2001) suggest an early (i.e. prehistoric) tension between emerging sodalities and older kinship systems with sodalities assuming greater control of ritual. Thus, Yoffee (2001:73) suggests that “If the Chaco ‘rituality’ is not politically organized, a species contradiction between those elites invested in the performance of ritual at Chaco and those elites of the local social organizations that comprised the greater Chacoan network might be considered.”

There is an alternative. Ortiz (1969) has argued persuasively that a concept of duality is integral to Tewa organization and that it has ancient roots. If Chacoan great house populations were organized through some form of dualism, the principle of duality may have had ancient roots in Chaco as well, and the presumed tension or contradiction might not have existed. Recent work by Wilshushen and Windes (Windes 2004) is providing increasing support for a movement of groups in the middle A.D. 800s from southwestern Colorado, and more specifically the McPhee Reservoir area, into Chaco Canyon via the Chaco Wash. These northern pueblo peoples may have brought with them some form of dual organization.
Ortiz (1969) observed that societies with dual organization usually inhabit areas with considerable change in weather from one half of the year to the other. Invariably this resulted in a dual subsistence system within the local area or seasonal movement between areas. Evidence suggests that environmental characteristics of upland areas surrounding the San Juan Basin, particularly to the north, permitted the aggregation of larger social groups on a more permanent basis. Those characteristics included a wide range of diverse resources within a close vertical proximity, the presence of permanent large game species, and sufficient moisture to encourage and enhance farming. Evidence for longer-term aggregation in the greater San Juan Basin during the Late Archaic has been found only in the Los Pinos communities of the northern Basin.

These environmental characteristics are essentially duplicated in the McPhee Reservoir area where large ninth century villages may have evolved from Late Archaic Los Pinos communities. While those environmental features are not so pronounced in Chaco Canyon, the proximity of relatively well watered farm land and some verticality of resources, including large game species, do characterize the juxtaposition of the canyon and the Chacra Mesa. A greater value of dual organization in adapting subsistence activities to the Chacoan environment may have lain in those inherent mechanisms that permitted the functioning of large aggregated groups. This could have been particularly beneficial in a frontier setting for coordinating large work forces practicing intensive agriculture and as a means for administering the mutual pooling and sharing of resources. I have discussed the potential value of dual division in the establishment and elaboration of great house systems in more detail elsewhere (Vivian 1990).

Ritual obviously was vital to this process. Wilshushen and Ortman (1999) have documented the evidence for ranked or ritual leadership in ninth century McPhee villages and similar manifestations may be present in early Chacoan great houses, especially Pueblo Bonito. But early great house farmers did not enter Chaco Canyon in a quest for a ceremonial center nor were their buildings erected as ritual landscapes. Their quest was for a sustaining environment and their nucleated buildings were erected to store foodstuffs and possibly to conceal them from existing canyon residents - the residents of small house sites. The monolithic presence of these buildings reinforced the cohesiveness of their residents while also conveying a sense of protective power over adjoining improved farmland. Ritual trappings on this essential core increased when moisture regimes were good but probably became static during downturns in precipitation.

This brief exercise in scenario-building is intended to underscore the need to return to a full consideration of all elements that constitute and contribute to a viable social system. If we unquestionably assume that Chacoan great houses were ritual landscapes and that our only task is to further delineate the process by which ritual created Chaco we will commit ourselves to elaborating upon an unconfirmed premise. The Chaco data suggest, in fact, that ecologic and economic factors were critical in fostering and sustaining the Chacoan system, whatever its makeup. Rather than rejecting so-called ecologic and economic models we should incorporate them with ritual and other models. This is vital not only for developing improved explanations of the cultural processes at work but also for giving us a better understanding of the Chacoan World and its people.
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