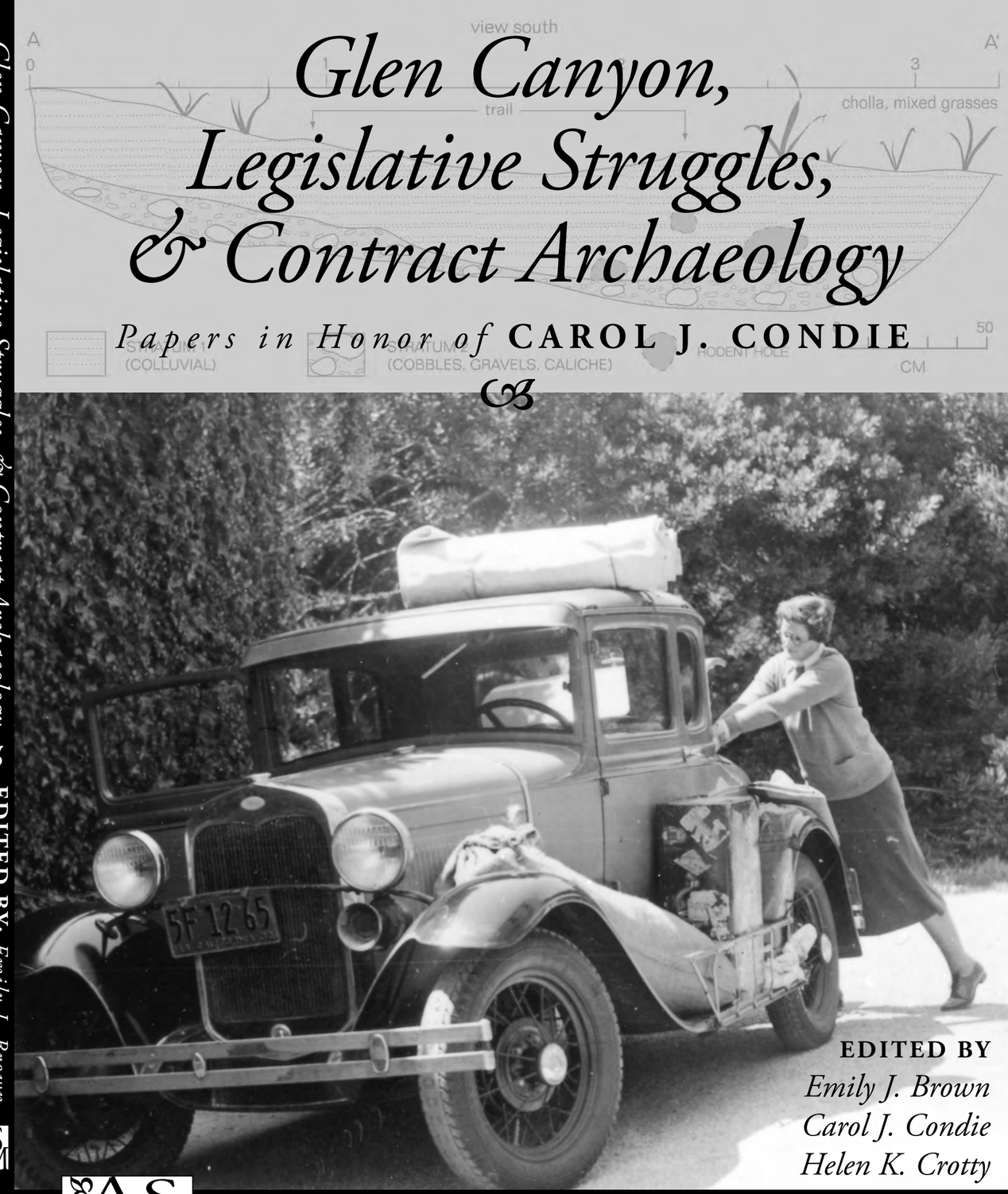
CONTRIBUTORS

PAPERS OF THE ARCHAEOLOGICAL SOCIETY OF NEW MEXICO 38 ❧ 2012

Glen Canyon, Legislative Struggles, & Contract Archaeology
Papers in Honor of CAROL J. CONDIE
❧ EDITED BY: *Emily J. Brown, Carol J. Condie & Helen K. Crotty*

38 ❧ 2012



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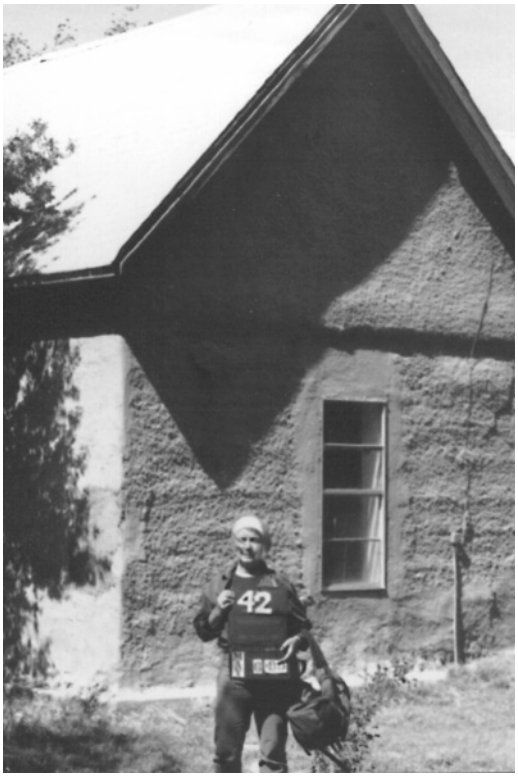

Papers in Honor of

CAROL J. CONDIE

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TABLE OF CONTENTS

PREFACE

I first met Carol Condie when I was attending meetings of the Board of Trustees of the Archaeological Society of New Mexico as editor of the newsletter and she was serving as a member of the board. Even before we began to work together more closely, if intermittently, as two members of a group of co-editors of the society's annual volume, I was impressed by her intelligently thought-out and well-spoken contributions to board discussions. I therefore felt a great sense of relief and gratitude when she volunteered to serve as one of the co-editors on the editing committee, of which I was the chair despite my lack of experience—the whole endeavor would go well after all! Carol's experience with editing, knowledge of archaeology and linguistics, and meticulous attention to detail continue to ensure that the ASNM annual volumes are professional publications. As I worked with her more closely I began to appreciate her other talents and contributions as well. Her involvement with developing public policy and her work to hold government agencies accountable for protecting archaeological sites on public lands will have a lasting impact. Her archaeological research is meticulous and expertly done, and all the organizations to which she contributes benefit greatly from her time and talents. Carol knows her own mind, is an excellent role model for those of us at earlier stages in our careers, and knows how to make mean dill pickle. As one contributor to this volume admiringly put it, Carol "is… made of stern stuff, and we love her."

—Emily J. Brown

Carol J. Condie, Autobiograhical Notes

CAROL J. CONDIE

CONTEXT

Since I didn't spring full-blown from the brow of Zeus, a little context may be useful. Both of my parents came from old Mormon families who settled in St. George and Santa Clara in the Virgin River Basin of southwestern Utah. But settlement in the basin wasn't just happenstance. Although it's some of the most gorgeous country on earth, Brigham Young, the President of the Mormon Church (1847-1877) had other reasons for wanting that corner of Utah filled with permanent settlers. The Old Spanish Trail ran nearby and Brigham saw the trail as a route for Mormon converts to follow from California to Utah. He also wanted local Indians converted to Mormonism as a way to safeguard travelers from possible Indian attacks. Perhaps most important, he urgently wanted Mormons to become economically independent and, to that end, handpicked settlers who held a diversity of skills that would lead to self-sufficiency for each community and for Utah as a whole. (I haven't done the research, but I suspect he may have imitated practices followed by Spanish colonizers in New Mexico. This, and irrigation customs prevailing among both Pueblo and Spanish communities, would have been observed by Mormon officials while the Mormon Battalion was in New Mexico in 1846 fighting for the U.S. during the Mexican War.)

My father's mother, Annie Stafford, an orphan raised by an aunt and uncle in Sheffield, England, converted to Mormonism at the age of 16. The missionary who converted her paid her passage to the U.S. and on to Utah. She, thus, became a bond servant and went to St. George, where she worked for his wife. When her benefactor completed his mission he returned to Utah and, after Annie had worked out her servitude, he took her as a second wife (which, as in many instances of plural marriage, made his first wife none too happy). Polygamy had been declared illegal in the U.S. in the Anti-Polygamy Act of 1862 and was later strengthened by the Edmunds Bill in 1882. Most Mormons ignored these acts. However, Utah (then the Territory of Deseret), anxious to become a state, had been rejected several times because of polygamy and, in 1890, the president of the church issued a manifesto instructing church members to abstain from polygamy. This, of course, created a problem for superfluous wives and children. Since Annie's husband was an important official of the church, they, like many others, spent a lot of time in Mexico City and Colonia Juarez running from the Feds. Later, she lived on the Arizona Strip in a community of plural wives. Husbands visited once or twice a year to replenish staples the wives couldn't raise in their gardens and flocks. The birth rate did not diminish (for Annie, the final total from this marriage was 12 children). After her husband died, she met and married my grandfather, George Condie, who was not a polygamist, in 1906. Beyond the fact that five Condie brothers joined the church and migrated to Utah and Idaho from Scotland, I know almost nothing about them. My father, Leroy, was three years

old and his mother was pregnant with his younger brother, Carl, when George died. Oddly enough, in spite of what seems now a shockingly hard life, Annie was always cheerful and optimistic.

My mother's family was Swiss. Her parents, George Graff and Bertha Stucki, were both descendants of original Santa Clara settlers. Beginning in 1854 Brigham Young had sent people to establish a scattering of small settlements in southwestern Utah. Experimental plots of cotton had been raised successfully since 1855 and after the Civil War began it became apparent that cotton from the States would be in extremely short supply. In October 1861 Brigham "called" 309 families to establish the "Cotton Mission," with St. George as the administrative center.

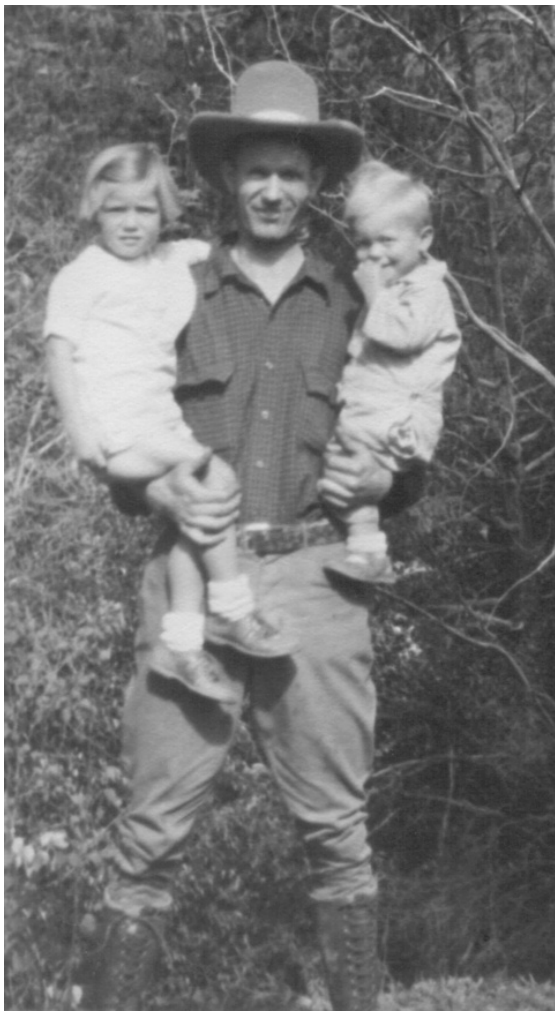

Figure 1. Roy Condie with George and Carol, ca. 1934.

(The cotton industry became responsible for the name "Utah's Dixie" being applied to the southwestern corner of Utah.) Among the 309 families were 85 Swiss immigrants from various cantons in Switzerland who had settled at different towns in northern Utah between 1857 and 1861. They were formed into the "Swiss Company" and sent to settle Santa Clara. They were skilled as farmers, vintners, carpenters, and other trades. Since I know their history best, I'll use my family as a brief example. One was a musician, but had also owned a small cotton spinning factory. Another was a farmer, but was also a turner who made farm implements and spinning wheels. My great grandmother's family, who arrived in 1874, worked as weavers of fancy silks. We weren't unique. Every family had a background of useful skills and knowledge. The old Swiss lifeway is mainly gone now, but the town's Swiss heritage is still visible in the beautiful flower gardens gracing every front yard.

GROWING UP

As for my immediate history, I was born in Provo, Utah on December 28, 1931 and my brother, George, followed 18 months later. Our parents were Thelma Graff and LeRoy Condie. After my father finished his Bachelor's degree at Brigham Young University and had worked in business for several years, we moved to New York, where he earned a Master's degree in Retailing at New York University. The idea seemed to be "If you don't have a good job, go back to school." In addition to no job, we also didn't have much money, but we frequented New York's many wonderful and low-cost museums. Then—back to Utah and to Salt Lake City. Fortunately, as I became a teenager and began to question Mormon dogma, my parents remained calm. (I realized later that they had made their own peace with the church.) Ours was a happy household, punctuated with lots of laughter. We hiked and roamed around every weekend and took vacations that no one else we knew took (this

left–
Figure 2. Thelma Graff Condie with George and Carol, ca. 1934.

below–
Figure 3. Family and friends, Santa Clara, Utah, Easter, ca. 1942. Back row, left to right: Stanley Ray, Leona Ray (Bertha's sister), Joe Ray, George Graff (Thelma's father), Thelma Graff Condie, Bertha Graff (Thelma's mother), Annie Condie (Roy's mother), Barbara Graff (George Graff's mother), Amanda Graff (George Graff's sister). Front row, left to right: Charmaine Ray, Marian Ray, Carol Condie, Lorna Graff, George Condie, Dale Gubler, DeLoy Gubler.

was during the Depression)—to Yellowstone a time or two, to New Mexico and Arizona every summer, etc. After I grew up I wondered how they did it. We weren't as poor as church mice, but close to it. The answer is that the self sufficiency handed down from earlier generations still prevailed. We never stayed in hotels, but always camped (together, my parents had made a huge umbrella tent on my mother's portable sewing machine). We almost never ate in restaurants, but took a lot of our own food with us (they always raised a big garden) and cooked it on a Coleman stove. While we lived in Provo, they raised chickens and we had a cow (which my dad often tethered in shady, grassy areas on the high school campus).

UNIVERSITY OF UTAH

After I graduated from high school, I went on to the University of Utah. The university was an enclave in a fashion I've never seen at another university and that may not exist elsewhere. The faculty included devout Mormons, Jack Mormons, and non-Mormons. Many of the faculty who were descendants of old Mormon families had neither left the church informally nor been excommunicated, but were no longer believers—the so-called Jack Mormons. They stayed in Utah because treasured family and friends were there. However, they were very clear that the University of Utah was *not* Brigham Young University and were exceedingly touchy about academic freedom. I suspect the University of Utah sustained perhaps the most active and fervent Academic Freedom and Tenure Committee in the U.S.

If the university was an enclave within Utah, anthropology and a few other departments were enclaves within the university. Members of the Anthropology Department joined with like-minded members of english, philosophy, history, biology, sociology, and psychology to hold monthly seminars, to which graduate and even undergraduate students were invited. For kids coming out of Utah public schools, these were heady occasions.

Jesse D. Jennings

Any description of the Department of Anthropology necessarily centers on Jesse D. Jennings. Although I had taken a couple of courses from him, my association with him really began during a field school when we were excavating a Fremont site on the Utah-Nevada border. Just prior to that I had been working at RKO on Salt Lake's Film Row. My boss was considered the meanest man on the Row. In those days, bookers would travel to Salt Lake from towns throughout Utah, Idaho, and Wyoming to book films for their theatres. Ell Winward's reputation was so bad that bookers would stand outside and peak through the slats of the venetian blinds covering our big plate glass windows to see if Ell had gone to lunch. If not, they would wait until he had before they came in so they could deal with his pleasant, cheerful assistant. I didn't know everyone was supposed to be terrified of Jess and the result of my recent occupation was that I thought Jess was being hilariously funny when he would fly into a rage about something. (Only later did I come to the realization that he was dead serious and that you had better have your ducks in a row before you talked to him. Otherwise—humiliation.)

Jess discovered I could type when I got tired of hearing his labored two-fingered typing one night in the house we rented for a field office/artifact conservation lab/kitchen/dining hall and begged him to let me finish the job. That led later to a stint as secretary of the department until I graduated.

CORNELL UNIVERSITY AND AFTERMATH

With a B.A. in hand, I left for a one-year experimental program in elementary teacher training at Cornell. When I returned I had planned on teaching, but the Utah State Board of Education was reluctant to honor a Master's degree from Cornell, so I worked another year as department secretary until I was granted a teaching certificate.

In the meantime, I married Kent Stout (and divorced him many years later). I taught third grade for a year, worked for one of the vice presidents of the university for a couple of years, moved to Burlingame, California for a year while Kent worked as an architect for the National Park Service, and returned to Salt Lake, where Carla, our first, was born soon after, in January 1958.

THE GLEN CANYON PROJECT

By this time the Glen Canyon Project was underway and I started back to graduate school and also joined the project to serve as assistant editor of the *University of Utah Anthropological Papers* and its two sub-series, the *Upper Colorado Series* and the *Glen Canyon Series*. This meant that a faculty member's name was shown as editor and I did the editing. (Erik, our second, was born in December 1960.) In what I have always considered a stroke of brilliance, Jess had insisted that the Park Service include a clause in the contract stipulating that funding for the next field season would not be released until reports of the previous season's fieldwork had been completed and published. Accordingly, we ran full tilt all year. (A reflex of this was that when non-project people would attend our parties they frequently wanted to sign on, presumably believing that we partied all the time. If they did come to work for the project they were aghast to discover that, instead of constant partying, we worked like demons!)

The Glen Canyon Project was good training. Several people went on to illustrious careers. As students and employees of Jess Jennings, we had several precepts drilled firmly into our brains. I think Jess would almost have overlooked murder before he would forgive not reporting the results of an excavation. Failing to place the information derived from a site in the public domain made one a looter. Lying about or faking data meant the death of your career. You could be wrong or misguided or stupid, but one lie and you could never recover. He was also violent about authors who were too sloppy to go back to an original reference, but relied on second-hand interpretations (which, of course, are often wrong or at least skewed). He even invented his own little test to ferret out such people. He would throw a ringer into a paper, complete with a false text citation and false bibliographic reference—then watch to see who cited it, a quick tip-off to who could not be trusted.

ALBUQUERQUE AND THE UNIVERSITY OF NEW MEXICO

Because my parents had moved to Albuquerque in 1958, we made frequent visits to New Mexico and became permanently enchanted. In 1964 we moved to Albuquerque and Paula, our third and last, was born in October. I started graduate school at the University of New Mexico in the summer of 1965. Like a lot of other people, I thought I was coming to the North American center of the archaeological universe, but was dismayed to discover a remarkable lack of interest in archaeology at UNM during that era. Since my kids were still too young for me to establish a field project of my own and since there were good linguists on the anthropology and linguistics faculties (Stanley Newman, Garland Bills, and Bruce Rigsby), I ultimately switched my focus to linguistics, found a wonderful Zuni speaker, and wrote my Ph.D. dissertation on transitivity in Zuni verbs. After I finally graduated, I spent several years at the Maxwell Museum of Anthropology as Education Coordinator and director of the Division of Interpretation until I decided I like working in the open air better than working inside.

THE WORLD OF CONTRACT ARCHAEOLOGY

By this time (1978), the kids were old enough to go to the field (or to stay home alone briefly), so I started a contract firm, Quivira Research Center. It would be more accurate to say I *started* to start a contract

Figure 4. Carol Condie, holding photo board in front of a residence in Elida, NM, 1999. The purpose of the project was to record all buildings along 63 miles of Interstate 70. This photo shows an unusual construction technique that we noted several times in Elida and nearby Kenna—poured adobe walls covered with cement stucco. Wooden forms were placed as they would be for a concrete pour, but adobe mud was poured into the forms instead and allowed to set up before the forms were removed.

firm. In those days, federal agencies could contract only with 501(c)(3) organizations, so I assumed it had to be a 501(c)(3). What I didn't know was that what seemed like half the people in the U.S. were establishing little churches in order to achieve 501(c)(3) status and avoid income taxes. This made the IRS highly suspicious of anyone requesting 501(c)(3) status. It took a couple of years and reams of paper in constant responses to endless queries before IRS was convinced that we really intended to conduct archaeological investigations instead of a scam. By 1983 federal agencies had instituted small business set-asides and I formed Quivira Research Associates, a sole proprietorship.

One of our first contract jobs was to record all of the old buildings at Ft. Bliss in El Paso. I should have known something was wrong because two other contract firms had defaulted on their contracts soon after they had started. Since Kent was an architect, we took the job on together. As we were leaving our initial meeting with Rex Gerald, the Principal Investigator, at the University of Texas at El Paso, I casually thought to ask him how many buildings there were at Ft. Bliss. He said "Twelve thousand." When we got outside, I said to Kent, "Oh, s_ _t, let's quit!" A few minutes later, however, I realized that an installation the size of Ft. Bliss had to have a list of buildings, if only for maintenance purposes. Kent had been in the Army and said the Post Engineer would have the list. They graciously said they could make us a copy by the next morning—and they had it ready at 8:00 a.m. Would that all organizations documented their buildings so thoroughly! The inventory included exact locations, dates of construction, dates of additions, square footage, construction materials, and other pertinent information—cutting our research and recording time down enormously. From then on, the project was fairly standard, with one eerie little exception. We were working on the quad at the old Beaumont Medical Complex. Beaumont had been famous for its beautiful gardens and buildings, but now many of the buildings on the old quad were slated for demolition. The entire quad, with its lawn still green and carefully manicured, was vacant and silent. The 1921 section of the hospital complex consisted of frame buildings connected by raised, covered walkways that ran above interior gardens. We climbed the ramp of a hospital building, passing oleanders in full bloom, and pushed through swinging doors into the front hall. Somewhere an air conditioner was running at top speed. We opened another door and suddenly found ourselves in an operating theatre with tiled floor, tiled ceiling, and tiled walls. Everything looked normal—except that two of the walls were gone, only jagged stubs attesting their former presence. For the space of several seconds it was as if we were the last two people on earth, the only connection to previous human life being the lonely sound of the air conditioner.

I'm sure my career as a contractor has been much like every other small contractor's. For the first year or two, I was afraid the phone was never going to ring again. A few years down the road and I began to *hope* it would never ring again. I didn't know then that contracting, regardless of the specific profession, is always feast or famine. One of the pleasures of the business world is the variety of interesting people one meets in the course of work or acquires as clients. Another is that you can fire unpleasant clients and, if you're a small independent firm, you can also fire unpleasant or incompetent employees.

Enough said about business, with one final story. I know few contractors who don't have silly stories to tell about the time they left some vital piece of equipment behind and had to improvise a makeshift substitute. In this vein, I should report the dumbest thing I ever did. It will surprise no one who knows me well when I reveal that I don't have a brain in my head in the morning. Accordingly, I always put all of the small field equipment, maps, work plans, etc. together in two big plastic boxes the night before and check them over, ready to load the next morning. On this occasion, we had a testing project at a small site in an area slated for expansion of the Tri-Sect Landfill. The landfill is about 50 mi southwest of Albuquerque, south of NM-6 and west of the Rio Puerco, a few miles southwest of Pottery Mound. To reach the site it was necessary to go through a locked gate, drive 6 mi south on the haul road, leave the haul road, go through another locked gate, skirt the edge of the existing landfill, pass down over a clay hill into a normally dry arroyo, then up the arroyo bed for a mile or so. For some unknown reason, this night, instead of leaving the plastic boxes, cameras, transits, tripods, and pinflags in the hallway where we would have to trip over them to get out the door, I put them back in the entry closet, thinking that since they were so big I would notice their absence in the morning and get them out of the closet. When the crew arrived the next morning, we loaded the shovels and screens into the Suburban and prepared to leave. A dim thought crossed my mind that we seemed a little empty, but I decided it was because my son Erik wasn't going to the field that day. The rest of us drove to the site, jumped out, and got ready to start pinflagging artifacts. —No pinflags! No cameras! No tripods! No field forms! No tapes! No equipment boxes! No nothing except shovels and screens! Obviously, we couldn't improvise everything we had left behind. A trip back to Albuquerque was the only option. As we were loading the orphaned equipment I told someone to run into my office and turn the computer on to check the weather forecast since a storm had been promised for the following day. Our little site was one of the last places I wanted us to be caught in a flash flood. We would never get out by ourselves—and, since no one ever passed by, we wouldn't be found for at least six months. The forecast hadn't changed and another hour found us back at the site, unloaded, and ready to roll. After we'd been pinflagging for 10 or 15 minutes, I happened to look west toward Lucero Mesa, which marks the edge of the Colorado Plateau in that area, from whence both large and small arroyos plunge down to the Rio Grande rift to the east—to see huge, angry, black clouds boiling off the edge of the mesa. —So, it was throw everything in the vehicle and depart as the first few raindrops hit us. Another hour took us back to Albuquerque, by which time we had been driving for four hours and had accomplished exactly nothing!

THE RIGHT PLACE AT THE RIGHT TIME

Several times I've been fortunate enough to be at the right place at the right time. Although in a few instances, I wasn't certain "fortunate" was the appropriate word, I feel privileged to have been involved in several extracurricular activities.

Crow Canyon Tours

From 1986 to 1997, I—along with practically every other archaeologist in the Southwest—served as a seminar leader for Crow Canyon field seminars. (Crow Canyon, a well-respected research and educational organization, is based in Cortez, Colorado.) It was an experience that was good for the soul and humbling

for the ego. Associating primarily with members of a single profession can cause humans to lapse into a narrow mindset. Mine was that everyone knows archaeology is a good and valid endeavor and I had long since ceased questioning it. However, from the beginning we had participants who asked "What good is archaeology? Why are you doing this?" At first I had to scramble to give them rational answers. But, I also found that some participants had mindsets for which I was unprepared. One person said to me—at Chaco, of all places, where we were surrounded by magnificent examples of prehistoric architecture—"But they really weren't very intelligent, were they?" Another person was offended when I said I thought prehistoric people were essentially employing scientific method by virtue of close comparative observations. (They were truly shocked when I pointed out that for modern archaeologists, as well, scientific method relies on close comparative observations because we cannot set up the kinds of experiments one learns to do in high school.)

For some people, meeting living Indians did nothing to destroy their fantasies. As we were leaving an afternoon of dances at Santo Domingo, one participant admonished me, saying that "we" really shouldn't let people set up booths to sell cotton candy, jewelry, flashlights, and other modern goods. One of the guides at Chinle had kept a Crow Canyon group particularly captivated one day as we drove up the canyon, telling them about Kit Carson's attack on Cañon de Chelly, the Long Walk, and the Navajos' time at Bosque del Apache. He had also mentioned the tribe's recent purchase of several big ranches. One puzzled participant asked me later why the Navajos were buying ranches. The people in my van were devastated when I explained that they were money-making ventures and that Navajos, like other people, needed to eat and to pay bills.

The inability to see others as humans instead of stereotypes can work both ways, of course. Bob and Florence Lister and I had stopped at Walpi, on the Hopi Mesas, with a group one day. Walpi had apparently been inundated recently with random people wandering all over the village and into people's houses so they had instituted guided tours. On this day, Bob and I were at the back of our group. As we progressed through the village, it became clear that the leader was not going to part with any information, so Bob and I started pointing things out to people near us, but we were soon told "No talking" by the rear guard. However, I couldn't bear to have us pass heedlessly by the pottery firing area at the west end of the mesa and asked the leader if she would point it out, but she refused so I gave up. Since Walpi is a tiny village, we arrived in a couple of minutes at a point where the leader stopped us and explained that two people at a time would be allowed to climb up to the second story to buy pottery from the house of the potter designated to sell her wares that day. While we waited, I asked the leaders if they had been having trouble with some visitors. Oh yes, they said, it had been terrible! They said the Germans were okay—they stayed in line and never said a word. But the Italians! They got out of line and ran all over, looking at everything, talking, laughing, and calling out to each other to come and see this! They said Walpi was thinking of not letting Italians visit the village anymore.

Lawsuit Against the Forest Service

In the early 1980s, while I was president of New Mexico Archeological Council (NMAC), a group called "Save the Jemez" asked us to join a lawsuit against the National Forest Service. The NMAC membership agreed and we began a two- or three-year round of collecting evidence, holding meetings, testifying, etc. We were later joined by the Sierra Club, who provided us with a skilled attorney, and the State of New Mexico.

The impetus for the suit was that sites were being damaged by timbering activities. In some instances, haul roads had been cut through huge masonry structural sites and walls had been knocked down. A more subtle form of damage occurred because the Forest Service apparently did not want to hire trained archaeologists to conduct archaeological surveys prior to timber sales and other ground-disturbing activities. Instead, they were using "para-archaeologists." These

were employees in other specialties who were given brief training sessions in site recognition and then sent out to conduct clearance surveys. In some cases, we believed, a para-archaeologist had a vested interest in finding that no sites existed in the area of the proposed disturbance, and in no cases were para-archaeologists the equivalent of degreed and experienced archaeologists. In the end, we settled, rather than going to trial, but the Forest Service throughout the U.S. was thereafter required to employ genuine archaeologists who conducted 100% pedestrian surveys prior to ground-disturbing activities. A small irony was that the morning after the settlement was announced, I received a congratulatory phone call from a colleague in Washington, who asked whether the news had hit the Albuquerque papers. I told him there was a small article on page 2 of the *Albuquerque Journal* that morning. He responded "It's on the *front* page of the *Washington Post*!" Furthermore, a couple of close friends who were on sabbatical in Peru saw a big article splashed all over the front page of a Sunday paper. So much for those of us who were privileged to be at the eye of the storm!

Petroglyph National Monument

The existence of Petroglyph National Monument is the result of hundreds of hours spent by dozens of people over many years, beginning in the 1970s, to convince city, state, and federal leaders of the importance of the remarkable images pecked and carved into the basalt escarpment on Albuquerque's West Mesa. The escarpment runs for 17 mi and contains roughly 20,000 images.

My involvement was not as a major player. Someone (lamentably, I can't remember her name) came into my office at Maxwell Museum in the mid-1970s to urge me to do something about the petroglyphs, which were already under threat of destruction from housing developments. I had no idea how one went about such an undertaking, so I ended up doing nothing. Fortunately, people who did know how appeared on the scene a little later and led the charge. A fairly sizable bunch of us attended interminable meetings and hearings, led politicians and anyone we deemed influential on tours of the escarpment, lobbied the New Mexico legislature and U.S. representatives and senators, and did everything we could think of to sway opinion.

Several times it looked as if the monument was a done deal, but just as we began heaving a sigh of relief, a new proposal for a new highway would pop up. Both four-lane and six-lane high-speed highways through the escarpment were proposed to accommodate new housing developments. Although the most destructive impacts were avoided, each threat required mounting a new full-scale response. Finally, the monument was formally established in 1990 and is now jointly managed by the National Park Service and the City of Albuquerque Open Space Division.

Albuquerque Archaeological Ordinance

Though it was on-again off-again for more than 20 years and insanely frustrating at times, another instance of being in the right place at the right time was the Albuquerque Archaeological Ordinance. The realization that we needed an ordinance stemmed from an instance in 1983 when a sewer line excavation hit human burials. Since there were no guidelines for such situations, several archaeologists began meeting informally and then began calling on the mayor, Bernalillo County Commissioners, and Albuquerque City Councilors. In late 1984 both the city council and the county commission passed resolutions to establish a 10-member City/County Archaeological Resources Planning Advisory Committee (ARPAC). We met every week from February 1985 through August 1985. Mark Harlan was elected Vice President and I was elected President.

We produced a detailed report that discussed the history and prehistory of the Albuquerque area and made recommendations for establishing the office of a City/County Archaeologist, setting up a public archaeology program, establishing archaeological compliance procedures for developers, and writing and implementing an ordinance. We had hoped we could present the report to the city and county by late

September or October 1985, but final production was out of our hands and the report was not delivered until February 1986. The report received the American Planning Association Planning Award for 1985-1986 and was circulated nationwide to help guide other communities trying to establish ordinances.

We naively thought we would have no trouble after that in getting a City/County Archaeologist funded and hired and that in two more years a full-fledged ordinance and a public education and involvement program would be developed. We were wrong, for several reasons. The first was that Harry Kinney, the mayor who had been so supportive of developing an ordinance, had left office in November 1985. The second was that when we met with the mayor who replaced him, Ken Schultz, he abhorred the thought of an ordinance. The third was that we should have moved faster. Had we known how to circumvent the city intern who was supposed to be working for us and who was responsible for the delay, the report would have been submitted while Mayor Kinney was still in office.

The next few years were no more successful. I met with the next mayor in 1998, who was more polite, but no more enthusiastic than Ken Schultz had been. In the early 1990s, a new committee began meeting and met for several years. Finally, in 1994, we produced an ordinance, which was introduced to the Land Use Planning and Zoning Committee and then went on to the city council, where it drifted and died. Between 1995 and 2005 several of us made sporadic attempts at a grass roots movement, meeting with city councilors, county commissioners, and city and county staff. All of these attempts were dismal failures because we still had no idea what we were doing.

But a final surprise awaited us. Unbeknownst to us, Martin Heinrich, then a city councilor (now the U.S. Representative from New Mexico District 1), saw the need for an archaeological ordinance and began discussing it with other councilors. Lawrence KIine, an architect and planner and a friend of Heinrich's, had served on the original ARPAC committee and told him of our previous ill-fated effort. When Lawrence told me about this we also called David Cushman, who had been on several of the ephemeral committees, had worked at the State Historic Preservation Office in Santa Fe, and had been the historic preservation specialist for Pima County, Arizona for five years. Dave and I met with Lawrence and Councilor Heinrich and offered our services to help produce an ordinance. Councilor Heinrich had been meeting with developers from early on, telling them he would prefer to have them involved, but he thought he had the votes to pass an ordinance with or without them. This time, no committee was established, but Dave and I recruited archaeologists who faithfully attended public meetings when we needed them. We met repeatedly with a group of developers who represented NAIOP (National Association of Industrial and Office Properties). They always cheerfully—and pointedly—began meetings with the fervent wish that we would go away. An attorney for the city council was assigned to write an ordinance, but she found herself in difficulties because she had little knowledge of archaeology. The job was later given to an attorney who had an extensive background in contract archaeology and was able to produce an excellent first draft of a workable ordinance. Dave and I critiqued her subsequent drafts and dickered with the developers. The head of the city's legal department met with us several times and also sat in on some of our meetings with the developers. The ordinance was passed unanimously by the city council and was signed by Mayor Martin Chavez in September 2007.

The moral to this story is that Martin Heinrich knew how the political system worked, went to some pains to meet with developers and listen to their concerns early in the process, knew who the appropriate people in the city hierarchy were to involve in the process, and was determined to pass an ordinance.

The Archaeological Society of New Mexico [ASNM] and The Archaeological Conservancy [TAC]

It has been my good luck to serve on the boards of ASNM (intermittently since 2001) and TAC (since

2003). The two organizations are entirely different in structure and composition, but very similar in philosophy and results. Terms of the officers and board members of ASNM are staggered, but there is always a cadre that provides a corporate memory. TAC's corporate officers and employees are permanent and operate on a year-around basis. Membership on the Board of Directors fluctuates very little. But the organizations are identical in taking their jobs seriously, putting in enormous amounts of hard work, never allowing their focus to drift, and (unlike the innumerable Albuquerque Public School committees I and a lot of other parents devoted fruitless years to) ending up with useful and meaningful results.

Friends, Children and Grandchildren, Canines and Felines, and Solanum elaeagnifolium

My final remarks on the joys of propinquity in time and place relate to friends, kids, animals, and one plant species. I feel fortunate to have a few treasured friends. —And, I feel exceedingly fortunate to have three wonderful kids and seven equally wonderful grandkids, but it's especially rewarding to be able to feel that we're all friends, in spite of being relatives. I'm lucky to live with a sweet-tempered little black-and-white rat terrier, quasi-lucky to live with three not-so-sweet-tempered cats, and not at all lucky to live with a yard full of *Solanum elaeagnifolium* that enthusiastically resurrects itself among the vegetables and flowers as fast as I can kill it. ☙

Figure 5. Family, August 2011. Back row, left to right: Dave Thompson, Kevin Thompson, Erik Stout, Adam Slavin, Antonia Stout. Middle row, left to right: Carla Stout Thompson, Paula Stout Slavin, Dalia Slavin, Rosa Stout, Carol Condie, Lily Stout. Front row, left to right: Lauren Thompson, Jason Slavin, Lauren Williamson (Kevin's fiancée), Carlos Jones (Rosa's husband).

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The Willow Springs Dice: A Complete Set of Basketmaker III Gaming Pieces

BRADLEY F. BOWMAN

A human burial was discovered at the Willow Springs Ranch, near the town of Winston, Sierra County, New Mexico, late in 2005. Archaeological investigations conducted at the site (LA 161968) resulted in the controlled excavation of a prehistoric activity surface and a burial pit that contained the skeleton of a Native American male, between 35 and 39 years of age at the time of death. A probably complete set of bone gaming pieces, or dice as defined by Culin (1975), was recovered in direct association with the remains, as was a marine shell button or closure device. A grant was solicited from the Archaeological Society of New Mexico, who provided funding for the radiocarbon assay of the remains. The resulting lab work produced a date consistent with the area's Basketmaker III period, suggesting the Willow Springs Ranch gaming pieces may be the earliest complete set of these artifacts documented from New Mexico. The purpose of the excavation was to protect the human remains, which otherwise could be targeted by looters. The project design was limited to recovering the remains, documenting the site, and collecting only the data necessary to fulfill requirements dictated by New Mexico law.

THE SITE

Initial work at the site documented a concentration of human skeletal elements eroding from the vertical wall of an arroyo cut-bank directly below a thin dark-stained horizontal lens. Observations and data collection completed during the excavation phase of the project revealed that the coloration of the lens was created as charcoal from a small fire was displaced, integrating into the soils of a prehistoric activity surface. The fire appears to have existed for only a brief interval as the soils surrounding the main concentration of charcoal exhibit little in the way of thermal-induced color change. Cultural materials were nearly absent in the activity surface deposits, as were additional features or other indicators of a long-term occupation. The original location of the fire was in direct contact with the approximate center of the upper margin of a clay-lined burial pit directly overlying the pelvis and lumbar regions of the corpse. It appears that the placement of a small fire over an interment is an intentional component of a mortuary rite (Bowman 2003). The cultural significance of this practice has not been determined.

TEMPORAL AFFILIATION

The project was a volunteer group effort initiated at the request of the New Mexico Office of Historic Preservation as state funding was not available to recover remains on private land. Subsequently, any expenditure necessary to complete the project became the sole burden of the volunteers. The absence of recovered time-sensitive diagnostic evidence demanded the implementation of expensive laboratory dating techniques to determine a temporal

position for the burial and the associated artifacts. Grant proposals to pay for the laboratory test were submitted to the New Mexico Archeological Council and the Archaeological Society of New Mexico (ASNM). The ASNM agreed to pay for the necessary radiocarbon test. A small section of human bone, Sample I.D. (01 Bowman femur sample) was submitted to the University of Arizona's Accelerator Mass Spectrometer Laboratory, for ^{14}C assessment on May 30, 2007. Bone collagen was extracted from the sample and calculated with the ^{14}C half-life of 5,568 years, producing an uncalibrated ^{14}C radiocarbon age of 1373 $\pm$ 45 years B.P. The sample was corrected for isotope fractionation, which generated a $d^{13}C$ value = -10.7. The data was then manipulated with the program IntCal09.14c (Heaton et al. 2009) producing both 1σ and 2σ calibration curves. One-sigma calculations yielded the dates, cal A.D. 619 – 679, too low in probability (p= 68.3) to be reliable, but the 2σ intercept produced dates within a (p=95.4) probability, cal A.D. 586 – 679 and cal A.D. 743 -769. Computer-generated graphics from this data displayed a prominent peak at cal A.D. 658. The calibration curve, Fairbanks0107 (Fairbanks et al. 2005) produced a mean calendar age of cal A.D. 658 $\pm$ 24. These corrected date ranges are consistent with the area's Basketmaker III period occupations.

DISCUSSION

The collection of 12 gaming pieces and a culturally altered marine gastropod were the only documented artifacts in association with the burial. Culin (1975) describes in detail historic period Native American usage of objects similar to the Willow Springs artifacts. These objects, made of bone, wood, or cane, were used as gaming pieces, and for convenience, he defines all Native American games of chance as "dice games." He noted the usage of these objects among 130 tribes belonging to 30 linguistic stocks. Culin (1975:44-45) states, "…from no one tribe does [a dice game] appear to have been absent."

Early specimens of bone gaming pieces in the Southwest have been documented from the Archaic Basketmaker II occupation at the North Shelter site, A.D. 46 through A.D. 260 + near Durango, Colorado (Morris and Burgh 1954). These artifacts occur in elliptical, rectangular, and discoidal forms, usually with one heavily incised flat surface and an opposite planoconvex surface bearing less surface decoration. The North Shelter collection appears to contain possibly three complete sets, and each set appears to have been made by a single craftsman, as is the case with the Willow Springs artifacts. Cougar Springs Cave, a pre A.D. 600 Basketmaker II site in Southern Colorado, produced two specimens (Gross 1986). A partial bone gaming piece was discovered at Ida Jean in the Four Corners area from an occupation dated between A.D. 1050 and 1200 (Brisbin 1973), and four specimens were recovered from the Mesa Verde occupation at Wallace Ruin near Cortez, Colorado (Mathien 1997). Bone gaming pieces have been reported from Pueblo I through Pueblo III sites at Chaco Canyon, and 16 artifacts were recovered from different localities at Pueblo Bonito (Judd 1954). A single gaming piece was discovered at LA 3279 in west central New Mexico. The artifact post-dates A.D. 1275 and exhibits dissimilar morphological characteristics to many of the artifacts previously discussed. This artifact, though rectangular, lacks the incised surfaces commonly seen on the northern specimens (Oakes and Russell 1999).

THE WILLOW SPRINGS GAMING PIECES OR "DICE"

The Willow Springs artifacts were recovered left and lateral to the vertebral column near the point of articulation of the fourth and fifth lumbar vertebral bodies (Figure 1). The position of the recovered artifacts near the individual's waist in association with the gastropod closure device or button suggests that they were interred in a pouch or container. The predominantly stacked characteristic of the concentration indicates that the recovered number of

Figure 1. Concentration of *in situ* gaming pieces.

Figure 2. Line drawing of the Willow Springs gaming pieces (left image, side 1; center image, lateral view; right image, side 2).

artifacts is what was originally interred, and that this number in all probability represents a complete set (Figure 1).

The artifacts were impregnated with dark-colored fluids released during decomposition. Viewed under 60-power magnification, they appeared to be made of wood, as the presence of the decomposition residue highlighted some features of the material's porous structure, at the same time masking other features of the material. One of the damaged objects (Figure 2, i) was submerged in a solvent to remove the body fluids.

The cleaned artifact lost many of the traits that had originally appeared as wood. When examined under 150-power magnification with polarized light, the artifact fragment revealed bone cells (osteons) enclosed in lamellar bone observed as a banded crystalline matrix. These features were compared to the cellular structure of recent turtle plastron, dog, and deer bone samples. These observations revealed that the artifact fragment shares most of the deer bone sample's attributes, strongly suggesting that the Willow Springs gaming pieces were made from large mammal cortical bone.

The twelve gaming pieces can be divided into three basic shapes, three discoidal, eight elliptical, and a single artifact that roughly resembles the profile of a human molar. The entire assemblage shows one nearly flat heavily incised surface (Figure 2, side 1) but the varied treatment of the opposite side (Figure 2, side 2) was apparently governed by the outline of the artifact. The three circular artifacts can be further subdivided by side 2 morphologies into two categories. Artifacts

Table 1. Metric Dimensions of Gaming Pieces.

Artifact Identification	Minimum Width (mm)	Maximum Width (mm)	Maximum Thickness
a	9.69	10.42	2.55
b	10.22	10.65	3.65
c	10.62	10-84	2.6
d	8.75	/*	3.05
e	8.7	17.42	2.85
f	8.81	17.08	2.9
g	/	19.17	2.55
h	9.26	18.73	2.77
i	9.24	/	3.29
j	7.81	16.94	2.71
k	9.33	19.59	2.63
l	12.89	14.93	2.8
* Incomplete specimen; measurement not possible.			

a and b exhibit a dome, but artifact c is relatively flat. Artifact b, side 2, is further embellished with a small shallow depression drilled at the center. These objects are nearly perfectly formed discs, and when each artifact's minimum and maximum widths are calculated, they reveal a mean diameter of 10.4 mm. The maximum thickness of two of these artifacts (Table 1, a and c) is almost identical, 2.55 mm and 2.6 mm; specimen b is considerably thicker at 3.65 mm.

The eight elliptical artifacts (Figures 1 and 2, d through k) exhibit a single incised line on side 2 at both ends of the oval. These artifacts differ only slightly from each other and it appears that they were intended to be alike. Maximum lengths (see maximum width, d through k, Table 1) range from 16.94 mm to 19.59 mm with a mean maximum length of 18.15 mm. Minimum widths vary from 7.81 mm to 9.33 mm, producing a mean minimum width of 8.84 mm. Maximum thicknesses range from 2.55 mm to 3.29 mm, with a maximum mean thickness of 2.84 mm (Table 1).

Artifact l (Figure 2) is unique in profile, both to this collection and in the reviewed literature. Side 1 is heavily incised, as is the rest of the collection; side 2 is smooth and devoid of any decoration. The minimum width of this specimen is 12.89 mm, maximum width is 14.93 mm, and maximum thickness is 2.8 mm (Table 1).

CULTURALLY ALTERED MARINE GASTROPOD

Evidence of marine shell distribution in the Southwest is well documented (Gladwin et al. 1938;

Figure 3. *Agaronia testacea*, dorsal view.

Figure 4. *Agaronia testacea,* lateral view.

Lang and Harris 1984; Mathien 1997; and Nelson 1991). The recovered Willow Springs specimen (Figures 3 and 4) was identified to the species *Agaronia testacea*. This marine gastropod originates from the Panamic Faunal Province (Nelson 1991), which includes the area from the central coast of Baja California southward through the Gulf of California to northern Peru. Nelson (1991:51) suggests, "Some *Oliva* [specimens] reported from Basketmaker sites in northern Arizona, New Mexico and Colorado, may have been examples of *Agaronia.*" The misidentification of these specimens could explain the infrequent reporting in early literature of these mollusks in non-Hohokam sites.

The Willow Springs specimen was altered by removing the posterior (spire) apex portion of the shell by what appears to have been a chipping and grinding process, creating an opening 4.86 mm in diameter. A similar reduction technique was used for the anterior (opposite end), enlarging the opening in the aperture to 4.89 mm. These alterations produced an open passageway through the center of the gastropod suitable for the insertion of a cord, presumably for attaching the artifact to a container that held the gaming pieces.

SUMMARY AND CONCLUSIONS

The skeleton of an adult Native American male was discovered eroding from a vertical arroyo cut-bank on the Willow Springs ranch in Sierra County, New Mexico. The site lacked temporally diagnostic artifacts and it was deemed necessary that an absolute date be calculated from the recovered skeletal material to determine the antiquity of the burial. Funding for the radiocarbon date was solicited from, and provided by, the ASNM. Lab work conducted by the University of Arizona on bone collagen extracted from a small sample of bone produced two reliable series of calibrated dates, cal A.D. 586 – 679 and cal A.D.743 – 769. Two discrete calibration techniques were employed and both produced a mean calendar age for the sample of cal A.D. 658. These dates are concurrent with the area's Basketmaker III occupations.

The remains were interred in a tightly flexed supine position, from an activity surface that exhibited evidence of a small fire placed directly over and in contact with the uppermost margin of the clay-lined burial pit. The significance of a small fire placed directly over the deceased is not currently fully understood. A set of 12 bone gaming pieces, or "dice," was recovered in association with the remains. These objects may be the earliest complete set of these artifacts recovered from New Mexico and the set contains one example with previously undocumented morphologic traits. The artifacts were presumably originally confined in a biodegradable box or pouch that utilized an *Agaronia testacea* shell button closure, evidencing early trade originating at a location somewhere between the central coast of Baja California and Northern Peru and ultimately reaching New Mexico populations. ☙

ACKNOWLEDGMENTS

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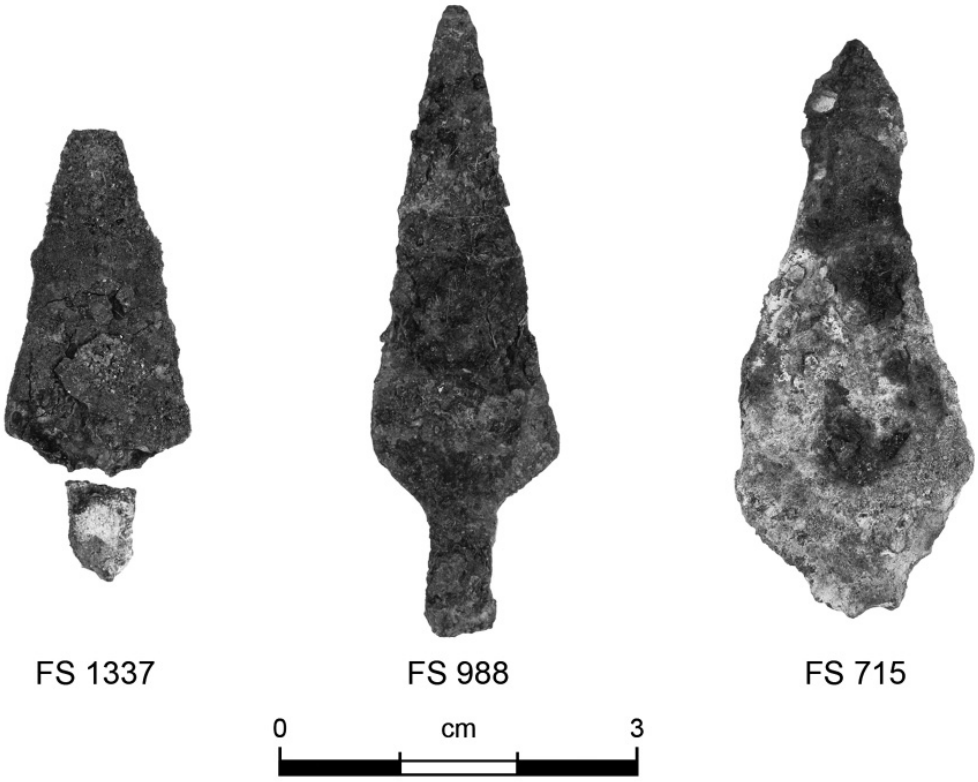
Is There A Point to This? Contexts for Metal Projectile Points in Northern New Mexico

JEFFREY L. BOYER

The *RANCHO* of Don Vicente Valdez, near Cuyamungue in north-central New Mexico, is a large compound of five residential and other structures dating from at least 1828, when Valdez acquired the land, to 1868 when he died, ending occupation of the *rancho*. Archaeological investigations at the site, LA 4968, conducted by the Office of Archaeological Studies in 2002 prior to NMDOT reconstruction of US 84/285, involved a remodeled house, two small circular structures, and several trash deposits. Among the thousands of artifacts recovered are three thin, narrow-stemmed, ferrous metal projectile points (Figure 1). As I investigated the context of the points, it became clear that thin, ferrous metal points are much more common on Native American sites in New Mexico than on Hispanic sites.[1] Further, the only recorded evidence I could find for manufacture of thin metal points comes from Native American contexts.[2] In this venue, I present information for Native American manufacture of thin metal points



in New Mexico and neighboring regions and suggest historical and economic contexts for manufacture that bear upon their presence on Hispanic sites.

EVIDENCE FOR METAL PROJECTILE POINT MANUFACTURE

Eiselt's (2006:288-290) work at Jicarilla Apache sites in the Rio del Oso drainage, northwest of the US 84/285 project area, recovered materials from metal point manufacturing processes, including finished points as well as discarded raw materials (barrel hoop fragments and "clippings"), point blanks, and sharpening stones:

> Barrel hoops were cut width-wise into sections according to the length of a desired point. Barrel clasps and short sections of hoop or strap were discarded. Strap blanks were then cut into point blanks using a cold chisel and hammer. The clippings and other unwanted parts were discarded. The jagged edges of the point were ground off by scoring the edges on a rough stone. Evidence for this stage comes from a vesicular basalt cobble/boulder associated with a rock ring at LA90815 [sic]. When overturned, the surface of the cobble revealed numerous metal gouges. A scatter of metal clippings and several strap blanks were found within a few meters of the cobble.
>
> The final stage of production entailed beveling and sharpening the smoothed edges of the blades with a sandstone tablet. Each of the two identified tablets found in the Rio del Oso were made of locally-available Santa Fe Formation sandstone. These stones were shaped into a rectangular form roughly five by ten or fifteen centimeters and contained multiple working surfaces.
>
> Evidence for metal point production and the distribution of finished points in the valley demonstrates that the Jicarilla made metal-tipped arrows for their own use in hunting and defense as well as for trade with the [Hispanic] residents of San Lorenzo. Exchange in metals was an important component of male interactions (Eiselt 2006:288, 290, brackets added; Fig. 9.3, page 289).

Johnson et al. (2009:35-37) also recovered points, point blanks, and "debitage" (Eiselt's "clippings") from the Jicarilla Apache camp at the Cieneguilla battle site near Pilar, also in north-central New Mexico. They begin a description of the points with a short discussion of metal points manufactured in the English colonies in the late seventeenth and eighteenth centuries for trade with Native groups. That discussion is relevant to the points found at the Jicarilla camp because variation in point sizes and styles found at the site may show that they were not commercially manufactured: "Commercially made trade points can often be identified by their uniform shape and thickness, beveled edges, and consistency in size and proportions. Some are stamped with manufacturers marks" (Johnson et al. 2009:35-36). In contrast to that description:

> It is remarkable that there is little consistency in the size and shape of [the 42] metal points at this site. In contrast, the size and shape of metal points from Mescalero Apache sites on the Lincoln National Forest in southern New Mexico are generally consistent in size and shape. This variation on the Cieneguilla battle site may be a result of the Taos trade fair, where points may have been available from a variety of sources in a variety of styles. It is also possible that a number of different bands were present at the battle, each with their own point style (Johnson et al. 2009:36).

The battle of Cieneguilla, however, took place in March 1854, much later than the famous Taos trade fairs, which do not seem to have continued with any significance following the opening of the Santa Fe Trail (Cunningham and Miller 1999:101). Consequently, variation in metal point sizes and shapes at the camp and battle site (Johnson et al. 2009:cover photo and Fig. 8, pg. 36) do not reflect access to metal through that particular venue. Indeed, the presence of manufacturing debris provides little reason to assume the variation reflects anything outside Apache manufacture. Whether it reflects styles related to band identity cannot be ascertained and is only proposed by the investigators based on apparent contrast with points found at Mescalero sites (e.g., Adams et al. 2000a, 2000b; see Seymour 2002:296).

Along with the points, investigations at the camp and battle site recovered a fragment of barrel hoop, presumed to be a point blank, and an unfinished point (Johnson et al. 2009:36). In addition:

> Fifteen locations of metal debitage were recorded, many forming a distinct cluster of artifacts somewhat between the two ceramic concentrations in the Apache camp. The debitage offers firm evidence that the Jicarilla were making their own points on the site. A number of the debitage pieces were trapezoidal in shape. All appear to have been chisel cut (Johnson et al. 2009:37).

Drawing on materials from Mescalero sites and the Jicarilla site, they interpret these artifacts as follows:

> Small pieces of metal, cut from a barrel band or other piece of metal, are commonly found on Apache camp sites. The scrap metal debitage is primarily the result of point manufacture. The chisel-cut fragments exhibit uneven linear cuts and the lateral margins of the fragments are stretched and uneven. The iron fragments can be long triangular cut pieces or small square or trapezoidal shaped angular fragments. The long triangular pieces represent the areas cut out along the point edges and the small square angular pieces often represent the tang and stem cuts. The shear-cut fragments tend to curl when cut. The shapes of the shear cuts mirror the chisel cuts. These fragments are fairly thick, generally from 1 to 1.5 millimeters, are consistent with the thickness of barrel hoops and metal straps, and are thicker than the debris left from tinkler manufacture (Johnson et al. 2009:36).

Simmons and Turley (1980:36-40) discuss interaction between New Mexican Hispanics and Native Americans involving metal items. They do not, unfortunately, clearly distinguish between tools made by forging (hot working) and those made by cold working, and use both Spanish Colonial documents and nineteenth century ethnographic research in presenting the spread of metal tools and other items, including arrow points, from European to Native American groups and between Native American groups. Interestingly, though, they do point out that archaeological and ethnohistorical research at Wichita sites along the Red River in Texas and Oklahoma shows that Wichitas acquired metal items from Spanish and French traders, some of which they modified for other purposes (Simmons and Turley 1980:37; also B. Westbury, personal communication, 2010). Woodall (1967a:180-183) summarizes these items, and some are illustrated in the report (Bell et al. 1967:Figs. 48-49, 54-58).

Harris and Harris (1967:160-161) specifically discuss metal projectile points from sites examined during the Wichita project, focusing on points from the Longest Site (Bell and Bastian 1967) on the north bank of the Red River and the Upper Tucker Site (Woodall 1967b) on the south side of the Red River. Both sites are assigned to the late eighteenth century, although no chronometric dates are provided. Most of

the metal points from the sites, all made of thin metal, are of the Benton type, a diamond- to leaf-shaped point that is considered the dominant type of the late eighteenth- to early nineteenth-century Norteño Focus (Chandler 1993 and references therein; Harris and Harris 1967:161; Perino 1968:10-11; http://www.texasbeyondhistory.net/st-plains/images/he8.html, accessed October 19, 2010; see also Perino [1971:16-17] for a description of the metal Claremore Point, associated with early nineteenth-century Osage). In addition to the Benton points, stemmed and non-stemmed, triangular, iron and brass points were found at the sites.

Woodall (1967a:181) notes that in addition to a variety of metal artifacts recovered from the Wichita project sites, including the points as well as wedges, axes, awls, scissor fragments, ornaments, and items possibly used as scrapers, there are:

> Chisels [that] are made from small iron rods rectangular in cross section. One end is hammered round and a narrow chisel point fashioned at its extremity. Proximal ends usually show signs of battering. All chisels recovered are rather delicate tools, and were probably used to cut thin brass, sheet iron, or wood.

Simmons and Turley (1980:37) contend that these artifacts found on Wichita sites show:

> that the Wichita along the Red River produced projectile points, awls, scrapers, and other articles from scrap iron. Workshop areas that have been excavated yield hundreds of small metal fragments discarded in the manufacturing process. In practically all cases, it seems, the iron was worked cold. The blades of many Wichita axes and wedges show a profusion of chisel marks indicating that these tools served the Indians as makeshift anvils. Those examples of chisels that have been recovered were made from small iron rods rectangular in cross section. They are rather delicate instruments with a narrow cutting edge and a mushroom of metal on the butt end, caused by hammer blows.

Evidence from the Wichita project points to Native manufacture of thin metal projectile points, apparently in the late eighteenth century, while evidence from the Rio del Oso, Cieneguilla, and Wire Horse Apache sites point to Native manufacture in the mid to late nineteenth century. In each case, artifact production was by cold-working methods rather than forging. Pyszczyk (1999:168) quotes Ewers (1958:122) in providing this description of metal point manufacture among the Blackfeet:

> The Indian arrowhead maker drove an old axe head into the ground and used its top surface for an anvil. He cut out the arrowheads from the hoop iron with a sharp chisel and sharpened their edges with a file. Completed arrowheads were either rounded or square-shouldered . . . They measured about two and one-half inches long.

Pyszczyk (1999:168) also cites Wilson (1918:362) who observed that Hidatsa point-makers "flattened pieces of heated metal on a stone anvil in the tipi to make arrowheads. They cut and shaped the arrowheads with a chisel and hammer." He also cites Densmore (1918:438), who was told by a Teton Lakota that he "cut arrow points from thin frying pans sold by traders or used by the soldiers." These would not have been cast iron pans, which could not be cut, and must have been pans made of sheet metal. While the Hidatsa example apparently involved heating the metal, which would make it temporarily softer and more malleable, this should not be confused with actual forging. Ethnographic descriptions cited by Pysczcyk support the manufacturing processes shown by debris and tools reported at Apache sites by Eiselt (2006) and Johnson et al. (2009), and by authors of

the Wichita project report (Bell and Bastian 1967; Woodall 1967a).

EVIDENCE FOR METAL POINT DISTRIBUTION

Wichita and related people in the southern Great Plains acquired metal items, including points and material to make points, from French traders in the eighteenth century (Harris and Harris 1967). Northern Great Plains groups also acquired metal and points from French, British, and American traders (Birk and Richner 2004; Ferris 1940; Pyszczyk 1999; Russell 1997). In contrast, I can find no substantial evidence pointing to commercially-manufactured points or to Native American manufacture of thin, metal points in New Mexico before the nineteenth century.[3] As Simmons and Turley (1980:22-35) point out, the Spanish government in Mexico attempted to maintain strong control over metal and metal-working in New Spain. Consequently, until Mexican independence and the opening of the Santa Fe Trail, metal was in short supply and unlikely to be available for making projectile points, particularly when materials were widely available for chipped stone tools. In those circumstances, we should not expect to find evidence for manufacture of cold-worked, thin metal points in settings dating before about 1820. I suspect, in fact, that cold-worked, thin metal points were not commonly made in pre-1846 settings in New Mexico, but archaeological evidence is insufficient to test that proposition at this time. Interestingly, though, Herr et al. (2009) do not include metal projectile points among diagnostic artifacts at pre-1850, pre-reservation Apache sites in the sub-Mogollon Rim region of eastern Arizona, although certain chipped-stone point styles are considered diagnostic and Euroamerican artifacts are present at some sites.[4]

Kirkpatrick (2010) describes and illustrates several points and point blanks from the Cimarron area in northeastern New Mexico. He notes that the Cimarron area was used and occupied by both Jicarilla Apaches and Moache Utes, and makes no attempt to assign the Cimarron points to one or both of these groups. Kirkpatrick speculates (D. Kirkpatrick, personal communication, 2010), however, that the points are likely Apache. A metal point was found on a site near Galisteo Spring in Santa Fe County (Pierce 2008) and is used to identify a mid-nineteenth century Apache component on the site. Thompson's (1980) points, mostly from surface contexts in southern New Mexico, southwest Texas, and northern Mexico, are ascribed to Apache manufacture. Metal points, including brass points (and tinklers) apparently made from rifle cartridges, have been found at Carlsbad Caverns National Park; none were in association with Euroamerican sites, and the only Euroamerican artifacts found on the sites were the rifle cartridges (D. Kayser, personal communication, 2010). Seymour (http://www.seymourharlan.com/My_Homepage_Files/Page60.html, accessed August 30, 2011) provides a photo of a thin metal point and barrel hoop fragments found at the Wire Horse Site (LA 61247) near Roswell in southeastern New Mexico. The artifacts may indicate manufacture of metal points or curation of manufacturing materials at the site, which apparently dates after about 1870.

Brown and Rohman (1996:32-33) recovered a triangular metal point from LA 105615 in Grant County, New Mexico. This specimen does not have a stem, although their illustration (Fig. 5.6, pg. 32) suggests that a stem might have been present at one time. Additionally, they recovered a triangular piece of thin metal that may have been a point blank. Brown and Rohman assign the point to an Apache occupation of the site. The type of points recorded in New Mexico by Thompson (1980), Johnson et al. (2009), and Weber (1995) that are identified as Benton Points in Texas and Oklahoma may also point to trade with Native groups living east of New Mexico, such as Comanches, Kiowas, Southern Cheyennes, and Southern Arapahos.

Seymour (2002:296) provides a summary of metal projectile points in the Fort Bliss area that allows opportunity for comments related to econo-socio-cultural contexts:

> The archaeological record suggests that iron points displace stone projectile points sometime in the early 1800s (perhaps even in the late 1700s), although Katz and Katz (1993) argue for the earlier introduction of metal points, for example at LA 48738 in Carlsbad, New Mexico (A.D. 1600 to 1750). By 1882 arrows and spears were rarely used (Betzinez and Nye 1958:5, 85), presumably because of the widespread availability of rifles after the Civil War. To date, few stone projectile points have been identified within later sites (e.g., those dating to the 1880s) and when stone points are present they are often on sites that were occupied for long periods, including prior to the start of the nineteenth century. Accounts indicate that stone points were not made after a certain time; according to Betzinez and Nye (1959:5) 'stone arrowheads had not been made for generations,' presumably not for 60 or so years before the 1870s or 1880s.

The introduction of metal points and in replacement of stone points by metal points by different Native groups was highly variable, and contemporary use of metal and stone points was often a matter of material cost and availability, point function, and tool efficiency (Pyszczyk 1999:167-168). While metal points might have generally been preferable to stone points, replacing stone points with metal points required other technological considerations and was not merely a matter of changing materials. Further, as Scheiber and Finley (2010) point out, the processes of replacing indigenous material items with items acquired from Euroamericans or made from Euroamerican materials involved aspects of socio-cultural transformation other than just material and technology. Seymour continues, "Two metal points have been found on and near Fort Bliss. The smallest of the points may be related to material availability (barrel band size) or to preferences of the local group or individual who made it. The larger point exhibits a serrated stem, much like another point known from Roswell (2002:296)." Pyszczyk (1999) contends that there were differing reasons for creating points that are more and less strongly attached to their shafts; serrating the stem should correlate with attaching the point more strongly to the shaft, which, according to Pyszczyk's research, was related to hunting rather than warfare (e.g., Nordhaus 1995:43).

Weber (1995) describes metal points from several surface contexts in the Rio Abajo region. He notes that, "The points exhibit considerable variation in configuration and probably could be classified into several distinct types, if a sufficiently large collection were available" (Weber 1995:167). As noted earlier, Weber's illustrations include points that are identified as Benton Points in Texas and Oklahoma. Regarding cultural affiliation, Weber (1995:170) states,

> The Rio Abajo lies within the northern fringes of the Apachería, land of the Apaches . . . and near southeastern edge of Navajo country . . . by 1800. Both Athapascan [sic] groups traded and raided into Socorro and the nearby villages and farms. Both of these groups are suspect as bearers of the [metal] points . . . However, a less notorious cultural group may have used metal points. These were the local Hispanic New Mexicans, who for lack of firearms armed themselves with bow, arrow, and lance. This also was true of the organized militia. A review of the militia of the mayorality of Belen on November 4, 1819, states that most members of the 3rd Co. of the 2nd Squadron were armed with gun and lance and 15 cartridges; 7 were armed with bow and 25 arrows, of which 6 also had a lance . . . It may be assumed that the arrows were tipped with metal points.

A similar situation occurred in the Santa Cruz region of north-central New Mexico, when an

1806 list of the militia showed that most members were armed only with bows and arrows (D. Snow, personal communication, 2010). Unfortunately, the documents do not describe the arrow points and there is, *contra* Weber (1995), little reason to assume that militia members and other Hispanic settlers used metal points. Without documentary or archaeological evidence to the contrary, it is just as reasonable to assert that most Hispanic residents of New Mexico, until firearms became ubiquitous in the second half of the 1800s, used arrows with chipped stone tips. This assertion is supported by the common presence of chipped stone projectile points among other chipped stone tools on Hispanic sites in New Mexico (Moore 1992, 2004, in prep.) and the apparent paucity of metal points on Hispanic sites. Moore (in prep.) provides a detailed discussion of Hispanic contexts in which chipped stone projectile points have been found and the characteristics of those points that distinguish them from points found on Native American sites and that indicate Hispanic rather than Native manufacture. It is also reasonable to contend that Hispanic settlers acquired chipped stone artifacts, including projectile points, by scavenging from archaeological sites and by trading with Native American neighbors (J. Moore, personal communication, 2010).

CONTEXTUALIZING METAL PROJECTILE POINTS

This review of thin, cold-worked, metal projectile points in the archaeological record indicates that the three found at LA 4968 may be among the few recovered, to date, from a Hispanic site in New Mexico. I suspect that this is not, in fact, the case, but it does show the relative paucity of these artifacts in non-Native settings. Following Eiselt (2006), I propose that the points found at LA 4968 represent an aspect of interethnic economic relations in the nineteenth century in north-central New Mexico, and that they were acquired through trade between Hispanic settlers and neighboring Native Americans, probably Jicarilla Apaches, given the proximity of the US 84/285 project area to mountain areas that were home to the nineteenth century Jicarilla.

Eiselt (2006; personal communication, 2010) contends that the metal points and manufacturing debris found in the Rio del Oso represent male-oriented relationships within interethnic economies. As such, they point to activities that complement female-oriented, interethnic relationships represented by, for instance, exchange of ceramic materials and vessels. Her position raises questions of point form and function that Pyszczyk (1999) addresses. For instance, Pyszczyk (1999:168-169) cites differences in early nineteenth century Omaha metal point hunting and warring functions that could be identified by point form, particularly the presence of barbed edges. Catlin (1973, cited by Pyszczyk 1999:169) identified similar differences for other Plains groups. Because Pyszczyk is primarily interested in technological transitions involving the contemporary and sequential use of metal and chipped stone points, questions of form and function lead him toward examinations of arrow and bow construction and efficiency rather than implications of metal point form and function within contexts of intra- and intergroup socioeconomics—although warfare might well be considered an aspect of intergroup socioeconomy (Brooks 2002). It is reasonable, nonetheless, to consider those contexts when examining metal points in the Southwest. While I can find no specific examinations of Jicarilla Apache metal point forms and functions, it is relevant to look at such points in light of Pyszczyk's data on arrow construction and use in order to question the functions of points found in isolated situations and on sites, both Native American and Euroamerican. A handful of points from a few Hispanic sites in this project area and in the Rio del Oso do not provide us with sufficient data for functional comparisons, particularly given the paucity of information on Apache point functions, but they do allow us to wonder about roles played by point forms and functions that were involved in Apache-Euroamerican exchange relationships.

Scheiber and Finley (2010) provide a socio-cultural interpretive context for technological transitions that included use of metal arrow points among Mountain Shoshone. Specifically, for their examination of "ethnotechnological" variation at Shoshonean sites, they point out:

> This mixture of indigenous and introduced items could be seen as representing an increasing use of new materials coinciding with a gradual abandonment of old ones, with ratios of objects seen as proxy measures for estimating temporal placement and culture change. This older acculturation model is still generally accepted today: 'In most cases foragers were understandably eager to obtain manufactured goods such as steel axes, knives, needles, metal pots, guns, and cloth that originated in the industrial core of the world system . . . because they made so many daily tasks easier' (Bodley 1999:468). Recently, Rodriguez-Alegría (2008) and others have questioned the model of quick replacement of indigenous with European technologies, calling for new narratives of change that emphasize other social, economic, and political factors. Likewise, Silliman (2010) underscores that fact that all materials used by native peoples become indigenous even if they were originally produced for European or American consumers. These objects and the way they were used were incorporated into broader social spheres and imbued with meanings and functions by the native peoples using them.

While their study examines cultural contexts for adoption and adaptation of Euroamerican materials by Native peoples (see also Silliman 2005, especially pp. 282-283 and references therein), their point is that material transfer involves active decision-making on the parts of both parties and that, consequently, transfer transforms both parties. Rodríguez-Alegría (2008) argues that material and technological transfers are not always rapid or "practical" (likely a highly variable ethnocentric concept, in any case, and one filled with dynamic cultural specifics), and that they require new examinations and creation of new, non-Eurocentric narratives of material aspects of ethnic interaction.

We should expect, then, that the presence of metal projectile points on Hispanic sites represents more than mere exchange. If the LA 4968 points were made by nearby Apaches, probably men (Eiselt 2006), they show 1) movement of "raw material" (metal items) from Euroamerican to Native American peoples, and potentially between Native American peoples, involving gender-defined exchange of items that likely embodied different cultural meanings at the time of exchange; 2) use of those materials in ways that do not reflect their original (i.e., Euroamerican) functions or meanings, involving culturally-defined, probably gender-specific, manufacture and use that not only altered the materials but changed their culturally-defined functions; and 3) return movement of finished tools from Native American to Euroamerican peoples representing additional gender-defined exchange that also embody differing cultural meanings. These issues are, of course, not limited to metal projectile points; by transforming Native American pottery manufacture and style for exchange with Hispanic settlers, for instance, Native American potters transformed the meanings they gave to vessels and styles while Hispanic consumers embodied those vessels with uses, values, and meanings taken from their own cultural foundations. Likewise, the controversial topic of Hispanic pottery manufacture or pottery manufacture in Hispanic communities raises issues of material markers and meanings of cultural transformation.

CONCLUSIONS

The three thin, ferrous metal projectile points recovered from LA 4968, the *rancho* of Don Vicente Valdez, reflect a number of impacts from events and subsequent processes related to U.S. presence in New

Mexico, beginning with the opening of the Santa Fe Trail in 1821 and increasing dramatically with the U.S. occupation of New Mexico beginning in 1846. Economically, those events suddenly increased access, on the part of both Native American and Euroamerican New Mexicans, to wider and wider varieties of manufactured goods. In turn, those goods were incorporated into and modified existing settings of intra- and interethnic technology and manufacture, use, and exchange. Processes of incorporation and modification were both "practical" and symbolic, and place metal—and chipped stone—points in the same evolving econo-socio-cultural contexts as Native American and "Hispanic" pottery, domesticated plants and animals, and even building materials and construction methods such as adobe bricks and window panes. Those evolving contexts present the most interesting aspects of Euroamerican artifact analyses because they move us beyond static typology and function into dynamic cultural and ethnic interaction, alteration, and development. ☙

ENDNOTES

1. As noted later in this paper, I am not concerned here with metal points manufactured commercially, apparently in great numbers, for exchange between Euroamerican traders and Native Americans. I am also not concerned here with crossbow bolts or forged point or lance tips found occasionally in Exploration Period (ca. A.D. 1540-1598) contexts.

2. Benjamin Titsworth, a blacksmith in Las Animas County, Colorado, forged several hundred arrow points for a band of Comanches who were camped on his land in the 1870s or 1880s (M. Church, personal communication, 2010). Since the points were forged rather than cold-worked, Mr. Titsworth's efforts are not considered to be an exception to this statement.

3. As noted later in this paper, Katz and Katz (1993, cited by Seymour 2002:296; see also Katz and Katz 2006:28-29) contend that LA 48738 near Carlsbad, which yielded a metal projectile point, dates before A.D. 1750. The age is, however, based on a radiocarbon date that does not actually fall within their Ethnohistoric 2 Period (ca. A.D. 1600-1750) (Katz and Katz 2006:29), casting doubt on dating the site to the eighteenth century.

4. It is, however, entirely possible that metal used to make projectile points was unavailable or minimally available in the sub-Mogollon Rim regions before 1850.

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Navajo and the t-/k- Isogloss

DAVID M. BRUGGE

The clearest phonological division, or isogloss, between the eastern and western Apachean languages is the shift from western Apachean stem-initial t- to eastern Apachean stem-initial k-,[1] sometimes referred to as the t-/k- isogloss, not an entirely accurate description as the shift in Kiowa-Apache holds true only before –a and –o, becoming ch'- before –e and –i. Otherwise t- becomes k- in Jicarilla Apache and Lipan Apache and remains t- in Navajo, Western Apache, Chiricahua Apache and Mescalero Apache. (Young 1983 following Hoijer 1938)

These changes are paralleled in part by several other consonants, but quite irregularly so. Most changes are found in Kiowa-Apache, clearly suggesting that Kiowa-Apache differentiated considerably earlier than the separation of the other languages from each other. The split between the others had to be later, but the division must have been early enough that the westerners retained the stem-initial t- while the Jicarillas and Lipans shared the stem-initial k-with the Kiowa-Apaches. A scenario that explains this change seems difficult to visualize, although the most likely might be continued but looser contact between the Kiowa-Apaches and other Apaches on the Great Plains. The proliferation of Spanish names for Apachean groups on the Plains prior to the Comanche advance in the eighteenth century seems to indicate that the major ancestral branch of the westerners split relatively early, probably following bison across the Rocky Mountains into western Colorado and Utah. The stem-initial t- is the ancestral form and is widespread among the Northern Athabaskans. (Brugge 2008)

In 1974 Muriel Saville-Troike published a paper in which she described nineteenth century word lists in an attempt to gain historical insights into language change among the Southern Athabaskans. She had, of course, to deal with transcriptions by many persons unfamiliar with the sound systems that occur in Apachean languages and whose attempts to render these in the English alphabet varied in many ways among themselves. Only one, Albert S. Gatschet, had received instructions from the Smithsonian Institution and demonstrated some sophistication in his recordings. In addition, the dialects of their first languages influenced how they perceived some of the Apachean sounds. (Saville-Troike 1974:68-69)

She used a substantial number of vocabularies, mostly in the form of word lists, but a few dictionaries. She had 14 Navajo word lists and 10 Apachean lists divided among several tribes and bands. The three dictionaries were from the twentieth century. In addition she had data from her own observations and those of Richard O'Brien to draw on. (Saville-Troike 1974:83-84)

A further complication was the depth of knowledge of the Apachean languages of the various informants. Some were native speakers, but others included people who were or had been captives among the Apacheans for various lengths of time.[2]

A number of the changes over time that she describes are simple semantic changes in usage preferences, all with relation to Navajo. Some of her

data on recent usages come from observation of the speech of Navajo children, both in elementary school and teenage informants (Saville-Troike 1974:70-72, 83, n.2). Most of her presentation, however, is devoted to phonological change. She deals with both intertribal distributions and intratribal variations over time and space. Most significant is her questioning the validity of the t-/k- isogloss in its relationship to Navajo and her proposal to include several other phonological changes in the description of the isogloss. (Saville-Troike 1974:74-82)

It is not easy to follow all of her descriptions in terms of her methods of orthography and determinations of locations. With regard to the latter, she specifies the tribe, group or band by abbreviations in her tables, but the ordering of these confuses the reader as to which tribe the band or group names relate. For instance, Cop, Coppermine,[3] is a band within Ch, the Chiricahua Tribe, while PL, Pina Leño [sic] and SC, San Carlos, are Western Apache groups along with WM, White Mountain, but none of these are placed together in the tables. Pina Leño might refer to either the San Carlos Group or the White Mountain Group (Goodwin 1942:2). Only J, Jicarilla, L, Lipan and KA, Kiowa-Apache are in a logical order[4]. (Saville-Troike 1974:73-74, 77)

In her analysis of the t-/k- isogloss, she finds no use of stem initial t- in Jicarilla, Lipan, or Kiowa-Apache. In the western Apachean languages she finds two instances of the use of stem-initial k- in Mescalero, one instance in the dialect of the Eastern Band of the Chiricahuas and only two for the Western Apaches, these one each for the White Mountain Group and the group sometimes called Pinaleño in the nineteenth century. There are no data for the three unmentioned Western Apache groups, the Cibecue and the Northern and Southern Tonto. This amounts to a total of five instances in the 10 samples from these tribes. In the 14 samples from the Navajos there were four reports for the k- usage involving two words, "three" (*taa/kaa*) and "turtle" (*istyel/esket*). (Saville-Troike 1974:74)

The k- usage in Navajo was found in sources dated 1865, 1868, 1870, and 1874. The two earliest dates are said to have been collected "east of the Lukachukai Mountains" (Saville-Troike 1974:75), but under what circumstances is not mentioned. In 1865 most Navajos were in exile at Ft. Sumner as they were for the first half of 1868, but in the remainder of 1868 they were receiving rations first at Ft. Wingate and then at Ft. Defiance as they had just returned from exile and were largely without resources for living again in their old country. There were also Navajos, probably a substantial number, who escaped captivity and successfully hid, most in the far western reaches of their own country or even beyond (Iverson and Roessel 2002:51-67).

It is noteworthy that all evidence of the k- usage among the Navajos occurs during and shortly after the exile at Ft. Sumner. The assertion that this was the primary usage among the Navajos in all the vast extent of their range east of the Lukachukais up until the late nineteenth century rests on only two sources.

The other phonological changes that Saville-Troike (1974:78-79) would group with this asserted Navajo k- usage are y/z, gh/w or y, o/u and what she terms consonant and vowel harmony within morphemes.

The y/z rule is illustrated best by the long recognized Navajo use of *yas* in the west and *zas* in the east for "snow." In her sample of six words she finds at least some y- use in all Apachean languages but Kiowa-Apache while z or zh is also the standard or alternate choice in all but Kiowa-Apache and Mescalero Apache. There is really no distinct pattern in these usages contrasting eastern and western Apachean (Saville-Troike 1974:73-75). The data for gh/w or y have an equally unimpressive distribution based on a sample of only three words. Since locations within Navajo country are given for few of the Navajo records, it is probable that there are no significant patterns either temporally or geographically (Saville-Troike 1974:75-76). There does seem to be some sort of east-west difference in the o/u usage among the Navajos at present, but with sufficient exceptions and overlap in her distributions that it cannot be defined in any precise terms, a typically Navajo "fuzzy" pattern that

defies easy characterization (Saville-Troike 1974:77-78). The tendency among the Navajos to regularize consonant and vowel harmony does seem to increase through time, but it does not seem to have followed this development in the other Apachean languages, at least with regard to the consonants (Saville-Troike 1974:78-81; see also Bray 1998 and Phone et al. 2007).

Variations in observed speech in Navajo can be the result of several causes: formal versus informal speech, change to fit into a song, archaic forms—especially in religious discourse, generational differences, idiosyncratic habits, and, of course, localized dialects or usages, not to mention speech by persons for whom Navajo is a second language. The flexibility that I have casually noted of the phoneme n from enhancement to syllabic status to reduction to nasalization of an adjacent vowel, especially in the varied ways Navajos pronounce names with historic depths, such as names for sacred places (*Dibé Nitsaa* versus *Dibéntsaa*, "Big Sheep Mountain") and older clans (*Kin yaa' áanii* versus *Kįiyaa'aanii* "Towering House People") is a good example of this fabled fuzzy quality.

Overall there is some "fuzziness" in the differences between the Apachean languages that blurs any easy historical description. This is true also among the Northern Athabaskans to the extent that "relationships...cannot be adequately described in terms of discrete family tree branches, [which has been attributed to] the fact that these languages are not isolated but surrounded by related dialects. Diffusion from various directions was always possible" (Krauss and Golla 1981:68).

In the north, particularly in the boreal forest and on the barren grounds, resources were limited and undependable, making starvation a very real danger (Rogers and Smith 1981:135-137; Van Stone 1974:27, 31). It was important to be able to rely on neighbors in times of need and to be willing to help as well. I believe this orientation toward taking in the starving has survived among the Apacheans and accounts for readiness to accept outsiders that led to the adoption of many foreign clans by the Navajos and in close relations with other tribes such as the Kiowa-Apaches with the Kiowas, the Navajos with the San Juan Paiutes and the Tontos with the Yavapais. This may also be as much a factor as any other behind the perplexing distribution of phonological and semantic usages among the Apacheans today, including the sporadic violations of the t-/k- isogloss.

I do not doubt that there may have been in the past and perhaps even today be a few speakers of Apachean dialects differing from Navajo dialects among the Navajos, such as might be attributed to spouses from other Apachean tribes, captives taken from those tribes or even present as refugees escaping persecution or captivity among colonial powers or accusations of witchcraft among their own people.

I need to conclude with the fact that having worked for many years recording place names, clan names, and personal names, many of which incorporate *to',* "water," or *tábąąh*, "shore" or "water's edge," throughout all parts of Navajo country in the course of writing descriptions of Navajo archaeological sites and interviewing Navajo elders on oral history, without any of these names using a stem initial k- for either of these two words, I immediately doubted the contention that the dividing line for the t-/k- isogloss is or ever had been at the Lukachukai Mountains. A great many of the personal and place names I recorded were obviously of considerable antiquity and as such it might be expected that an older form could be preserved.

There is, of course, the possibility that there once existed among the Navajos a community or local group of some sort where the stem initial k- was in use, just as there does today among the Chipewyan (Krauss and Golla 1981:80) but the data that we have today are far too sparse to allow us to do any more than hazard guesses as to dialect differences within the tribe prior to 1900. ☙

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ENDNOTES

1. The t and k in this case are the heavily aspirated stops, not those unaspirated which have been variously transcribed as t or d and k or g respectively.

2. One, the informant for the Whipple Navajo word list was a Mexican who had spent only six months as a captive of the Navajos (Gordon 1988:136, n.14, 230).

3. Coppermine is an old name for the Chiricahua Eastern Band (Opler 1941:1).

4. There are discrepancies in some of the words used for comparisons. For example, due to typographic errors and poorly understood orthographies, "seven" is included in Table II, but it does not have aspirated t or k.

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Revisiting the Hopi Sash

GLENNA DEAN AND CAROL JAMES

THE HISTORY OF "REVISITING THE HOPI SASH"

Glenna Dean was the New Mexico State Archaeologist for 11 years and just retired from her position as Executive Director of the Northern Rio Grande National Heritage Area, based in Española. She worked for many years as an archeobotanist, combining pollen analysis with a love of artifacts made of fiber. She is self-taught in many textile techniques. Glenna met Carol at a major textile arts convention in Albuquerque where her demonstration of fingerweaving made so much sense that when she began writing *Sprang Unsprung*, Glenna talked her into investigating the 3/3 circular-warp sprang technique of older Hopi sashes. Carol's experiences with this unusual kind of sprang made light bulbs go off in both heads. Science should always be this much fun!

Carol James grew up in an environment that valued textile arts. Now a skilled textile practitioner, she has found channels for perfecting her art. She is particularly fond of off-loom techniques, in part because of their portability. A former nurse, she found a different place in the local hospital through Manitoba Artists in Healthcare promoting de-stressing by weaving. Participation in re-enactment groups led her to research, teaching, and the publication of *Fingerweaving Untangled* and *Sprang Unsprung*. Her days are filled re-creating historic pieces and promoting a modern understanding of ancient textile arts. Anywhere is a good place for her to sit and weave and infect others with her interests. Indeed this is the way she met a certain former New Mexico State Archaeologist…

SPRANG

"Sprang" is an ingenious textile technique—more than braiding, not quite weaving—that can be used to produce bags, head coverings, shirts, gloves, stockings, mittens, hammocks, belts, sashes, scarves, and baby slings (Collingwood 1974; James 2011; Skowronski and Reddy 1974) on either a figure-eight warp or a circular warp (Figure 1). Produced with hand movements much like braiding, sprang uses no weft. Unlike braiding, sprang is specifically produced on warps with fixed ends. This seemingly minor distinction underlies a fundamental difference in the way sprang textiles are produced as opposed to braided or fingerwoven textiles (James 2008).

In sprang, transposing individual warps from one position to another in a row makes stitches that are simultaneously mirrored in two opposing rows because the warp ends are fixed (imagine a rubber band stretched between thumb and forefinger; give a half-turn to a pencil inserted between the front half and the back half to create two opposite twists). One of these rows is above the area in which the work is being done and the other is below. Slender sticks, serving as temporary wefts, hold the stitches in each row and are moved (more accurately, shoved) in both directions away from the work area when all stitches in a row

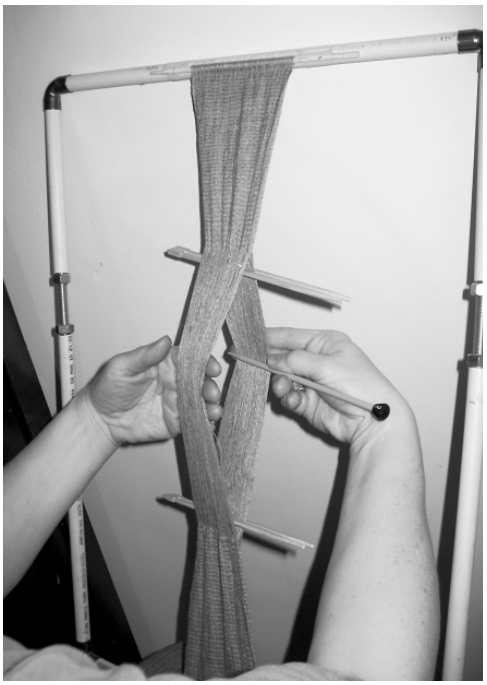

Figure 1a. Figure-eight warp; cloth "grows" from each end to the middle. Photo by Carol James.

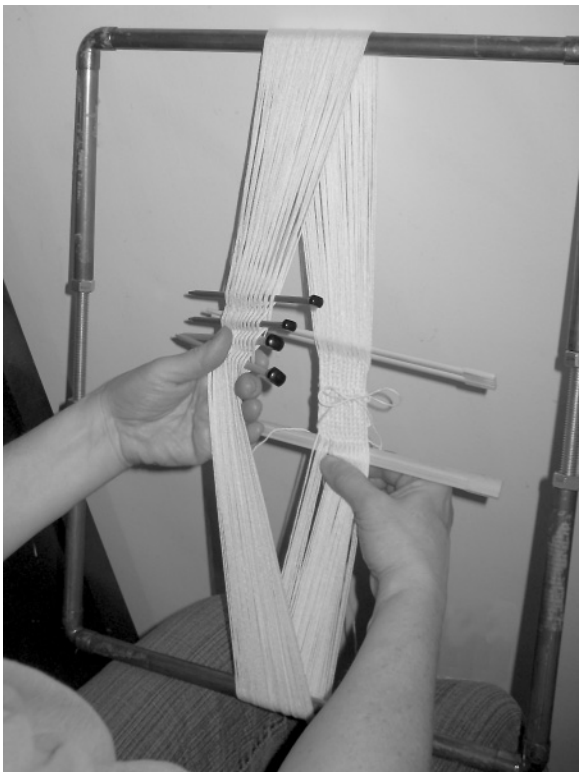

Figure 1b. Circular warp; cloth "grows" from the middle (bow at back) toward the ends as sticks holding rows are shoved up around the top bar and down around the bottom bar where chopsticks secure the cloth between them. Photo by Carol James.

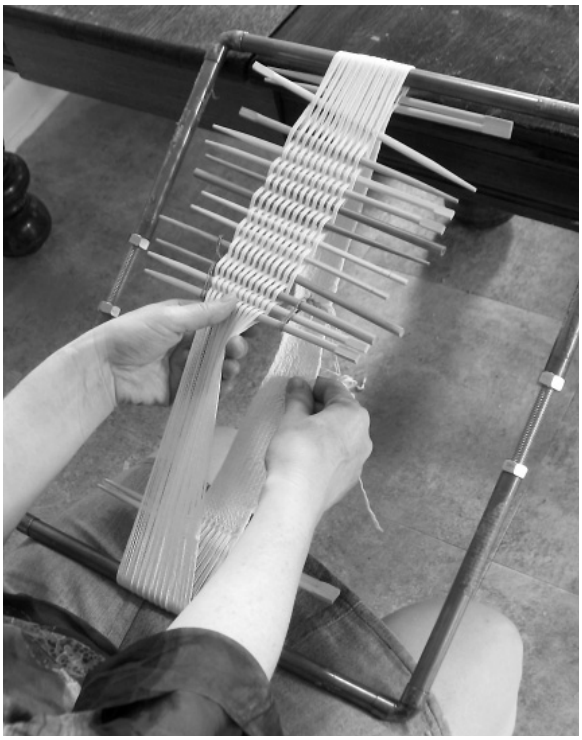

Figure 2. 3/3 herringbone sprang underway on circular warp; note cloth accumulated at back (worker's right hand). Photo by Carol James.

are made. Each row locks the stitches in the previous row, such that the holding sticks can be removed in succession as the cloth accumulates (Figure 2).

Interlinking, interlacing and intertwining are all techniques that can be used with the mirror-image textile technique, sprang (Collingwood 1974:31). Sprang textiles—featuring a pattern very similar to a chain-link fence have a long history in Europe and Colonial America and Canada (Collingwood 1974; James 2011; Skowronski and Reddy 1974). Sprang textiles are also present in North and South American archaeology (D'Harcourt 1974:79-82; Hurley 1979:112, Fabric Number 13 [compare with James 2011:45]; James 2011:35; Kent 1957: 603; Teague 1998:79-82; Teague 2000:163, Figures 9.4 and 9.5). Interlinking (looks like chain-link) and interlacing (looks like basket weave) structures are the primary focus of these authors. "Stitches" can be manipulated to produce patterns, letters, and numbers (Figure 3). As seen in Figure 4, the weftless fabric is very stretchy widthwise, enabling it to conform to the body

Figure 3. Replica silk sprang sash by Carol James showing some of the design possibilities of chain-link. Photo by Carol James.

Figure 4a. Non-Commissioned Officers in British and US Army wore wool sashes of chain-link sprang construction well into the 1800s.
Replica sashes by Carol James. Photo by Carol James.

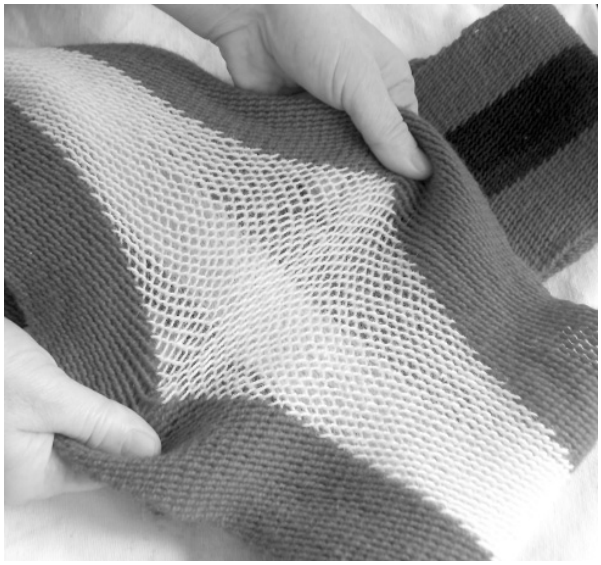

Figure 4b. Lateral elasticity of sprang textile.
Replica sash by Carol James. Photo by Carol James.

Figure 5. Meeting-line in middle of plain chain-link sash; note reversal of stitches. Photo by Carol James.

Figure 6a. Hopi-style 3/3 circular-warp sprang sash, purchased by Glenna Dean from Oke Owingue Arts and Crafts Cooperative, San Juan Pueblo, New Mexico.
Photo by Carol James.

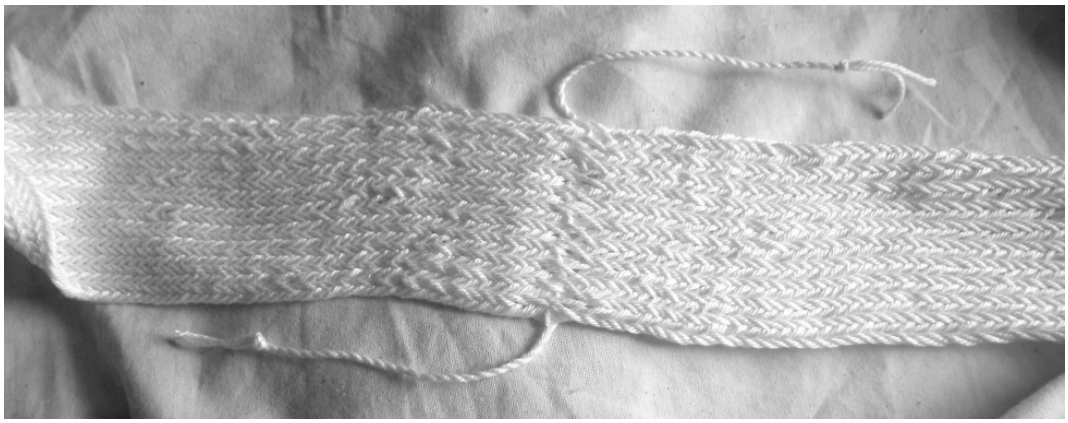

Figure 6b. Meeting-line holding string elaborated as a tassel.

comfortably. The sprang textiles in Figures 3 and 4 all have the characteristic middle meeting-line (Figure 5), either side of which the patterns (if present) appear in mirror image. It is this middle meeting-line that is the hallmark of fabrics constructed using the circular warp sprang technique.

The wide, "weftless-woven" Hopi Rain or Wedding sashes (Figure 6) collected and described by Kent (1940) and Underhill (1944, 1948) exhibit a unique 3/3 braided pattern (interlacing) that creates the stretchy fabric characteristic of textiles constructed without a weft. (A 3/3 is a pattern in which threads travel over three threads, then under three threads.) This interlacing pattern can be made by free-end braiding or more easily with fixed-end sprang. Indeed, the Hopi sashes were constructed using the circular sprang technique (Kent 1983a:70-75; Kent 1983b:82-85; Webster 2000:195-196).

In the Hopi sash, the starting row is visible as a meeting-line in the middle of the finished cloth (Figure 7) and prevents the rows of opposing stitches from undoing themselves. The outer ends of the sash are finished with tassels and long fringes (Figure 6a). Construction techniques of the elaborate tassels might date back to the fourteenth century at Bear Creek, far to the south of Hopi (Kent 1957:579, 597). The meeting-line in the Hopi sash is the giveaway that the construction technique was sprang, not free-end braiding. To compare and contrast, see James (2008, 2011).

The Hopi sashes were made on a horizontal frame dressed with a circular (tubular) fixed-end sprang warp wound around the two end poles (photographs and Huber's drawings in Kent 1940 provide excellent details on all aspects of sash production). On the circular warp, fabric can be made on the upper layer of warp and pulled around one of the two end poles to advance new warp, uniquely allowing a sprang textile to be made in half the space (a figure-eight warp must be as long as the textile being made—a

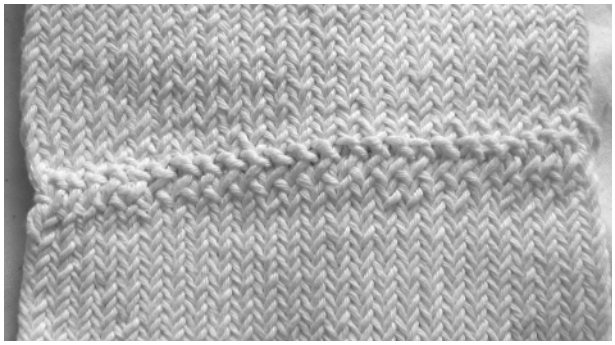

Figure 7a. Hopi-style 3/3 sprang circular warp sash, meeting-line, one face.

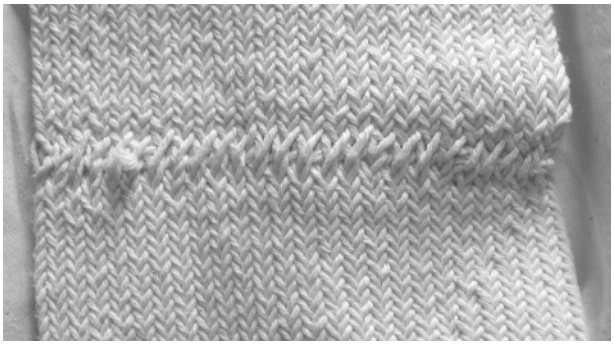

Figure 7b. Hopi-style 3/3 sprang circular warp sash, meeting-line, other face.

substantial consideration when a fringed sash is to be some 10 feet long!). Simple frames for twining and other techniques were present in the Southwest before A.D. 700 (Teague 1998:116), and complex fabrics were produced on them by means of free-end braiding (Kent 1957:586, 597-600). By A.D. 1100 on the Colorado Plateau, weaving on the vertical true loom produced fabric alongside textiles made on frames (Teague 1998:112, 113, 123, 181, Figure 8.5). Teague (1998:123) suggests that the vertical true loom might have been a natural extension of "a locally familiar vertical support framework." It would not be unreasonable to suppose that a "vertical support framework" had also been used for sprang warps.

So far in this discussion, we have a sash braided on a space-saving circular warp that additionally makes two rows of stitches at a time, one above and one below the work area, leaving a tell-tale meeting-line in the middle. Elasticity (Figure 8) is built into the sash because of the nature of the weave, and the absence of a weft. What are the origins of the 3/3 herringbone pattern?

Figure 8. The 3/3 herringbone pattern, worked as a braid or as sprang, produces a cloth that is both dense and elastic. Photo by Carol James.

Webster (1997:232-233, 292-294; 2000:190-192, Table 10.2) reports archaeological examples of "3/3 oblique interlacing," possibly sprang, worked in cotton from Hopi and worked in yucca from Zuni Pueblo from contexts dating between A.D. 1300 and 1600. Significantly, archaeological examples of 3/3 oblique interlacing were absent from contexts dating between A.D. 1300 and 1850 from Rio Grande pueblos, Pecos Pueblo, and the Salinas pueblos (Webster 2000:Table 10.3). The 3/3 oblique interlaced bands of agave or cotton reported by Webster show the same herringbone pattern as the 3/3 sprang of the Hopi sashes, but the preserved fragments lack a center line (the hallmark of fixed-end sprang) and could have been produced by 3/3 free-end braiding (Kent 1957:593-595 and Figure 105) or fixed-end sprang. Kent (1957:579-581, 583 and Figure 96A) describes a tie made in 2/2 "counterpaired warp-twine," a technique that takes advantage of a fixed-end warp on a two-pole frame but where elasticity is sacrificed. Instead, sticks hold half of the transposed warps in a herringbone design until a *separate weft* is inserted as each stick is withdrawn (see also Collingwood 1974:Appendix 2, last paragraph).

The 3/3 herringbone pattern, worked as sprang, appears from present evidence to be limited to Hopi and Zuni. Similarly limited might be the use of a circular warp for *sprang*. "No Mesoamerican precedents or parallels exist historically or prehistorically for [the] vertical loom" (Teague 1998:113). This means the circular sprang warp and 3/3 sprang pattern might be yet another unique regional invention of the pueblos of the Colorado Plateau.

Hopi-style sashes are now largely woven on true looms. Weaving with a weft exchanges horizontal warp-faced weave for the 3/3 sprang pattern, but warp floats create diamond patterns that echo the original visual texture of herringbone. Some woven sashes incorporate color along the edges or in a central band. This unique textile, wide with long fringes and elaborate tassels, is now worn in pueblos throughout New Mexico in updated form.

SUMMARY

Textile fragments exhibiting a 3/3 herringbone pattern have been found at several archaeological sites in the American Southwest spanning several centuries. It is a distinctive and uncommon textile structure that can be produced by several methods. Whatever its antecedents, the 3/3 herringbone sprang sash with its center meeting-line and long complex fringes is a textile unique to the Hopi people. Without using a weft, it employs a specific technique that produces two mirror-image rows for every row of work, a clever but almost forgotten method of textile production worthy of greater attention. The distinctive middle meeting-line in older-style Hopi sashes demonstrates that *circular warp sprang* was known on the Colorado Plateau, perhaps the only place in North American history.

The sprang technique seems to have been very well known all across Europe in the 1700s and early 1800s and then precipitously fell into oblivion. Collingwood (1974:35) summarizes the history of sprang: "Although it is now realized that sprang has a wide distribution both in time and space, at the end of the last century it seems to have been unknown to European textile specialists." Knowledge of even the chain-link version of sprang was nearly lost between 1820 and 1880, and was rescued by archaeological finds. Display of a Bronze Age hairnet and its exact copy in chain-link sprang aroused considerable interest at the 1889 Paris World Fair, the result of a student's analysis of the ancient artifact and recreation of the basic technique (Collingwood 1974:35-36).

Looking ahead, sprang fabrics worked on a circular warp in the 3/3 herringbone pattern have an uncertain future, as the utility of sprang's remarkable density and elasticity appears only occasionally in indigenous sashes and perhaps no other textiles. Archaeological evidence could establish that 3/3 circular warp sprang developed independently in North America. Toward that end, researchers are encouraged to familiarize themselves with the method by trying the 3/3 herringbone method for themselves. A ball of string and a couple of straight sticks are all that are needed to follow instructions on setting up the circular warp and commence the pattern (James 2011:68-69). A half hour spent exploring this technique will open doors to understanding and admiration of this ingenious weftless structure. You must know what you are looking for to find it. ☙

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Isabel Kelly Among the Southern Paiute: 1932 to 1934

CATHERINE S. FOWLER

Isabel Truesdell Kelly (1906-1983) had a distinguished career in anthropology that spanned 55 years of concentrated work as an ethnographer, archaeologist, and applied anthropologist. An avid field worker, often in rigorous situations, she pursued research topics in several locations in the western United States and Mexico as well as in Colombia, El Salvador, Ecuador, Chile, Puerto Rico, Bolivia and Pakistan. Trained as an academic anthropologist at the University of California, Berkeley, she entered the field at a time when women were beginning to seek and expect full recognition of their professional abilities. Although fulltime academic work eluded her, perhaps in part because she chose to live in Mexico, she was consistently employed in a professional capacity throughout her career. Her body of work is impressive, a legacy to her determination to be an anthropologist and to work in interesting places and on interesting problems of her choosing (Fowler and Kemper 2008:139).

Here I focus on an early aspect of her career, her fieldwork among the Southern Paiute people of southern Utah, northern Arizona and southeastern California. Southern Utah is familiar ground to this year's honoree, Carol Condie, as both of her parents came from the small community of Santa Clara in its southwestern section. Another connection to Kelly for Carol is that she edited Kelly's monograph while working for the Glen Canyon Archaeological Project at the University of Utah (Kelly 1964). The story that follows, related largely in Kelly's own words from her letters from the field,[1] is one of a "plucky" and independent person doing the job she set out to do to the best of her abilities—something that is also characteristic of our honoree. Both also place great value on collecting deeply descriptive and detailed data through fieldwork and making them central to any analytical and theoretical statements.

SOUTHERN PAIUTE FIELDWORK, 1932

In February, 1932, while engaged in post-doctoral ethnographic fieldwork among the Coast Miwok in California, Isabel Kelly received word that she had been granted a fellowship in the Biological Sciences by the National Research Council for a one year study titled "A Determination of the Ethnic Groupings of a Great Basin Tribe, with Special Reference to the Geographic and Subsistence Bases" (Robbins to Kelly, February 9, 1932, KADL). This project was an outgrowth of her recently completed dissertation, "The Fundamental Basis of Great Basin Culture," which was a comparison of the subsistence systems and adaptations of Great Basin peoples based on the extant literature. The fellowship project was designed to gather specific field data on the relationship of cultural to environmental variation, a topic Kelly had explored in her dissertation and as a graduate student in courses from mentors A. L. Kroeber and Carl O. Sauer at the University of California, Berkeley. The orientation for this project was to be specifically

ethno-geographical and to involve gathering detailed field data on boundaries, settlements, economic cycles, and the utilization of natural resources, much as she had been doing with the Coast Miwok people. She would then analyze these against the backdrop of different environments. The original proposal did not specify a group as the focus of the field efforts, but sometime during that spring, Kelly chose to work with the Southern Paiute people of the southern Great Basin. This was an excellent choice, as the Southern Paiute span two major geographic provinces (Basin and Range and Colorado Plateau), as well as several different environmental zones and settings. She was also interested in looking at the nature of ethnic boundaries in hunter/gatherer groups, as in her dissertation research she had found such groups characterized in the literature as having "weak localization," a feature she thought questionable (Kelly, March 1, 1933, KADL). The fellowship funds were to be administered by the Laboratory of Anthropology in Santa Fe (Jesse Nusbaum, Director), where Kroeber was affiliated, and where she had been a student fellow in archaeology in 1929.[2]

In mid June, Kelly, then 26 years old, arrived in Santa Fe to take up the fellowship. She had been granted $1800 for the one-year study, which was to include collecting all of the types of data specified in her proposal plus a few months for writing up the results. The project was to be under the joint oversight of Kroeber and Robert Lowie, also of the University of California, Berkeley, and A. V. Kidder of Harvard University. Kroeber was able to supplement her field account for the year with an additional $250 of university funds (Kroeber to Nusbaum, June 2, 1921,

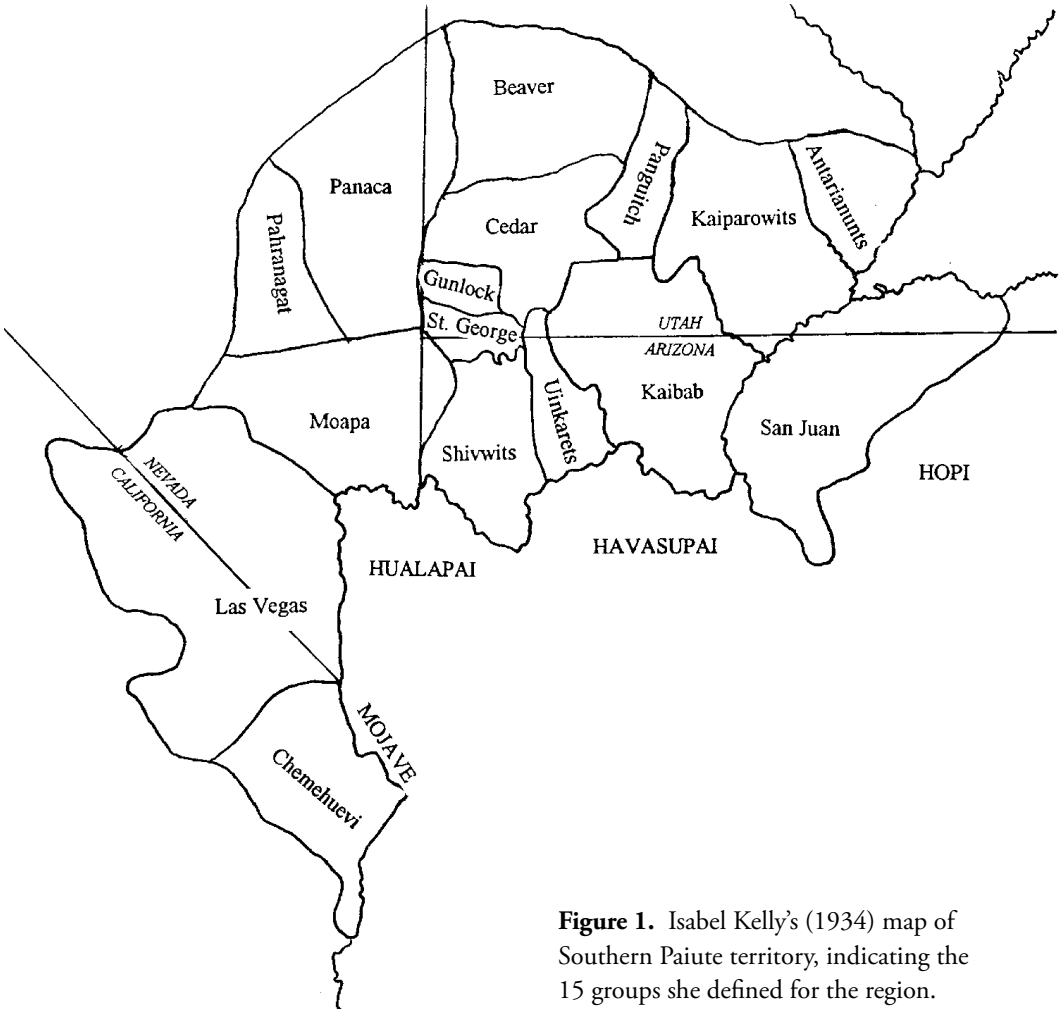

Figure 1. Isabel Kelly's (1934) map of Southern Paiute territory, indicating the 15 groups she defined for the region.

Figure 2. Isabel Kelly leaving Berkeley, California, for the field in June, 1932, in her new Ford. Courtesy of DeGolyer Library, Southern Methodist University and Robert Van Kemper.

LAA). She also had roughly $400 additional from the Laboratory of Anthropology, Harvard's Peabody Museum, and the American Museum of Natural History to purchase ethnographic objects for their collections Wissler to Nusbaum, June 15, 1932, LAA. This level of funding ($2,450 for the year), even though partly committed to purchases, was thought to be adequate, particularly considering the scarceness of funding (and jobs) in Great Depression period. Kelly would ultimately spend roughly 15 months in the field over the next two and a half years on this amount plus a second year National Resource Council supplement of $1,000. The extension in field time was partly due to the difficulty of the field situation and partly to an auto accident she suffered in the field and the recovery time required. She finished the fieldwork in 1934, amassing large amounts of primary data on Southern Paiute ethno-geography and ethnography throughout their pre-contact territory, which she was the first to define (Kelly 1934) (Figure 1). Unfortunately, with the time delays, there was almost no time remaining for synthesis and write-up, and she was unable to publish the bulk of the data at that time. They are now being prepared for publication.[3]

Kelly arrived in Santa Fe in her single seat 1932 Ford (Figure 2) with a fellow Berkeley student, Jane Gabbert, sent with her by Kroeber because of an incident that had occurred on a Southwest reservation that previous summer.[4] As noted, she was not totally new to ethnographic fieldwork when she took on this project, as she had spent a summer among the Surprise Valley Northern Paiute of northeastern California after

her senior year in college and had completed a year's intensive ethnographic work among the Coast Miwok immediately after finishing her doctoral work. Her experiences in Surprise Valley had been difficult, with various Native people in the community refusing to talk with her, even "siccing the dogs" on her more than once. But in her inexperience, she thought that that was the normal course of fieldwork. Kroeber later assured her that that was not necessarily the case, but that in his own field work, he had found Great Basin peoples particularly difficult.[5] Her Coast Miwok experience had been much more positive, although she reported in her letters from the field that she and her consultants were often totally exhausted at the end of each day (Kelly to Kroeber, January 24, 1932, DABL). She also worried over her lack of preparation in linguistics and that there would never be time enough to adequately analyze the data properly (Kelly to Gifford, February 20, 1932, DABL). Thus, Kelly was prepared for what might be a difficult time with the people, but also, to some degree with the country. In the 1930s, the region was quite remote, with few roads and accommodations, and with different environments from low, very hot deserts to high and cold plateaus. None of this deterred her from taking on the task soon after she arrived in Santa Fe.

Kelly and Gabbart left Santa Fe on June 26 for Kaibab, Arizona, the first stop in her field circuit. They drove via northern New Mexico and southern Colorado, passing Mesa Verde, then into Utah and northern Arizona, arriving on July 1. On July 2, Kelly reported to Nusbaum on their adventures along the way:

> Aside from minor details, we had a most enjoyable trip. We ran into heavy and continuous rain above Abiquiu, and it took us some hours to negotiate the stretch from there to Canijilon—going sideways most of the time. We mired twice, and the juniper forest in that region is considerably depleted from our chopping activities. We spent the night at Pagosa—apparently missing a good deal of fine scenery after dark. From there we had an easy run to Mesa Verde. What glorious country we went through! The entrance to the park is magnificent beyond words. Although the sunset was not up to specifications, the panorama alone is worth the entire trip… Both Jane and I felt, however, that the ruins were somewhat dwarfed by the breath-taking scenery (Kelly to Nusbaum, July 2, 1932, LAA).

In the same letter (Kelly to Nusbaum, July 2, 1932, LAA), she describes the situation at Kaibab and her field accommodations:

> The immediate Paiute situation is not bad at all. We arrived last evening and after much consultation, it was decided that we might camp in the school kitchen. We were grateful for this kind gesture, as we brought a thorough-going rain with us. The kitchen is of more than ample size and is equipped with three tables, a kitchen range, and running water. Luxury beyond our fondest hopes!

And rather immediately, Kelly got to work. Much to her surprise:

> We discovered that Dr. [C. Hart] Merriam had been there only three days ago. Last evening his informant presented himself, unsolicited, and was engaged. The very thought of a Paiute who volunteers services as an informant makes me cross my fingers. This morning I tore over hill and dale with him and his wife, and we collected thirty or so plant specimens. The entire camp takes a flattering interest in our activities, and we are continually receiving impromptu calls, and what not. About fifteen perch outside the door and, all told, things are quite

> different from the surly Northern Paiute (Kelly to Nusbaum, July 2, 1932, LAA).

Kelly's initial enthusiasm was tempered somewhat when the person who presented himself as a consultant turned out to be too young for, as she later characterized her field approach, the "unabashedly how-it-was-in-your-grandfather's-day" approach (Kelly 1964:iii). She wrote about three weeks later:

> From the expressions of sympathy, I judge that my letter to Dr. [H. P.] Mera must have been highly lugubrious. I am famous for the mournful letters which I send in from the field. Since I wrote, I have changed informants and am now working with a man who claims to know everything. Although he falls somewhat short of his modest claim, he is really an excellent informant, and I am genuinely pleased with the material that is accumulating. I am trying to do the Kaibab rather thoroughly in order to supplement the nucleus that [Edward] Sapir has.[6] With the other Paiute I shall confine myself more to straight ethno-geography (Kelly to Nusbaum, July 27, 1932, LAA).

Given that Kelly also had funds to collect Southern Paiute material culture for various museums, she was anxious to learn what was available and what the museums were expecting. She wrote to Nusbaum: "I am anxious to discover what sort of museum specimens Dr. [Clark] Wissler [of the American Museum of Natural History] wants. We have been offered the usual buckskin gauntlets, etc., but I have wind of a seed gathering apparatus which might be nice. I suppose Dr. Wissler would prefer a somewhat bedraggled but genuine article to a neat looking model?" (Kelly to Nusbaum, July 2, 1932, LAA). When she still had not heard from Wissler by month's end, Kelly devised her own plan:

> I shall do the very best that I can in the way of specimens, but of course, Paiute specimens are not likely to be particularly inspiring. I think that I shall be able to get a rather nice series of the old basketry styles–save possibly the water jar. I wonder if he would care to have any of the modern tourist basketry products? They are not frightfully expensive, and fifty years from now they might hold some interest for anyone who wanted to study the craft and its developments. I have explicit information that this modern style was learned from the Moapa, and so it might prove of interest to compare the original and the Kaibab version. To date I have a model of a woman's bark skirt; a model of a yucca sandal; and a cradle just outgrown by my informant's great grandchild (Kelly to Nusbaum, July 27, 1932, LAA).

Ultimately, in the course of her entire field studies, Kelly acquired some 325 items of material culture from 12 Southern Paiute/Chemehuevi subgroups for the various museums.[7] The collections are similar in depth and breadth, very well documented and form an excellent corpus for comparison (Figure 3). In a letter to Wissler she wrote:

> Naturally there is not very much of the old culture left, and I should say that about half of the lot are models, but they are good models, conscientiously made. For every specimen I have the name of the maker, together with his band affiliation or affiliations–data which should be useful for comparative purposes... I think that I can say complacently enough, that I have bought every object obtainable which could be of any conceivable interest either now or in the future (Kelly to Wissler, December 16, 1932, KADL).

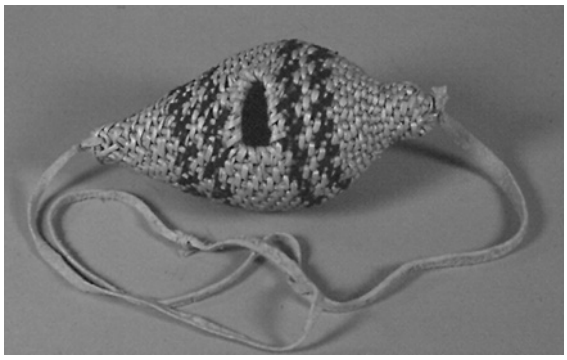

Figure 3. Pitch container, carried by man on hunting trip; pitch was applied periodically to moccasin soles to prevent their wearing out. Made by Mary Ann Frank, Las Vegas/Moapa Southern Paiute (willow, devil's claw, buckskin; 5.2 in. long). Collections of the Museum of Indian Arts and Culture, Santa Fe, NM (Cat # 9990/12). Photo by C.S. Fowler.

Kelly remained at Kaibab for roughly two months, working with people she termed the "last four of the old Kaibab." Her companion, Jane Gabbert, left to return to school, and from here on, Kelly was on her own with her trusted Ford and her camping tent. But, she added in a letter to A. V. Kidder: "I just can't tell you how splendidly things have gone so far. Really, life is good. I have been getting good material, so good that my fingers have been crossed since the first week out" (Kelly to Kidder, September 19, 1932, KADL). She had added considerably to the general ethnographic notes taken by Edward Sapir during conversations with Tony Tillohash in 1910, which he had kindly sent her. In 1964, Kelly published the bulk of her data on the Kaibab Southern Paiute group along with some of Sapir's material in the monograph for the Glen Canyon Project (Kelly 1964). She also included in that publication her basic data from the Panguitch, Kaiparowits and San Juan groups.

While at Kaibab, Kelly had the opportunity to work with two individuals from Escalante, Utah, near the Kaiparowits Plateau. One, a man, she refers to as "impossibly deaf," and the other, a woman, as "moderately good" (Kelly to Kidder, September 19, 1932, KADL). In an interesting note on field methods, she says that the woman was "less than satisfactory on ethno-geography" until they took a field trip to her home country, which she had not visited in 35 years. Upon seeing the area, the woman remarked on several changes to the environment that occurred in her absence, including marked arroyo cutting, reduction of vegetation and increased aridity. Kelly commented that these observations would be of great interest to any cultural geographer or economist. The value of working with people directly in the field and at sites they knew had been espoused by geographer Carl Sauer at Berkeley, and Kelly was following suit.

Kelly moved on to Cedar City, Utah in early September, 1932. From that base, she traveled to several areas, and interviewed the few individuals she could find from different groups in that vicinity (Cedar, Beaver, Parowan, Panguitch, Indian Peak). In all cases, she referred to them as the "last survivors" of their particular groups—undoubtedly speaking to their knowledge of the "old ways" rather than to their actual status. Regarding Indian Peak, near the Utah/Nevada border, she wrote to Kidder:

> The past week I have been camped in the wilds at Indian Peak, sixty miles from here, on exceedingly chummy terms with 7 Paiute, 9 horses, and countless canines resident there. This does not include the amiable bobcat which wandered into my tent one moonlight night. Material on the bands in that vicinity is pretty scrappy, mostly because one has to deal with lone survivors who know only their own little local settlement (Kelly to Kidder, September 19, 1932, KADL).

Kelly speaks further to Kidder of her plans to go in search of the elusive San Juan Southern Paiute people who live south of the Colorado River in what is now called the Navajo Strip but was once the Paiute Strip:

> I leave Thursday for Navajo Spring, near Lee's Bridge, where I hope to tackle an isolated family hailing originally from the Paiute strip. Kroeber advised against trying

> to reach this band—one must travel some miles by horse through the heart of Navajo country. I thought though that I might have my cake and eat it too. If the Navajo Spring informant is even passable, I should be able to get at least the boundaries and thus complete my map. Incidentally, I should like to have a crack at these so-called "Tuba City Paiute". They ought to be interesting because of their isolation from the whites and because of their Ute-Navajo contacts. Evidently it is they who supply basketry for the Navajo trade. As you can see, the Paiute are strewn all over the Southwest, and about the same can be said for me (Kelly to Kidder, September 19, 1932, KADL).

Thus, in early October, Kelly backtracked to Navajo Spring, near Lee's Ferry and Marble Canyon. Once there, she makes the following comment on weather conditions: "Winter draws on apace and I am still trailing gaily over the Plateau, flirting with the chance of a snowstorm that will close the roads. As a matter of fact, the road here from Kanab was quite washed out in spots, and there was a heavy downpour of rain last night. My trusty tent is still flapping in the breeze, dry inside thank heaven" (Kelly to Nusbaum, October 2, 1932, LAA). She had great difficulty finding families, and once she did, she could not enlist a satisfactory interpreter. However, she persevered and managed to collect "a few scant data on the Paiute strip people—not nearly as much as I should like, but better than nothing" (Kelly to Kidder, November 7, 1932, KADL). They were largely unknown at the time, although in the late 1930s, Omer Stewart would also find them and complete a Culture Element Distribution list (Stewart 1942). Their importance in the trade in Navajo ceremonial baskets, as well as the development of fine coiled weaving generally, would also become better known (McGreevy and Whiteford 1985; Stewart 1938; Tschopik 1940), as would their ethnography (Bunte and Franklin 1987). Kelly's San Juan data are included in her publication (Kelly 1964).

After her sojourn at Navajo Spring, Kelly moved on to Santa Clara, Utah, near the Shivwits Reservation. She attempted to enlist Tony Tillohash, Sapir's consultant for his Southern Paiute linguistic studies in 1910 (Sapir 1930-31), but failed as he was "too busy harvesting pine nuts" (Kelly to Sapir, March 9, 1933, KADL). She conveyed a copy of Sapir's volume to Tony, who was very pleased and "spoke warmly of you and asked to have greetings relayed." She was able to do some of her best ethno-geography here, however, working closely with one of the last men familiar with the Shivwits Plateau, on the north side of the Grand Canyon. She and he mapped much of the area, cataloging its many springs, their camps and the local subsistence resources.[8] She wrote to Kidder:

> Business continues to be amazingly good. I have been at Santa Clara nearly a month; have just finished with the Shivwits and am beginning to pry into the affairs of the St. George band. These Paiute bands seem endless and are invariably represented by a lone survivor. The latter are trying as they frequently know little, and even that can't be checked. Some nice items on trade are coming to light, though (Kelly to Kidder, November 7, 1932, KADL).

Kelly comments to Kidder a bit on the local scene (Kelly to Kidder, November 7, 1932, KADL), lamenting some of the conditions in St. George:

> Even the simplest things are hard to find in this country. And as for a primary commodity like money, it's absolutely not to be had. I have to trot 75 miles to Cedar City to cash a check. The local bank is cagey and would not even deign to examine my barrage of cancelled checks, bank statements, and certificates of this and that. I always thought I had an honest face! They did accept a check for collection

> on my Santa Fe account (two weeks ago and still no action) and naively asked if Santa Fe were near Los Angeles!

Kelly also laments to Kidder that she barely has time to do anything but her ethnographic work. Particularly vexing are all of the archaeological sites she is seeing in her field wanderings, and a lack of time to investigate them. After all, as she wrote Kroeber when she was doing fieldwork among the Northern Paiute in Surprise Valley, "archaeology is my first love and my only love" (Kelly to Kroeber, May 27, 1930, DABL). She had been a student at the Laboratory of Anthropology archaeological field school in the summer of 1929 with Kidder, excavating at Tecolote Ruin, and in all of her correspondence with him, she continually speaks of the many sites in the northern Arizona/southern Utah area that she wished she could explore. After her Southern Paiute field work, she would get the chance to realize her dream of working in archaeology again, in southern Arizona and west Mexico (Fowler and Kemper 2008). By the time Kelly finished at Santa Clara, she was able to say that she had relatively full ethnographies in addition to ethnogeographies of the Shivwits and St. George groups.

In mid November, Kelly returned to Berkeley to consult with Kroeber and also to prepare the specimens she had collected thus far for photographing, illustration, and shipment to the museums. She had intended to spend the month of December in the field at Moapa, Nevada, but was unable to do so because of a flare-up of back troubles exacerbated by travel over rough roads. As she wrote to Nusbaum in December:

> I have been obliged to take an enforced vacation which Harold Hitchcock, the famous bone specialist, claims has been brought on by too much Ford-driving. He insists that everyone who drives a Ford must pay a penalty sooner or later and that account is being taken of me because of my having averaged something like 2000 miles monthly since June. At any rate, I must confess that although I have always been a rugged and hearty soul, the last few weeks have been made miserable with a stiff back—all of which involves sleeping on planks, sporting 20 inches of adhesive tape, and desisting from driving. All of this is a perfect nuisance... (Kelly to Nusbaum, December 20, 1932, LAA).

SOUTHERN PAIUTE FIELDWORK, 1933-34

After a month's field study on the Moapa Reservation, Nevada, in January, 1933, where again Kelly received excellent cooperation and data, she returned to Berkeley to begin writing up her materials and to again see to the collections. She also applied for a renewal of her National Research Council fellowship for a second year, during which time she planned to concentrate her work among the westernmost Southern Paiute groups in southern Nevada and California—the Las Vegas people and the Chemehuevi, their close linguistic and cultural kinsmen. These groups, along with the Moapa people, live in the Mojave Desert as opposed to the Great Basin Desert, an environment of considerable interest to Kelly for comparative ethnogeographic purposes. She was granted the extension the following June, and after three months in Santa Fe working on her field notes from the previous year, she again set out for Berkeley before returning to the field.

This time, however, her spirit of adventure led to difficulty. While in Santa Fe, she had met two Harvard students in the Southwest on Rockefeller fellowships, and they had asked if they could meet her in Flagstaff and join her on her trip to the coast after they had visited Grand Canyon. On September 11, she picked them up in Flagstaff and the three headed north in her trusty one-seat Ford. At Cameron, one of the men began driving, and apparently unfamiliar with the hazards of dirt roads, rolled the car, ejecting the three. After a four-hour wait for help, the three were taken by truck to the Flagstaff hospital. Kelly wrote to Nusbaum that the two men escaped major

injury (dislocated shoulder; back sprain), but she had fractured three vertebrae. Of her car and belongings she reported: "The hack is a mess—one whole side demolished. We were strewn all over the lot and the baggage strewn about for 50 feet. Even a whisk broom in the pocket of the Ford and the tools from beneath the seat were widely disseminated…" (Kelly to Nusbaum, September 13, 1933, LAA). Her most immediate concern after the accident was for her field notes which she initially feared were lost, but which were ultimately recovered in her battered trunk. Kelly quipped: "Just as I suspected, it *isn't* safe to travel about with a Rockefeller student" (Kelly to Nusbaum, September 13, 1933, LAA). Kelly spent two months in the hospital recuperating, cheered by periodic visits from Harold and Mary Colton and Katherine Bartlett of the Museum of Northern Arizona.[9]

After additional time recuperating in Berkeley, Kelly was back in the field by mid November, first at Moapa, and then at Parker, Arizona, for Chemeheuvi fieldwork, and then later Las Vegas for additional studies with that group. Her interest in the Chemehuevi was piqued by the comparative ethnogeographic problems, but also by the more recent history of at least some Chemehuevi people's move to the flood plain of the Colorado River where they were influenced by the "virile and distinctive Mohave river culture" (Kelly to National Research Council, December 15, 1933, KADL). She remarked of her field situation in Parker:

> Parker is pleasant as regards temperature, but the town itself is about as dismal, dusty, and uninviting as anything I have seen in years. Work is coming on satisfactorily, but not with such success that I am frenzied with enthusiasm. My Las Vegas informant is a more satisfactory informant than the local people—but he is blind and 200 miles from his native heath. However, the local material is supplementing his very nicely (Kelly to Nusbaum, January 29, 1934, KADL).

The following month, she wrote to Nusbaum that things were continuing to go well and that the supplementary funds he had just sent to purchase collections for the Laboratory of Anthropology were most appreciated: "you may be sure that the sum will be stretched as far as humanly possible. Field work is a pernicious disease—there is just no giving in as long as one can manage to scrape along" (Kelly to Nusbaum, February 16, 1934, LAA). She continued, saying that her notes from the Chemehuevi were by now "voluminous," and that given the warm weather and all the work she has been doing outside, "in complexion I am coming to look like a cross between a Rhode Island Red and a Plymouth Rock" (Kelly to Nusbaum, February 16, 1934, LAA).

In a letter to Kroeber, Kelly refers as well to star gazing with the Chemehuevi during the very pleasant nights at Parker. She was impressed by their knowledge of constellations, finding the data quite comparable to those reported by Leslie Spier (1928) for the Havasupai. She regretted that she could not spend another couple more months there, as the materials were continuing to "roll in most satisfactorily" (Kelly to Kroeber, March 14, 1934, KADL). After leaving Parker, Kelly moved to Las Vegas, where she worked for a month, and then back to Moapa for another short period. She returned to Berkeley in mid April, 1934 after spending some five months in the low desert areas.

POST FIELDWORK YEARS

In the next few years, Kelly was able to publish some of her voluminous Southern Paiute data in a series of papers, mostly focusing on group boundaries and on shamanism. Her paper on boundaries and territories was the first, and remains the primary statement on Southern Paiute territorial distribution (Kelly 1934). In it she speaks not only of the careful mapping she tried to do in the field, but of her consultants' impressions of dialectic variation, economic distinctiveness and other features separating as well as unifying the fifteen large groups she identified. Her works on

shamanism (Southern Paiute and Chemehuevi) provide rich consultant accounts of specific practices and of the persons known to have been involved in the latter part of the nineteenth and early twentieth centuries. Two of the papers were written to honor her mentors at Berkeley, Robert Lowie (Kelly 1939) and A. L. Kroeber (Kelly 1936). The remainder of her Southern Paiute field notes were then set aside—but not forgotten—for some three decades, while she pursued her "first love," archaeology, first in southern Arizona (Hodges Ruin) and then in west Mexico (Fowler and Kemper 2008).

Kelly returned to her Southern Paiute materials in the 1960s, in between continuing various projects in Mexico as well as elsewhere. In the 1950s, the Indian Claims Commission was established, and lawyers for tribes as well as the federal government began looking for published and especially unpublished data for the cases being brought before it for compensation to tribes for lands taken by illegal appropriation. Kelly's unpublished materials were focal points for both sides in various cases involving Southern Paiute/Chemehuevi tribes. This stimulated her to think about writing up her materials, especially when her mentor, Kroeber, suggested that they do a joint monograph on the Chemehuevi (Kroeber to Kelly, August 20, 1955, KADL). She responded that in addition to her Southern Paiute materials, she had at least 10 other major projects that she needed to finish, and added: "I'll never be able to liquidate all that, if I live to be 150, as I write very slowly and painfully (Kelly to Kroeber, September 4, 1955, KADL). Although Kroeber died before either could complete the monograph on the Chemehuevi, Kelly did publish the data on her eastern Southern Paiute groups at the request of J. D. Jennings as part of the Glen Canyon Project, thereby relieving her of roughly one-third of the burden she felt regarding her Southern Paiute materials (Kelly 1964). She fully intended to continue with publication of the remaining materials as well, but the nine other tasks she listed for Kroeber intervened. In the 1970s, however, she worked on a comparative sketch on the Southern Paiute for the massive Smithsonian Institution project, *Handbook of North American Indians*, which she had largely completed by the time of her death in 1983 (Kelly and Fowler 1986). She also authored a sketch of the Coast Miwok for the *California* volume of the same *Handbook of North American Indians* series (Kelly 1978). The remainder of her equally voluminous Coast Miwok ethnographic materials appeared posthumously (Collier and Thalman 1991).

Kelly's fieldwork, including that among the Southern Paiute people, is but one of the numerous major field projects she conducted, most of them multi-year, in the U.S., Mexico and many other areas of Latin America as well as Pakistan (Fowler and Kemper 2008:166-7). Her professional vita is a very long one, characterized by many publications that are deeply analytical and theoretical, but always based on rich data that she personally collected in the field. She was professionally employed throughout her life, although not in an academic setting. Perhaps because of this, she was able to pursue her research with a vigor and rigor that many would envy. ☙

ENDNOTES

1. Kelly's correspondence is contained in her files at the DeGolyer Library, Southern Methodist University; the Laboratory of Anthropology Archives, Museum of New Mexico, Santa Fe; and in the Department of Anthropology Archives, Bancroft Library, University of California, Berkeley (particularly C-23). Additional correspondence is in the A. L Kroeber Papers and Carl O. Sauer Papers, Bancroft Library. All of these sources were consulted for this paper. Access and permission to quote are hereby acknowledged to these institutions as well as to Robert Van Kemper, Department of Anthropology, Southern Methodist University, Kelly's literary executor. A condensed version of Kelly's fieldwork is also found in Fowler and Kemper (2008; see also Kemper and Marcucci 1989).

2. Kelly was a student in archaeology in 1929 under A. V. Kidder at Tecolote Ruin (LA 296) sponsored by the Laboratory of Anthropology. Kroeber was in charge of the ethnographic field training program that year on the Walapai Reservation (Woodbury 1993:106).

3. The Kelly Southern Paiute archive is being processed for publication by me through the permission of her literary executor, Robert Van Kemper.

4. This involved the murder of a female graduate student on one of the Apache reservations (Schmerler tragedy), and officials were a jittery about having a lone woman in the field (see Parezo 1993:361-62). Nusbaum and Kroeber corresponded about the situation, and both felt that if she had a companion, the Bureau of Indian Affairs area director might be more easily persuaded to give permission for Kelly to work on the reservations.

5. Kroeber (1935:9), based on his own field experience, characterized Great Basin Shoshoneans as "superficial[ly] lack[ing] amiability," and not without "churlish incivility." Kelly told me of her difficulties in Surprise Valley in 1978 when I visited her at her home in Tepépan, Mexico.

6. Kelly had written to Sapir about working among the Kaibab in 1932, and he had kindly sent his unpublished notes based on ethnographic data received from Tony Tillohash when he was interviewing him on the language (Sapir 1930-31). Kelly used the notes for comparison, and refers to them continually in her sketch of the Kaibab (Kelly 1964). The full text of his notes was published with comments by Fowler and Euler (1992) as part of the multi-volume "Collected Works of Edward Sapir."

7. Kelly's Southern Paiute material culture collections are at the following institutions: American Museum of Natural History, Museum of Indian Arts and Cultures (for the Laboratory of Anthropology, Museum of New Mexico) and the Peabody Museum of Archaeology and Ethnology, Harvard University.

8. I showed these materials to Leroy Condie, Carol's father, who was very familiar with the Shivwits Plateau from his youth. He helped me considerably in sorting out the locations and names of several of the springs. Mr. Condie was much involved with the Southern Paiute people of southeastern Utah, and we thoroughly enjoyed our visits talking about the people we both knew.

9. A series of letters, Kelly to Nusbaum, September 15, 1933 and Nusbaum to Kelly, September 21, 1933 (all LAA) documents these visits.

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"The Trail of an Archaeological Wedding Journey" —and Beyond, Frank and Theresa Russell in the Southwest, 1898 to 1903

DON D. FOWLER AND NANCY J. PAREZO

"Live in Gallup?" inquired the porter. "Oh, no. Our home is in the East." "Lawd! Must have a grudge agin yourselves then, stoppin here!" (T. Russell 1906:39). Thus begins Theresa Peet Russell's delightful narrative tale of her three-month archaeological honeymoon trip with husband Frank Russell by horse, mule and wagon across northern Arizona Territory in the summer of 1900. They had been married two days before in Iowa and immediately set out for the Southwest.[1]

How the Russells came to make their honeymoon journey, coupled with Frank's earlier solo Southwest trip in 1898; their nine-month mule and wagon archaeological reconnaissance in 1901 south to north from Tucson across Arizona into southern Utah and back to Tucson; their seven months adventure in ethnography with the Pima in 1901-02; Frank's untimely death in 1903; and the beginning of a whole new life and career for Theresa, is the stuff of our story.

THE HONEYMOON TRIP

The Russells got off the Santa Fe Railway train at 3:00 A.M. on July 3, 1900 and were taken to a hotel which looked out on the railroad yards and the Harvey House. The next day, John Lorenzo Hubbell arrived with some of his family. Frank had met Hubbell in 1898 and now would contract with him to outfit Frank's exploring party for the summer. In 1900 "Don Lorenzo" was 47 years old and controlled several trading posts on the Navajo Reservation, centered on the home post at Ganado. He and his sometimes partner, C. N. Cotton, dominated the Navajo weaving industry and market for many years. In July, 1900, Hubbell was returning from the Republican national convention.[2]

On July 5, "the Russell family [and the Hubbells] were loaded up in a brand new road wagon with spring seats and cushions thrown in." The party traveled to Ganado, stopping for lunch at Cienega, site of "then-a-building" St. Michael's Mission (T. Russell 1900:2).[3]

BEGINNINGS

Frank Russell (1868-1903) (Figure 1) and Frances Theresa Peet (1873-1936) (Figure 2) were born and grew up in Iowa. Frank enrolled in the University of Iowa in 1887. He is said to have arrived at the university with ten dollars in his pocket, but managed to partly work his way through school as a scientific illustrator. Frank became a student and assistant of the famed naturalist Charles Cleveland Nutting (1858-1927), curator of the university museum. In 1891, Frank accompanied Nutting and A. G. Smith, a mathematics professor, on a collecting trip to the Saskatchewan River region of Canada.

When Russell returned to the university for the 1891 fall term, he apparently met entering freshman Theresa Peet. Later, during their honeymoon trip, in her September 1, 1900 journal entry, Theresa wrote (T. Russell 1990:71) that she spent a miserable rainy

Figure 1. Frank Russell, Sacaton, Arizona, 1902. Photograph by Theresa Russell. Courtesy Stanford University Library Special Collections, Negative Number BP-3617.

Figure 2. Theresa Russell, ca. 1920. Photographer unknown. Courtesy Stanford University Library Special Collections, Negative Number BP-3616.

night huddled in the covered wagon somewhere in the Painted Desert waiting for Frank, who had gone off on foot to scout a new route toward Keams Canyon, "... no fervent revivalist ever inquired "Where is my wandering boy tonight?" more anxiously than I did. For he must be out there somewhere in the storm, with not even a coat to keep him dry. And on our ninth anniversary too! What a celebration!" Frank, in fact, had managed to stay dry and returned safely early the next morning.

Frank graduated from the University of Iowa in 1892 with a B.A. in zoology. During the 1891 Arctic trip, he had met Roderick Ross MacFarlane (1833-1920), a long-time Factor for the Hudson's Bay Company. MacFarlane made significant ornithological and other collections across the Arctic North for the Smithsonian Institution and Canadian museums for 40 years (Preble 1922). Russell wrote, "His enthusiastic descriptions of the field, as yet almost unexplored, roused in me a strong desire to visit the Far North" (Russell 1898a:iii-iv). Nutting received approval from the University regents for Russell to make a two-year solo expedition. Unfortunately, they did not appropriate any monies, but the first year was covered by the University president and the second year by faculty, students, and townspeople.

It was an epic journey, across hundreds of miles of the Arctic North on foot and snowshoes, by dogsled, by canoe and skiff, and an occasional steamboat, battling voracious mosquitoes in the summers and suffering intense cold in the winters. The adventure was capped by a canoe trip down the Mackenzie River from Great Slave Lake to its estuary in the Arctic Sea and on to Herschel Island. In the 1890s whalers over-wintering at Herschel Island made brief summer hunts and then sailed around Alaska and southward to the United States. Russell was able to get aboard a whaler there and arrived in San Francisco on October 27, 1894. On November 2, he arrived by train in Iowa City to

be enthusiastically met "by the faculty, students, and citizens, to the number of several hundred" (Russell 1898a:157). Although he doesn't say so, presumably Theresa was among them. Russell's book, *Explorations in the Far North,* chronicling his astounding travels and containing ethnographic sketches of several First Nations groups was published by the University of Iowa (Russell 1898a).

Russell received a master's degree from the University in 1895 and then entered Harvard to study anthropology. He was made an Instructor in Anthropology, taking a bachelor's degree in 1896, a master's in 1897, and a doctorate in 1898 (Russell 1898b). Regarded as "a rising star," he was made a permanent faculty member in 1899, serving alongside Frederic Ward Putnam and Roland B. Dixon. Russell returned to the Arctic in the summer of 1897 to do some ethnography and collect ethnographic and skeletal material. Sometime during the summer, however, he contracted the tuberculosis that would change his and Theresa's lives.

Theresa graduated Phi Beta Kappa from the University of Iowa in 1895, majoring in philosophy and English, and began teaching in Iowa schools. She and Frank clearly were deeply committed to each other. They courted when they could, given Frank's Arctic sojourn and his move to Harvard in 1895. In 1898, with his prospects of a permanent position at Harvard in the offing, Theresa enrolled for the 1898-99 school year at Radcliffe College.

BEGINNING THE SOUTHWEST SOJOURNS

Frank's tuberculosis changed everything. He could no longer work in the Arctic, and humid and wintery Cambridge exacerbated his condition. He was advised to move to the dry Southwest for "the rest cure." But lolling idly about was not to his liking.

Russell was very ambitious to advance in his chosen field of anthropology. His mentor, Frederic Ward Putnam, was a long-time permanent Secretary of the American Association for the Advancement of Science (AAAS), and Frank soon became involved in the organization. Through the AAAS he met the anthropological luminaries of the day, especially W. J. McGee, a fellow Iowan. McGee was John Wesley Powell's protégé. After 1894, when Powell was forced to resign as Director of the U.S. Geological Survey and retreat to his beloved Bureau of American Ethnology (BAE), McGee became de facto head of the Bureau, giving himself the title Ethnologist-in-charge (Fowler 2010:104-116, 144-147). McGee, always wanting to advance Washington-based science, saw helping Russell as a way to consolidate Washington-Boston anthropology.

FRANK'S 1898 SOUTHWEST SOJOURN

Rather than opt for a rest cure, Russell undertook an active southwestern trip in 1898, with some support from McGee. He spent July 1 through September 16 working with the Jicarilla Apache in Dulce, New Mexico, where he recorded origin and trickster myths, a curing ceremony (involving sand paintings), and conducted the first ethno-botanical research in the region (F. Russell 1898c-e).

Frank badly wanted to see the ceremonial dances at Hopi. An avid bicyclist, as many were in the 1890s-1900s, he headed off, later reporting to McGee that, "I rode the entire distance [700 miles, from Dulce, New Mexico to Hopi and back] on my wheel over blind trails, sand, two mountain ranges, and forded the San Juan fives time and 'astonished the natives,' both white and red, with the appearance of a bicycle in the back country" (FR to WJM August 31, 1898). During the trip he became acquainted with J. L. Hubbell at Ganado and Thomas Keam at Keams Canyon. Russell then apparently traveled to southern Arizona, presumably by train, and began initial ethnographic work with the Pima. He returned to Cambridge, writing reports and teaching during the period 1899 to the summer of 1900.

THE 1900 TRIP

Russell pushed for continued support from, and possibly a job with, the BAE that would allow him to spend most of his time in the field in the Southwest to help his tuberculosis. In 1900 McGee had no money to help out, but provided encouragement. Russell decided to make a trip anyway under Harvard University auspices, partly using his own funds. He applied to the Office of Indian Affairs for a permit to excavate archaeological sites on the Hopi Reservation, hoping to find large, undisturbed ruins with extensive, intact burial grounds from which to collect skeletal material and artifacts. In 1898 he had heard from various sources that such ruins existed; but they proved to be mostly chimerical.

BEGINNING THE WEDDING JOURNEY

Hubbell outfitted the Russells with a team (two horses and two mules), "a prairie schooner carrying over a ton in load," two Hispanic teamsters, Nosifor and Erminio, and a Navajo guide, cook, and interpreter, Net Sliver, who had worked for Hubbell since he was a teenager. The load included a typewriter. As would his archaeological successors a century later, Russell began his survey with the most up-to-date word processing equipment. Thus outfitted, the party headed across the Chinle Valley with Black Mesa on the far horizon. Theresa, who had never been west of Iowa, nor apparently ever before camped out, took it all in stride (T. Russell 1900, 1906).

In 1900, the only sure guides to archaeological sites in the area were reports by the Mindeleffs (C. Mindeleff 1900; V. Mindeleff 1891), and they were of sites on or adjacent to the Hopi Mesas. Frank was looking for new sites. As they proceeded, they inquired of various Navajos they met about large ruins and received vague directions to "heap big casas, plenty wesos [*huesos* meaning bones], ollas, mucho everything" (T. Russell 1906 IV:281). Such places turned out to be "undeniably chiquito," or not where they were said to be, or both, and if found at all were usually looted. After various wrong turns on poor to non-existent roads, dry camps, muddy springs yielding murky "lejoy" water, wayward horses and mules, and finding only disturbed and "chiquito" archaeological sites, they headed to Keams Canyon.

There they were received hospitably by Thomas Varker Keam (1842-1904) and his staff. By 1900 Keam was at the height of his powers as an Indian trader and broker for the frantic collecting of Southwestern archaeological and ethnographic materials by Eastern and European museums in the closing decades of the nineteenth century (Parezo 1986, 1987; see also Fowler 2010:104-116, 138; Graves 1998:139-170; Wade and McChesney 1981).

Well fed and rested, the Russells continued onto the Hopi Reservation. Frank and Sliver spent much time and effort chasing rumored large, intact ruins in Blue Canyon, Oraibi Wash, Polacca Wash, and elsewhere. Some of the sites they saw had been recorded earlier by the Mindeleffs; none were suitable for Frank's purposes. They ultimately found some small ruins to excavate in the vicinity of Antelope Mesa, an area previously exploited by Keam's diggers. They paused at the ruins of Awatovi long enough for Theresa to do her laundry and hang the clothes on the ruined walls of the church, using stones as clothes pins (Davis 2008).

At Awatovi, they encountered a party led by Charles L. Owen, from the Field Columbian Museum in Chicago. Owen had recently been hired and sent to Arizona by George Amos Dorsey, the Field Museum anthropology curator, to make prehistoric and historic Hopi pottery collections. He apparently succeeded, for the following year Dorsey would brag that "we have left Washington [i.e., the Smithsonian Institution] in the rear so far as prehistoric pottery is concerned" (Dorsey 1901).

On August 18, 1900, a letter from Putnam with an enclosed letter from the Office of Indian Affairs finally reached Frank, denying his application for a permit to excavate on the Hopi Reservation. The Russells also learned that Charles E. Burton from the Office of Indian Affairs was due back from the

East, expecting to question Frank about why he had proceeded without a permit. They concluded that their "day of grace was short, and since the Moki Snake Dance was to take place at Shongopovi ... it was deemed advisable to take the whole outfit to the middle Mesa; at which point we would be twenty miles on our way to the Colorado Chiquito where we decided to spent the rest of the summer" (T. Russell 1900:54-55). The Little Colorado River was also conveniently off the Hopi Reservation and beyond Burton's jurisdiction.

The Russells (probably correctly) assumed that George Dorsey was behind the denial of the permit. Dorsey saw himself and the Field Museum, established in 1893, in fierce competition with other museums, especially the Smithsonian, for artifact collections, using political connections if necessary to gain an advantage (Almazan and Coleman 2003).

The Russells did witness the Snake Dance at Shongopovi, which Theresa described in wide-eyed detail (T. Russell 1900:55-61). At the end of the ceremony, they were summoned to meet with Burton and show their official permission to excavate on the reservation. They sent their regrets, broke camp at the foot of the mesa and hastened toward the Little Colorado (T. Russell 1900:62).

The rest of the summer was spent searching for the always elusive large, intact sites. The only one they found was on private property; the owner wanting to dig it for himself. After various adventures and misadventures, the pair headed home. They reached Ganado on September 13, settled their accounts, and bought some Navajo weavings and silver. They boarded a train at Gallup on September 16 and reached Boston on September 20. Theresa wrote that "a little dose of domestication" would not be hard to take. But,

> I fancy a nine month accumulation of juicy steaks and fresh fruits, of daily newspapers and gowns *en traine*, of gossip and theater... will not be sufficient to tarnish the haunting memory and majestic influence of sand and sagebrush, tortillas and frijoles, of camp and caballos, of Sliver and Erminio, of the agua that was always "very lejoy," of smiling skies; so that next June...[we might return] (T. Russell 1900:82).

THE 1901 SEASON

But in fact the Russells were back in the Southwest by early March, 1901. Frank's tuberculosis flared again, making a return to the dry and warm Southwest imperative. McGee now offered Frank a salaried position with the BAE, starting March, 1901. He found money for the position by furloughing Matilda Cox Stevenson on the grounds that she was years behind in finishing a report on her work at Zuni Pueblo. It was a serious mistake. She had powerful political connections in Washington and McGee was forced to hire her back in 1902 (Parezo 1993; Miller 2007).

On the promise of the Bureau job, Russell resigned his position at Harvard in late January, 1901, and he and Theresa arrived in Tucson in early March. His assignment was two-fold. First, to conduct a major archaeological reconnaissance of southern and central Arizona, and second, to continue and enlarge his ethnographic fieldwork with the Pima Indians.

The Extended Reconnaissance

The reconnaissance in fact covered sections of southern and east-central Arizona, a transect across northern Arizona as far as the flanks of Kaibito Plateau, northeastward across Monument Valley and the San Juan River to Bluff, Utah, and on to southwestern Colorado. They then returned to Tucson by traveling southward along the New Mexico-Arizona border.[4]

The Southern Arizona Survey

The Russells began surveying the Tucson Basin in early March, 1901, at first using their bicycles, sometimes on horseback, or walking. For longer trips another mode of transportation was required. Frank purchased a team of mules and harness and wagon for $400, which, he told McGee, he considered a private contribution; he paid the teamster from his government stipend. It is

unclear how long the teamster was with them.

On April 7, 1901, the party headed north from Tucson to the San Pedro River Valley. They camped along the river and made side trips to see cave sites and pictograph panels. They then went north to the confluence of the San Pedro and Gila rivers. Next, they turned southeast and visited "cliff houses" in the Galiuro Mountains, then on into the Gila River drainage. They continued up the Gila River valley to Solomonville and Safford, where they recorded numerous ruins, most of them destroyed by the new canals for the burgeoning agricultural development then underway in eastern and central Arizona.

After returning briefly to Tucson, the pair headed southeast through Apache Pass, Bisbee, and Naco, and probed the flanks of the Huachuca Mountains. Leaving Theresa and the team to rest in Fairbanks, Frank walked 25 miles to Benson and took the train back. They then headed to Patagonia and Greaterville, Pantano, Tanque Verde, and back to Tucson. Russell reported in May that they had traveled 927 miles by mule and wagon that he had walked an additional 75 miles, and recorded about 100 ruins.

On May 1, 1901 Frank finally received his official appointment as a staff member of the BAE. However, there were bureaucratic complications. When he sent in his forms for reimbursement of expenses, he was told that he hadn't been properly sworn in as a federal employee. Frank replied, "Your note has just reached me. No, I was not aware that a plain 'ethnologist' was expected to swear on beginning to drive mules in this benighted country. However, having driven them seven weeks, I am ready to swear to anything if you will explain what" (Russell to W. J. McGee, May 16, 1901, BAEC).

There were further complications. Frank could not become an official government employee until he had taken a civil service exam, required of all federal appointive positions by the Civil Service Act of 1883. The exam, probably written by McGee, came from Washington, but there were delays in getting it administered. Finally, an appropriate person, apparently a postal employee, was found. Russell wrote: "It was finally held in the back of the post office. The Commissioner began sweeping out the post office as I began the examination and it was also hot. I hope you will not read the papers!" (Russell to W. J. McGee, June 10, 1901, BAEC).

The Second Reconnaissance

On June 12, the Russells left Tucson by wagon, headed north. They examined some sites near Florence and continued on to Casa Grande, the first archaeological site to be set aside by the U.S. government in 1892. Russell wrote, "The place needs a custodian.[5] The warning against defacing the building seems to act as an incentive to some persons to do that which is forbidden" (Russell 1901-02:June Report 1).

They explored along the foothills of the Phoenix Mountains, noting several sites that might warrant further investigation. But it was growing hotter, and they headed east for higher country, exploring in the Tonto and upper Salt River valleys and adjacent areas, including the Sierra Ancha region. They recorded numerous archaeological sites that would later be associated with the Mogollon and Salado cultures (Haury 1934; Reid and Whittlesey 2005), a number of them undisturbed.

The two then turned northwest toward the Verde Valley, stopping to see Tonto Natural Bridge before proceeding to Montezuma Castle and Montezuma Well. When the 1906 Antiquities Act was passed, Montezuma Castle was one of the first sites to be declared a national monument. Montezuma Well, on the other hand, did not come under government control until 1947.

By the end of June, the Russells were in Flagstaff where they rested for several days. They continued north to Willow Springs, six miles west of Tuba City. Near the spring are several very large boulders of red Wingate sandstone. They are covered with Hopi clan symbols, left by those who followed the legendary Salt Trail from the Hopi mesas and down into the end of Marble Canyon at the confluence of the Little Colorado and Colorado rivers (Colton and Colton 1931; C. Mindeleff 1900).

Frank knew of the Salt Trail and had heard reports of archaeological sites in Marble Canyon and the lower end of Glen Canyon and planned to survey in that direction. However, a Southern Paiute man told him that the tinajas they would have to depend on for water were dry that year. Frank then shifted his gaze to the canyons of the Kaibito Plateau and adjacent areas toward Navajo Mountain. At the Red Lake Trading Post he hired trader Eugene Powell as a guide and Navajo interpreter. The trio was met at the mouth of a tributary of Navajo Canyon by:

> ...a small deputation of well armed Navajo who announced that that was the end of our journey. Mr. Powell explained and re-explained that the Great Captain at Washington had sent me to see and to photograph the ruins. ...they did not want a white man in the maze of canyons... They cited the case of the Field Columbian Museum parties that worked the Little Colorado ruins last year. That party they said, (and they spoke truly for I saw them myself) had dug about the ruins of the ancient people and left their bones lying about on the ground. 'If any of our women who are with child see these bones while herding the sheep it will be bad for them' (Russell 1901-02:June report, 8-9).

Eventually the Russells were permitted to proceed, but there was little grass for the animals; so they only stayed a short time. They did, however, visit about 20 ruins, two of which were relatively large. They tried a second time, but were again turned back because of drought conditions and Navajo concerns.

The Russells returned to Red Lake, again resting for a few days before heading northeast to Marsh Pass, down Laguna Creek and on through Monument Valley to the San Juan River. They apparently surveyed for sites in the Marsh Pass area, although it is unclear if they entered Tsegi Canyon. They crossed Monument Valley quickly due to a lack of water and feed for the animals.

They did hear that the Wetherills had taken "five hundred boxes of pottery and the like out of that district" (Russell, September Report, pg. 1). The "five hundred boxes" probably reflects local lore about the activities of the Wetherill brothers in the 1890s when they spent about a month digging in Keet Seel and elsewhere in Tsegi Canyon. In the spring of 1895, Richard Wetherill wrote to Talbot Hyde, "We dug from one burial mound 400 pieces of pottery—very fine. ...this is by all odds the finest collection of pottery I have ever seen" (cited in Fowler 2010:194).

The Russells crossed the San Juan River at Bluff City, Utah. There they learned of a site north of town that the local Mormons intended to clear out and preserve as an example of "Hebrew architecture." This may be the Chacoan outlier known as the Bluff Great House, recently excavated by Catherine Cameron and Steven Lekson (Cameron 2009). They heard of ruins farther north, toward and in the Abajo Mountains, as indeed there are, including the Alkali Ridge sites, excavated by J. O. Brew (1946) in the 1930s, and the hundreds of sites around Blanding, Utah and on Cedar Mesa (Matson 2006) and adjacent areas, many looted for over a century by the locals.

They also heard of sites of the "so-called 'Basket-Makers'[that] may yield valuable information on general problems of relationship and migration" (Russell, September Report, pp. 1-2). Russell was probably referring to the sites that the Wetherills and others had excavated in Grand Gulch and adjacent canyons in the 1890s (Blackburn and Williamson 1997). They had found rich troves of burials, baskets, and other material items (but no pottery) stratigraphically beneath "Cliff Dwellers" materials. In 1897 T. Mitchell Prudden (1897) had published a popular article, "An Elder Brother of the Cliff Dweller," calling attention to the earlier culture the Wetherills had named "Basket Makers" (later Basketmakers). In 1914, A. V. Kidder and Samuel Guernsey (1919) began a systematic and successful search for Basketmaker sites in the Monument Valley-Marsh Pass area, validating the Wetherills' observations and

providing a firm foundation for subsequent research on Ancestral Puebloan sites in the San Juan Basin and adjacent areas (Fowler 2010:313-320).

The Russells then headed east, visiting some of the well-known ruins in the Hovenweep area first described by William Henry Jackson (1875, 1876) and William Henry Holmes (1876). They stopped at Cannonball Ruin, recorded by A. V. Kidder, S. G. Morley and J. G. Fletcher during the legendary 1907 School of American Archaeology field school led by Edgar Lee Hewett (Fowler 2010:265-266) and partially excavated by Morley (1908) the following year.

Apparently leaving their wagon and animals at a livery stable in Mancos, the Russells boarded a Denver and Rio Grande (D&RG) train for Denver to attend the AAAS meeting during the last week in August. McGee was at the meeting (Science 1901:349), and he and Frank discussed Frank's work and future plans. At the conclusion of the meeting, the Russells joined a junket by train, wagon, and horseback to Mesa Verde (Ring 1901:01 [reprinted in Nusbaum 1980:65]). The trip was arranged by the Colorado Cliff Dwellers Association, a women's group incorporated in 1900 to call attention to Mesa Verde and seek its designation as a national park. They and others were ultimately successful in getting Mesa Verde declared as the first cultural national park in 1906 (Fowler 2010:306-309).

On September 8, the Russells rode the D&RG train from Mancos to Antonito, transferred to the Texas, Santa Fe, and Northern Railroad (the famed "Chile Line") and traveled to Santa Fe. Over the next week, they visited Santa Clara and San Ildefonso pueblos and the Pajarito Plateau, probably examining Puyé and adjacent sites. They then returned by train to Mancos on September 15 or 16, retrieved their mules and wagon, and drove to Bloomfield, New Mexico and then south to Chaco Canyon.

On September 19 and 20 they visited ruins, "within a radius of seven miles of Pueblo Bonito" and found little evidence of excavations therein. "I should say without hesitation that the stories of vandalism that have been circulated concerning that region are utterly false," Russell wrote to McGee (Russell 1901-02:September Report 2). The reference here is to the campaign then being waged by Edgar Lee Hewett and others to drive the Wetherills and the Hyde Exploration Company out of Chaco Canyon (Snead 2001:48-57; Fowler 2010:199-200).

The Russells continued on, possibly stopping at St. Michael's Mission, thence to Gallup and on to Zuni, reaching there by September 23 or 24. After a brief stop, they headed south along the New Mexico-Arizona border to Clifton, Arizona. Leaving the team in Clifton, Frank rode for four days along the eastern border of the San Carlos Reservation and recorded several large masonry ruins, which he thought were previously unknown, including, apparently, the Point of Pines complex.

The weary pair arrived back in Tucson on October 10, 1901. Frank estimated that since June they had traveled 1,535 miles in their wagon, 200 miles in others' wagons, over 220 miles by rail, 50 miles by horseback, and over 100 miles on foot. In a later letter to Harvard president Charles W. Eliot, Russell said they had traveled 1,000 miles during their March to June survey in southern Arizona—probably an over-estimate. For the entire reconnaissance he added, "We sometimes drove our wagon where vehicles had never gone before.... Sometimes we walked all day in burning sand with the temperature ranging as high as one hundred and twenty in the shade" (Russell to Eliot, February 3, 1902, copy in PMA). Whatever the actual number of bone-jarring miles, the entire reconnaissance effort was epochal, probably never equaled again.

The Russells, José Lewis, and the Pima[6]

When the Russells reached Tucson in early October, 1901, Frank settled in to write his reports, but his tuberculosis flared again and he was periodically confined to his bed. In mid-November he and Theresa moved to Sacaton, the main settlement for the Pima (Akimel O'odham) Gila River Reservation. McGee urged Russell to hire a Papago (Tohono O'odham) man, José Lewis, whom McGee had earlier

Figure 3. Jose Lewis, 1902.
Photographed by Frank Russell. Courtesy of the National Anthropological Archives, Smithsonian Institution, Negative Number INV06414900.

employed during his 1894 ethnographic expedition to Papaguería and the Seri country (McGee 2000). Lewis was literate, obviously perceptive, and became an outstanding ethnographer of his own people.

The three began working together on the Gila River Reservation in mid-November. The Russells rented a room in Sacaton from a local Presbyterian minister, Charles H. Cook, and were given access to Office of Indian Affairs wagons and animals for travel. Lewis was an excellent choice. He had arrived in Sacaton two weeks before the Russells and immediately began preliminary research (Roffler 2006:385). In his Pima ethnography, Russell (1908:19, *n.* a) mentions Lewis only as his "principal interpreter." As Bernard Fontana (1975:xiii-xv) rightly states, however, Lewis should, in fact, have been listed as co-author of the monograph. He compiled essentially all the data on linguistics, myths and songs and was the interpreter for the extensive discussions of Pima history aided by the mnemonic calendar sticks that carried Pima history back to the 1830s (Russell 1908:34-66, 206-389). Russell, using his trusty typewriter, transcribed the materials Lewis brought to him. Lewis was also of major assistance in helping the Russells gain access to over 200 Pima houses in search of heirloom ethnographic artifacts. With Lewis's help, Theresa was able to conduct interviews with some Pima women. Her "Sikachu, a Woman of Pimeria" (T. Russell 1901) is an excellent study, deserving separate treatment.

DENOUEMENT AT HARVARD

By February, 1902, Frank's tuberculosis was seemingly in remission and he asked Harvard President Charles Eliot to be reinstated. He had some doctors submit letters testifying to his apparent good health. Some large part of this request probably had to do with the uncertainty of his position with the BAE, because Matilda Stevenson had actively pursued and won reinstatement, and McGee was forced to rehire her (Parezo 1993:45). Harvard readily agreed to his return.

The Russells left Sacaton in early June 1902, paused for a time in Iowa, and were at Harvard by mid August. At some point in the following months, Frank submitted the bulk of *The Pima* manuscript, the catalog of the 300+ ethnographic artifacts and numerous photographs he had collected for the National Museum, and two short articles on Pima history (Russell 1903a-b).

But things almost immediately went sour. Harvard did not increase his $1000 per year salary, and with the BAE position in jeopardy, payment of monies owed Russell were not soon forthcoming. Frank's tuberculosis returned, aggravated by the cold, humid weather. In early November he resigned his position again.

The Russells fairly quickly moved back to Arizona. With the help of Theresa's brother and A. G. Smith, the Iowa mathematics professor, they bought a ranch near Chloride, north of Kingman. They struggled to get the land cleared and crops planted,

but Frank's condition continued to deteriorate. He died on November 7, 1903, age 35. His body was shipped to California and cremated. His peers celebrated his short and brilliant career and mourned his passing (Hodge 1903; Pepper 1904). Some months later, Theresa received a check from the BAE for $500 as final payment for the manuscript. The published report appeared in 1908 (Russell 1908, 1975). Theresa later edited and published a series of Pima myths Frank had collected (Russell 1909). The ranch was sold and Theresa returned to Iowa and then to Cambridge.

THERESA RUSSELL'S SECOND LIFE

Theresa had met the Pragmatist philosopher/psychologist William James (Richardson 2011), perhaps during her time at Radcliffe in 1898-99, or as a faculty spouse at Harvard, 1900-1902. James (1842-1910) was obviously impressed with her. In 1906, he gave a series of lectures at Stanford University. Theresa went with him, serving as an Assistant in Philosophy while he was there.

She remained at Stanford as an Instructor in English. Subsequently, she attended Columbia University to study with Ashley H. Thorndike (1871-1933), an expert on English literature. Her doctoral dissertation was "Satire in the Victorian Novel" (T. Russell 1920). She returned to Stanford to teach English literature and was promoted to full professor in 1930. During the 1920s, she published essays on the English poet Robert Browning (1812-1889), collected as *One Word More on Browning* (T. Russell 1927). Her final major work was the delightful *Touring Utopia, the Realm of Constructive Humanism* (T. Russell 1932). She never remarried, but shared a house with another Stanford faculty member, Elizabeth Buckingham. She died of cancer in 1936, bringing to a close a little remembered, but epic saga of two remarkable and devoted individuals and their archaeological and ethnographic lives and work together in the American Southwest at the turn of the twentieth century. ☙

ENDNOTES

1. We draw on several archives. The Frances Theresa Russell papers and some of Frank Russell's papers are in Special Collections in the Stanford University Library. Other Frank Russell materials are in the Harvard Peabody Museum Archives. The F. Russell and W. J. McGee correspondence is in the BAEC letter files in the National Anthropological Archives; other McGee papers are in the Library of Congress.

2. On John Lorenzo Hubbell, his life, family, trading empire, and political activities see especially Blue (2000), Brugge (1993),McNitt (1962), and Powers (2001).

3. Both Frank and Theresa Russell kept daily journals of the 1900 trip. Only a portion of Frank's journal, from July 3 to July 29 has survived. Theresa's handwritten journal is complete. In 1906 she published her journal and occasionally indulged in some literary license (Russell 1906). For example, in the published version, she has them riding their bicycles from Gallup to St. Michael's Mission (T. Russell 1906 I:44-46). Their 1900 journals state they rode to the Mission and on to Ganado in Hubbell's "road wagon."

4. Russell (1901-02) chronicled these amazing trips in his monthly reports to and correspondence with McGee. Elsewhere (Parezo and Fowler 2012) we trace in detail the routes taken by the Russells during their reconnaissance and discuss some of the sites they visited. Here we briefly summarize the highlights of their trips. Unfortunately, Russell's detailed notes on sites he recorded throughout the 8-month journey disappeared from the BAE files sometime before the 1950s. They may have been copied or used by Jesse Walter Fewkes as background to the excavations he undertook in northern Arizona after 1903 (see Fowler 2005:17-24). We are left with Russell's somewhat terse monthly reports and a few letters to McGee.

5. In fact, 20-year-old Frank Pinkley (1881-1940) was appointed as custodian in September, 1901, the beginning of his legendary forty-year career, at first at Casa Grande, and later (after the formation of the National Park Service in 1916) as Superintendent of the Southwestern National Monuments management unit of the National Park Service, overseeing 27 national monuments (National Park Service 2011:Chapter 18).

6. We are aware that the current names for Pima and Papago are Akimel O'odham and Tohono O'Odham, respectively. Here we use the names used by the Russells and other Anglos in 1901-02.

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Potters' Resources in the Vicinity of Tijeras Pueblo, New Mexico

HAYWARD H. FRANKLIN

Tijeras Pueblo (LA 581) is one of the best known settlements of the late P III–early P IV (Coalition into early Classic) Periods in New Mexico. As a large settlement situated in a strategic geographic location, the importance of Tijeras Pueblo, both prehistorically and currently, has been recognized by many archaeologists. Owing to its location within the Cibola National Forest, the site and its immediate surroundings have been protected, unlike the fate that befell most Classic Period towns along the middle Rio Grande drainage.

The major occupation began about A.D. 1250 and lasted until about 1425 (Cordell 1980:181). With over 400 dendrochronological dates now available, it is a well-dated locality. Its ceramics span the period between the black-on-white ceramics of the Coalition period (Santa Fe Black-on-white [B/W], Wiyo B/W and Galisteo B/W) and the earliest Rio Grande glazeware pottery (Los Padillas and Arenal Glaze-polychrome (G/P), and Agua Fria Glaze-on-red (G/R) (Eighth Ceramic Seminar 1966). Indeed, the earliest dates on Rio Grande glazeware (ca. A.D. 1315) were obtained at Tijeras Pueblo (Cordell 1975). The ceramics have been discussed in Cordell (1975, 1980), Warren (1980), Anschuetz (1984), Eckert (2006), and Habicht-Mauche (2011).

The site was partially excavated during the period of ca. A.D. 1971 to 1976 by the University of New Mexico field school under the direction of James Judge and, later, Linda Cordell. Despite the excellent field work, succeeding years saw only limited laboratory analyses of the collections. Rough sorting of artifacts and annual reporting occurred during field schools, but the only actual major publication was by Cordell (1980). Contributors to that small volume provided syntheses of their analytic work on different classes of artifacts. Helene Warren was one of the contributors, and her short account of the site's ceramics affords some conclusions, but little analytical detail and no raw data. Thus, despite its importance, it is surprising how little is known about the Tijeras artifact assemblages.

RESEARCH BACKGROUND

In recent years there has been much attention given to the production and distribution of Rio Grande glazeware pottery, for example, Habicht-Mauche et al. (2006). New studies dealing with the obtaining of glaze paint materials have shown that ceramics materials were often traded widely. Exchange of finished wares was also extensive during this time, and the measurement of this trade obviously depends on adequately identifying the production of each pueblo, or at least its local district, as was pointed out by Shepard (1942). While the transport of distinctly different types and wares affords an obvious line of evidence, exchange within the same ware/type sphere (such as Rio Grande glazeware or utility ware) is much more difficult to identify (Schleher 2010). For example, analyses of P IV glazeware

site assemblages on the Middle Rio Grande and its tributaries have been hampered by the inability to identify "internal" localized ceramic trade relations in any detail. Thus, due to the lack of detailed knowledge of Tijeras ceramics, it has been difficult to identify trade ceramics from Tijeras Pueblo when present as imported pottery within the assemblages of other contemporary pueblos. Analyses of ceramics from Pottery Mound (Eckert 2003; Franklin 2010), as well as Valencia (Franklin 1997), Montaño Bridge (Franklin 2008), and Chamisal pueblos (Franklin, in process) have attempted to elucidate inter-village trade relationships that were clearly complex during the P IV period. These efforts are hampered, however, by lack of detailed information about ceramic production at key locations, one of which is Tijeras Pueblo. Distinguishing pottery made specifically at Tijeras is difficult, since all the pottery types, painted and utility, manufactured here were also produced elsewhere. Potters in the Middle Rio Grande Valley, Galisteo Basin, and Pa-ako all made many of the same black-on-white and glazed pottery types as those of Tijeras Pueblo (Eckert 2006).

Trade ware from the western pueblos, especially the Little Colorado area (Wingate, St. Johns and Heshotauthla Polychromes) also appears at Tijeras, and certainly suggests strong contacts with that region. Painted wares from the west are generally typified by white to light buff pastes and potsherd tempers. Nevertheless, it is now seen that white clays similar in color to those employed in the Acoma-Zuni wares was also available locally to Tijeras potters. Furthermore, both Tijeras and the western wares utilized ground potsherd as their main temper ingredient. This makes it much more difficult to separate the "real" imported western pottery from the local Tijeras Glaze A glazewares.

PURPOSE OF THIS STUDY

A small study was designed to gain additional data on a small ceramic collection to confirm and augment Warren's (1980) report. In order to gain a glimpse of the materials incorporated into typical Tijeras pottery, and to be able to use this "signature" to identify it as intrusive in other contexts, I studied a small collection of early glaze and utility wares from various surface proveniences. At the same time, with U.S. Forest Service approval, samples of local clays were obtained from likely sources near the main site in 2009. Some tempering material, especially mica schist, was also collected from nearby sources. Matching ceramics to clays and tempers of the immediate environment would yield information on sources of potters' materials that could have been, and probably were, utilized in ceramic manufacture at Tijeras. No study of clay resources had been done previously, and only limited information on temper sources was available.

With these limited objectives, this study has shown some promising results, reported here. These are preliminary findings, however, in two respects. First, limited time in early 2009 prevented a thorough survey of all clays and tempers which may lie within the broader environment or catchment area around the site. Second, as of this writing in 2011, Judith Habicht-Mauche is currently undertaking a larger study of Tijeras ceramics which will be more comprehensive than this brief examination. In addition, Steven Rospopo is in the process of conducting X-ray tests at Sandia Laboratories. Therefore, this small study is to be seen only as a prelude to future projects.

COLLECTION OF CLAYS AND TEMPERING MATERIALS

Clays

Small samples of clays were collected at several locations in the immediate vicinity of Tijeras Pueblo in March 2009. An attempt was made to cover all the most obvious clay sources that might have been utilized by prehistoric potters within a close catchment area. Indeed, it has been known for some time that good quality red clay is available near the habitations.

Clearly, clays and tempers are probably available at greater distances, but the proximity of these sources makes it more likely that the potters would have preferred them.

Ten samples were obtained: eight from deposits in and around the drainage immediately north of the site, and two from across the highway (NM 337) in the road cut on the west side of the roadway. All came from surface exposures and are easily extracted with simple tools today, as they would have been in the past. Collection locations were recorded with photographs and UTM coordinates.

Samples 1-4 were obtained from red and white clay deposits outcropping in the wash immediately north of the main part of the site, in an area where it is believed early potters may have extracted materials (Habicht-Mauche 2010). Here, the main clay is a red to reddish brown material derived from decay of the Permian Abo and Yeso formation strata. These red beds of sandstone cover an area to the east of Tijeras (Bauer et. al. 2003:85; Connell 2006). The Abo formation contains mudstone, purplish to reddish brown, with some gray layers within it (Kelley and Northrup 1975:50). Their decay yields red clays, evidently utilized both here and the at the Salinas pueblos of Quarai and Abo to the south. Near Tijeras, veins of white clay are exposed in close proximity to the red beds, and afforded a source of material suitable for white slips.

Samples 1 and 4 are of the white material, while 2 and 3 are the nearby red clay. Moving to the east of the closest clay beds, samples 5 and 6 were collected from exposed red clays in the same drainage bordering the site to the northeast. Geologically, they have the same origins as samples 2 and 3, although mixture with silt and soil makes each locality slightly different in color and workability.

Downstream to the west, sample 7 has a more purple-red color. Sample 8 was collected from the drainage coming from the south, which passes the visitor center and parking lot (Cedro Arroyo or a tributary of it). Here the deposits are not as red, and are generally brown-tan in surface exposures. Clay here tends to be of lower quality than the red Abo beds.

Finally, samples 9 and 10 were collected on the west side of the paved highway opposite the visitor center. Here, the gray Madera limestone overlies earlier beds of the Sandia formation (Bauer et. al. 2003:182). Although these are harder and more compacted sedimentary deposits, they might have offered a greater variety of different colors than the main Abo red beds, above. Sample 9 is purplish-red; sample 10 is dark yellow-white in raw appearance.

Tempers

Potters typically needed to add aplastic materials to clays as temper to reduce drying shrinkage and to add strength to vessel walls. A variety of materials was used for this purpose in the Southwest, including crushed potsherds from old pottery and various lithic materials. At Tijeras, examination of temper materials has revealed that crushed potsherd was by far the most common temper in both whiteware and glazeware types (Habicht-Mauche 2010; Warren 1980). While other materials are sometimes seen, it appears that potsherd temper dominated in Santa Fe and Galisteo B/W, as well as the red-slipped glazewares of Glaze A times (Los Padillas G/P and Agua Fria G/R) that are common at Tijeras Pueblo.

Conversely, the utility pottery, including corrugated styles and plain gray, were customarily tempered with rock, not sherds. Here, the preferred rock material was micaceous schist, or phyllite schist, which appears as laminated chunks within the cross-section of utilityware sherds at Tijeras. Consistent use of this schist in utility pottery at Tijeras has been noted by others (Habicht-Mauche 2010; Warren 1980:156). Although Warren (1980:166) also recorded minor schist temper in the glazeware, none was observed in this sample. A bit of schist could become included in painted wares by way of sherd temper derived from utility vessels.

Geological availability of this schistose rock is also within the local potential catchment area of prehistoric potters. Exposures of schistose rock appear as bands or veins within the metamorphic rocks which

underlie the massive Sandia Granite of the Sandia uplift. These metamorphic deposits are revealed along the east side of the Manzano Mountains south of Tijeras Canyon and are clearly visible within the canyon itself. The metamorphic zone includes gneiss, schist, quartzite, phyllite and greenstone (Kelley and Northrup 1975:22). Modern road cuts at the edges of old Route 66 (NM 333) clearly reveal the metamorphic rocks in the lower canyon, typically of a brown to purple color (Bauer et. al. 2003:82). These are exposed only in the lower part of the canyon and are not seen in the upper canyon, as they are covered there by more recent strata. However, at "Deadman's Curve" on old Route 66, several large outcrops of schistose rock are visible and accessible. Several hand specimens of this rock were collected for comparison to pottery temper. The rocks vary from quartz-mica to mainly densely compacted laminar planes of gray-green phyllite schist. It is noteworthy that these are not simply scattered mica flecks in granite, which also occur in the region. This is a specific and identifiable material of local geological origin. According to geological maps (Connell 2006), the metamorphic exposures containing schist do not outcrop farther east than the cut at Deadmans's Curve. If so, then potters would have needed to travel about 2 mi down the canyon to collect materials they needed for utility pottery temper. Warren (1980:156) also recorded "schist rock a mile or two west of Tijeras Pueblo."

Small amounts of sand are seen in the pottery, and it often accompanies the major materials, sherd and schist. In the utility and glaze-on-red sherds observed here, sand was never the major ingredient, and it is possible some of it found its way into the paste clay inadvertently.

POTSHERD SAMPLE

A small comparative sample of 20 sherds, 10 glaze-on-red and 10 utility, were drawn from surface collections stored at the Maxwell Museum. These derive from at least six proveniences across the site, and thus are fairly randomly distributed. No sherds were believed to come from the same original vessels. This small group is clearly not adequate to represent the site's entire repertoire of ceramic types; but it is sufficient to determine whether the two dominant kinds of pottery could have been made using locally available materials.

TESTING OF CLAYS AND POTSHERDS

Clays

First, the clay samples were pulverized with a hammer. The dry color was recorded with reference to the Munsell Soil Color chart. Each clay was soaked for two days in sufficient water to achieve the best plasticity. Clay was then worked manually to evaluate plasticity and workability. Purity of the clay was estimated, and it was then formed into small briquettes and allowed to dry in open shade for several days. Briquettes were fired in an electric kiln in an oxidizing atmosphere (900° C for 10 minutes). Munsell colors of the fired briquettes were recorded in open shade. Photographs recorded the colors of the dry, and then the fired, briquettes. Shepard (1963) describes the theory and method of clay-ceramic comparison using oxidation techniques.

Potsherds

Each sherd in the sample was clipped, and the temper identified with a binocular microscope. Clips were fired in oxidizing atmosphere to 900° C for 10 minutes, as were the clay briquettes. Later the color was compared to a Munsell Soil Color chart by sunlight in open shade. Photographs were taken of sherds, clips, and temper inclusions.

RESULTS

Clay

Body clay of glazed pottery and utility wares made at Tijeras was red, while slips and paints utilized white, and possibly yellow, clays. Black-on-white pottery utilized white-gray firing clays, of course. Foot survey revealed abundant clay and temper resources available

Table 1. Tijeras clays collected in March 2009, near Tijeras Pueblo.

Sample	Natural Dry Color	Refired Color 900 degrees C.	Plasticity and shrinkage
1)	10YR 8/1 – 10YR 7/1 "white" "light gray"	10YR 8/1- 10YR 8/2	excellent
2)	7.5YR 5/4 – 7.5YR 5/6 "brown"	5YR 6/6	good
3)	7.5YR 5/6 "brown"	5YR 6/8	excellent
4)	10YR 7/1 "light gray"	10YR 8/2	excellent (same as 1 but harder)
5)	7.5YR 5/4 "brown"	2.5YR 5/8	fair (clay mixed with sand & soil)
6)	7.5YR 5/4 "brown"	2.5YR 5/8	good (clay is more pure)
7)	5YR 5/4 "reddish brown"	5YR 7/6	excellent
8)	7.5 YR 5/4 "brown"	5YR 6/8	fair
9)	7.5YR 6/2 "pinkish gray"	2.5YR 6/6	good (dense, hard)
10)	2.5Y 8/2 "pale yellow"	7.5YR 7/6	fair to good (shale clay, gray with some yellow)

near Tijeras Pueblo. Clays spanning red, brown, purple, yellow, and white and gray are available. Not all are equally pliable, and not all may have been utilized prehistorically. Nevertheless, clays matching those of the body and slip clays are definitely present in close proximity to habitation areas. Indeed, there is a wider range of colors in the raw clays than was employed in the finished pottery.

Table 1 gives the oxidized colors of the clay samples; Table 2 shows the colors of the potsherd sample. As is the case with oxidized firings, all the clays became lighter and redder after firing. This is caused by raising the iron-based minerals to their highest and brightest levels of oxidation; organic impurities are burned out. The white-gray samples (1 and 4) contain little iron, and so became simply white when fired. The pale yellow sample (10) from the road cut became a bit more intense. All the other red-based clays became more intense; on the Munsell scale, they started out on the 7.5YR page, and after oxidation ended up on the 5YR or even the 2.5YR pages, which are redder. At the same time, the chroma (saturation) and value (brightness) increased.

Comparison of oxidized sherds to oxidized raw clays is shown in Table 3. Here, the range of Munsell colors represented by both the clays and potsherds are listed, together with the name of the color group from the Munsell book. Colors in the table are arranged vertically in order of the pages, from 10YR (for the pale white-gray) to 2.5YR (red), and descending order of value (brightness). Next, clay sample numbers and their oxidized colors are given. Third, the corresponding colors of refired sherds of glazeware and utility ware are listed (20 in all).

Several trends are seen in this tabulation. First, oxidized clay colors, like their raw colors, span a wide range of colors from white to light yellow and various reds and brownish reds. The majority of the clay samples fall into the range of reddish yellow to medium red on the 5YR and 2.5YR Munsell chart pages. Two samples, however, (1 and 4), fired pale buff-white (10YR).

Second, the totals of the 20 sherds show a distribution that coincides with this range of raw clay colors entirely (Table 1). No pottery fired to a color lying outside the range of the clays tested here. Further, 12 sherds (both decorated and utility) match exactly with one of the Munsell colors of the clays. In another 7 sherds, the color difference was only a matter of an adjacent color chip from a clay sample; that is, on the same page, and only one chip apart. Only 1 sherd was more distant on the chart; it differed by only 2 color chips from the nearest clay. Thus, all tested sherds fell within the range of tested clays, and

Table 2. Tijeras LA 581, Refired Sherds.

Bag	Sample	Type	Refired Color	Temper
6975	1)	Agua Fria G/R	5YR 6/8	sherd, fine sand
6975	2)	Agua Fria G/R	10YR 8/3	sherd
6975	3)	corrugated clapboard	5YR 5/8	schist
6975	4)	plain gray utility	5YR 5/8	schist
6975	5)	smeared indented corr.	5YR 5/6	schist
6973	6)	Agua Fria G/R	5YR 6/8	fine sherd, sand
6973	7)	Agua Fria G/R	5YR 6/8	fine sherd, sand
6973	8)	plain gray utility	2.5 YR 5/8	schist
6973	9)	smeared indented corr	5YR 6/8	schist
6973	10)	plain gray utility	2.5YR 5/8	schist
6963	11)	Agua Fria G/R	5YR 6/8	crushed granite
6963	12)	plain gray utility	5YR 5/8	schist and sand
6963	13)	plain gray utility	5YR 5/8	schist and quartz
6974	14)	Agua Fria G/R	5YR 6/8	sherd, fine sand
6974	15)	Agua Fria G/R	5YR 6/8	sherd, fine sand
6974	16)	Agua Fria G/R	5YR 6/8	fine sand, sherd
6889	17)	Agua Fria G/R	5YR 7/8	sherd (red and white)
6889	18)	plain gray utility	2.5YR 5/8	schist and quartz
6889	19)	plain gray utility	5YR 5/8	schist and quartz
7000	20)	Los Padillas G/P	2.5YR 6/8	sherd and fine sand

most coincided exactly with a specific Munsell color chip, or nearly so. Clay samples 1 and 4 (white clays) were probably utilized for whiteware manufacture and white slips. The red-firing potsherd body clays of the glaze and utility wares cluster mostly with the clays from the red beds of the Abo formation (samples 2, 3, 5 and 6). As suspected, the high-quality clays that are also the closest to the pueblo appear to be the closest match to the majority of the 20 sherds tested.

Third, when the glaze-on-red sherds are separated from the utility sherds, some color differences appear. Overall, the utility sherds still fall into the same range of Munsell colors as the glazeware and the raw clays (Tables 1 and 2). However, minor differences are noted. Utility sherds tend to fire darker red than their glazeware counterparts. All are within the 5YR and 2.5YR hues, but the value and chroma tend to be lower (i.e., less intense and darker). While the utility ware was seemingly produced from the same red clay resources, the net result differs somewhat in color. This could be caused by several factors. Even if the clay selected were the same, the clay for utility purposes may have not been refined as much, leaving in more organic debris. Even with kiln oxidation, the organics can still affect the outcome. A second factor might be the inclusion of large amounts of schist, affecting the overall darkness of the potsherd cross section. Thus, it seems that basic clay resources were the same for both kinds of pottery, but that other factors ultimately yielded a darker and redder appearance to some of the utility ware examples.

The white clays (samples 1 and 4) could have been used for slip clays or body clays. The presence of substantial quantities of late P III whiteware pottery (Santa Fe, Wiyo, and Galisteo B/Ws) at Tijeras is well documented. Whether their white-gray color is due to reduced-oxygen firing practices or the use of a low-iron clay is not known. Further testing of whitewares would answer this question, since these types were not included in this study. However, it is now seen that

good deposits of white-gray clays were close at hand and could have been used to produce the whiteware bodies. White is also desirable for slip and paint on glazeware vessels. Used as paint on Los Padillas and Arenal Polychromes and as slip on San Clemente G/P, this white material would also have been valued. While essentially all the locally made glazeware utilized reddish body clay, one anomalous sherd (Table 2, number 2) has a white-gray body clay. This piece may represent an intrusive (possibly Kwakina Polychrome) from the Acoma-Zuni region, but the fragment is too small for certain identification.

The pale yellow clay (Table 1, sample 10) may not have been utilized by Tijeras potters, as it does not match body or slip clays seen on finished pottery. Presumably, it could have been used for a yellow slip, as known on Cieneguilla G/Y and polychrome. However, yellow-slipped pottery seems to not have been produced at Tijeras Pueblo, neither was it imported in any significant amount (Warren 1980:162).

Temper

Temper inclusions for the 20 potsherds are given in Table 2. All the glazeware sherds except one had potsherd temper as the main aplastic material. In several cases, small amounts of sand accompanied the potsherd temper. The single exception contained an igneous rock, probably granite. Potsherd is known to be the main temper inclusion in decorated pottery at Tijeras (Warren 1980:155). No micaceous schist was seen in any of the glazeware in this sampling, although Warren (1980:166) mentions schist as a secondary temper type in glazeware. Utility grayware pottery universally showed large chunks of micaceous schist as the major temper. Small amounts of fine sand sometimes accompanied the schist, and may have been an unintentional addition. Schist temper dominates regardless of corrugated or plain-surfaced utility style. This agrees with utility ware studied by Warren (1980:156). Tijeras shows a clear dichotomy between the painted and utility wares. Tempering materials were selected according to the ware being made. The nearly universal use of micaceous schist or phyllite schist in utility vessels suggests that the potters were aware of the advantages of mica in cooking vessels. The strength, durability, and heat retention are still recognized for micaceous cooking pots today, and these continue to be made by Pueblo and non-Pueblo potters today. The same desirable traits afforded by mica temper were appreciated prehistorically.

These tempering materials were all available in the Tijeras environment, as we have seen with the clays.

Table 3. Color Distribution of Tijeras Oxidized Clays and Sherds.

			Clays		**Refired Sherds**		
	Munsell Color	**Munsell Name**	Clay Sample No.	Oxidized Clay	Glaze/red	Utility ware	Total Sherds
	10YR 8/2	white	1, 4	2	1		1
	7.5YR 7/6	light reddish yellow	10	1			
light	5YR 7/8	reddish yellow			1		1
	5YR 7/6	reddish yellow	7	1			
	5YR 6/8	reddish yellow	3, 8	2	7	1	8
	5YR 6/6	dark reddish yellow	2	1			
	5YR 5/8	yellowish red				5	5
dark	5YR 5/6	yellowish red				1	1
light	2.5YR 6/6	light red	9	1			
	2.5YR 6/8	light red			1		1
dark	2.5YR 5/8	red	5, 6	2		3	3
Totals				**10**	**10**	**10**	**20**

Potsherd temper was as near as the local midden. At Tijeras, the mica is not in the form of loose flakes, but thick chunks of schistose laminar plates, as it comes from the outcrops. This material was available in the canyon, and is still seen outcropping in roadcuts along old Route 66. As noted above, the distance to this source is about two miles from the site, as also estimated by Warren (1980:156).

CONCLUSIONS

Technically, this preliminary examination relied only on Munsell color and binocular microscope. However, a basic template of Tijeras glazed and utility pottery emerged. The varied palette of local environmental clays would permit the construction of vessels belonging to all major pottery types known at Tijeras. Santa Fe B/W and Galisteo B/W required clay with low mineral content (samples 1 and 4) and firing without abundant oxygen. Glazeware utilized the reddish yellow to red clays for the vessel body. Glazeware also required fine red and white clays for slips, in order to produce Los Padillas G/P, Agua Fria G/R, and San Clemente G/P. The same basic red-firing clays were employed for the body of utility wares, with an emphasis on the redder end of the spectrum. Local resources could have supported the making of all these types. Although a yellowish clay (sample 10) was available, it is not certain if it was ever employed to produce a yellow-slipped glazeware (Cieneguilla G/Y) at this settlement.

Tempering materials seen in Tijeras wares are likewise abundant in the vicinity. The dichotomy between the tempers of service wares (sherd or sherd-sand) and utility gray wares (schist or schist-sand) is notable. Potsherds for temper were obviously abundant. Schist was readily available down the canyon at a distance of about two miles. Procurement of such basic body materials at some distances was probably commonplace at this time. The rarity of paint pigments and refined slip clays of certain colors promoted travel and materials exchange over long distances, as is well recorded in the Southwest. Body clays and desirable tempers also required some transport, however. For instance, P IV potters making glazed and utility wares with basalt temper were able to collect large quantities of this rock at distances of up to five miles from home, as recorded at Pottery Mound, Montaño Bridge, and Chamisal pueblos (Franklin 2010, 2008). In sum, this small study has confirmed what was known from Warren's earlier work. It has also shown that abundant clay resources and tempering materials were available to Tijeras potters, and match those used in glazed and utility pottery closely. Indeed, proximity of ceramic materials suggests that this factor may have been one of many which led to the choice of this location by Pueblo people.

DISCUSSION

Returning to the challenging question of the uniqueness of Tijeras characteristics, it was noted that the basic local pottery types are widespread in the Rio Grande glazeware realm. Furthermore, incomplete data from other P IV pueblos makes comparisons inadequate. It remains important, however, to know if the paste and temper attributes are unique enough to distinguish Tijeras manufacture. Can Tijeras pottery then be identified when present in the assemblages of other villages?

Certainly, it does seem that the vast majority of glazeware pottery made at Tijeras employed the distinctive local red to reddish-yellow paste clays. In this respect, they are remain similar to what is known of contemporary Glaze A glazeware production centers along the Middle Rio Grande Valley. The range of oxidized body clay color at Tijeras probably overlaps with that of other glazeware centers in the Valley, although the use of very red clays (2.5YR) may be exceptional. Consistent use of sherd tempers in the earliest Rio Grande glazeware is likewise not confined to Tijeras, although Valley potters adopted crushed rock, especially basalt, as a preferred material early in P IV times. At Tijeras, sherd temper remained dominant in glazeware until abandonment about A.D. 1425.

In utility wares, Tijeras maintained consistent

preference for the local mica schist, while Valley settlements preferred basalt or crushed granitic rock tempers. However, micaceous pastes and/or tempers are also found in Valley contexts, these are present in substantial quantities at most P IV towns in the Rio Grande Valley. The question of where all the micaceous utility ware was made and how widely it was traded remains to be answered (Franklin 2008; Warren 1981). In part, the solution lies in separating varieties of micaceous wares. While the Tijeras product is marked by large chunks of dense schistose rock, other micaceous wares may have mica flakes of muscovite or biotite scattered throughout the paste. In some cases, a micaceous clay (residual clay) must have had mica in it already, and mica was not added as a temper. Biotite mica is a common mafic mineral in the Sandia Granite (Kelley 1977), and residual clay deposits deriving from decomposition of such rocks may thus be micaceous in composition. Much more research will be needed to begin to identify the hard-rock and residual micaceous clay resources, as well as the pottery made from them in the Middle Rio Grande. Eiselt has made progress with micaceous wares of the Northern Rio Grande (Eiselt and Ford 2007), but much more needs to be learned elsewhere.

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Camp Joseph Smith: A U.S. Army Post in the Gallinas Mountains of New Mexico

SHARON HANNA

For several decades local residents have been aware of the mysterious arrangement of rock rubble at a location in the Gallinas Mountains. The formations resemble foxholes, which may have contributed to the legend that it was the site of a battle between Buffalo Soldiers and Indians. In the spring of 2005, while searching for a local historian who might have information about the connection between possible military camps and nineteenth-century Fort Stanton in Lincoln County, I was referred by the Lincoln County Historical Society to Walter Earl Pittman, a military historian, and to Charles Haecker, who has investigated many military sites and Apache encampments as a National Park Service archaeologist. That summer both Pittman and Haecker visited our ranch headquarters. Based upon artifacts found there, Haecker believed the headquarters had been an Apache as well as a military campsite. Later investigations revealed it was known as "Lagunitas."

Two years later the Torrance County Archaeological Society (TCAS) organized a field trip to the site in the Gallinas Mountains. Haecker and Pittman attended, along with Gary Cozzens, Archivist for Fort Stanton. After the trip, Pittman and Cozzens began researching Fort Stanton correspondence in the U.S. Army records at the National Archives and forwarding transcriptions of the letters related to the Gallinas Mountains to me for my research on the local sites. The history of the site related below is largely a result of their efforts.

Although the U.S. Forest Service had done a surface survey using volunteers with metal detectors at the site of the rock rubble depressions a few years earlier, in November 2008, Forest Service Archeologist Erin Hudson organized a two-day survey of the area with the assistance of TCAS members trained as SiteWatch monitors. This survey showed there was much more to study than our time permitted that weekend. With the cooperation of the Forest Service, I recruited TCAS SiteWatch member Mitchell Stucker and his wife Linda, who had previously done surface surveys for the Bureau of Land Management, to oversee a surface survey of the camp area in July, 2009. A summary report of our findings follows the history of the camp.

THE HISTORICAL RECORD

With the establishment of Fort Stanton in Lincoln County, New Mexico in 1855, the military needed roads to connect the new fort with other established military facilities. In March, 1855, Brevet Major James H. Carleton and a column of men were sent out to open a road from Los Lunas to the site of the new fort (Ryan 2003:4). Major Carleton built dams at water features along the way (2nd Lt. Thomas Henderson to Capt. G. S. Hollister, September 20, 1863, NAR). A significant water stop along this road was a spring in the Gallinas Mountains of present day Lincoln and Torrance counties. Pottery sherds and scarred trees indicate that the Gallinas spring

had long been a major source of water for Indian tribes traveling through the area.

References in military records point to significant military encounters with various tribes at or near the site. In August, 1861, during the occupation of Fort Stanton by Confederate forces, four Confederate soldiers manning a lookout on Gallinas Peak were attacked by Kiowa. One soldier survived and escaped back to Fort Stanton. A few days later, two of the dead soldiers were buried where they were found by the Confederate burial detail (Lt. R. Pulliam to Lt. Col. J. R. Baylor August 25, 1861, NAR). This incident and another with Mescaleros convinced Colonel Baylor that he could not fight the Union army and the Indians at the same time. He ordered the abandonment of Fort Stanton after occupying it for less than a month (Ryan 2003:41).

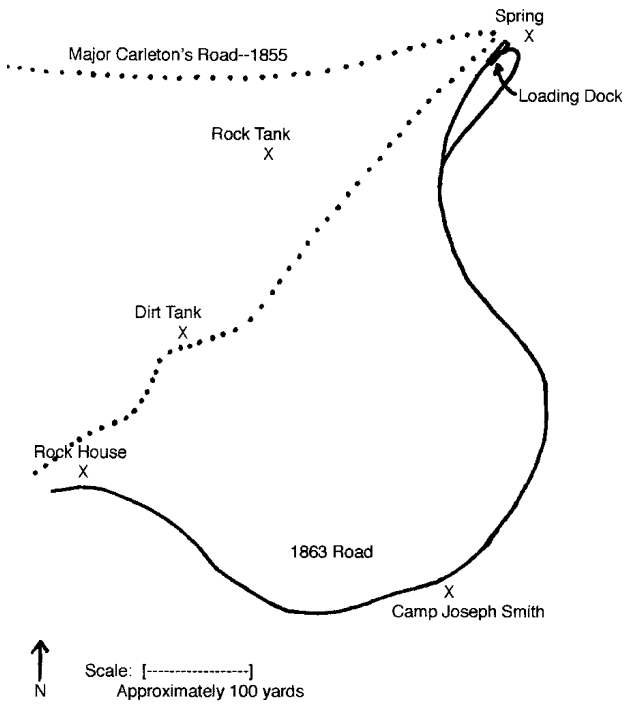
On October 18, 1862, Captain James Graydon, leading the First Mounted New Mexico Volunteers in the Union Army's return to Fort Stanton, attacked a group of Mescaleros camped near the west Gallinas spring. The Mescaleros had already agreed to surrender to Colonel Christopher [Kit] Carson and were on their way to Santa Fe to do so when they encountered Graydon and his men. The chief had previously surrendered his children to Colonel Carson, who had given the chief a pass and asked him to bring in more of his people. Graydon, who had just received General Carleton's order to kill all Apache and Navajo males, acted accordingly. At least 11 (Ryan 2003:47-48) or 16 Indians (Sanchez 2009:28-35) were killed, and several more wounded (Graydon to B. C. Cutler, Acting Assistant Adjutant General, October 23, 1862, NAR). Apaches removed most of the bodies before the next group of soldiers arrived at the scene; the remaining four bodies were buried by those soldiers (Maj. A. Morrison to B. C. Cutler, October 24, 1862, NAR). Apparently, none of Graydon's men were wounded in the encounter. The Mescalero realized that talking about peace would no longer be sufficient; to survive they had to surrender (Ryan, 2003:47-48). Another repercussion from this event led to a gun battle at Fort Stanton on November 5, 1862, resulting in the deaths of Captain Graydon and Dr. John Whitlock, a friend of Kit Carson (Thompson 1992:51-63).

In September, 1863, 40 soldiers from Fort Stanton were assigned to build a camp at the spring in the Gallinas Mountains and stay there for three months. The camp was named Camp Joseph Smith in honor of their commander (Capt. George Hollister to Lt. W. H. Higdon, October 13, 1863, NAR). The soldiers cleaned and developed the main spring, building a wooden flume to transport water down the hillside to a cement-lined rock reservoir. The reservoir, with a capacity of 30,000 gallons, took the men three days to build. On a ridge above the reservoir, log huts were built among lookout trees used for observation of the trails approaching the Gallinas Mountains (Maj. J. Smith to B. C. Cutler, October 4, 1863, NAR).

While the soldiers occupied Camp Joseph Smith, their objectives were to patrol traffic on the crossroads of trails in the area, develop water sources, make their presence known to the tribes still using the area, and to investigate the potential for mining for precious minerals (particularly gold) in the mountains, which apparently was a very significant reason for their presence according to reports by Major Smith (Smith to Cutler, October 4, 1863, NAR). The springs in the Gallinas Mountains continued to provide water for travelers and freighters until 1900, when the arrival of the railroad into the area eliminated the need for the former military road.

THE SURFACE SURVEY

On July 7-10, 2009, SiteWatch-trained volunteers Mitch and Linda Stucker, Evelyn Moran, Jack Woodson, and I performed a surface survey of Camp Joseph Smith and the surrounding area. The site surveyed included the main spring, wooden corrals and an adjacent concrete-lined rock reservoir, foundation rubble of huts on the ridge with lookout trees, and the remains of the rock walls of a nearby building (Figure 1). The wooden features are decaying and would be destroyed by a wildfire. Recording their existence was the primary goal of this survey. The survey crew divided the area into three sections: the camp on the



ridge, the water features (spring, flume and reservoir area with adjacent corrals), and the site of the rock structure. The area studied is roughly 1 mi long and over a 1/2 mi wide.

We mapped the area on the ridge first, locating several lookout trees and at least eight structure foundations. In his letter of October 4, 1863, Major Smith referred to the shelters as log houses that were about finished (J. Smith to B. C. Cutler, October 4, 1863, NAR). Appearances indicate that some structures may have been open on one side (Figure 2).

left–
Figure 1. Schematic map of the layout of Camp Joseph Smith.

below–
Figure 2. Remains of bases of log huts in Camp Joseph Smith. Local residents speculated that these rubble arrangements may have been battlefield foxholes.

Figure 3. The long, straight, lighter groove is the ditch that held the wooden logs of the flume in place. It now serves as a wildlife trail to the spring. The pieces of log at the bottom of the picture were part of the flume, but the picture doesn't show the hewn surfaces. There is also some hail screen near the bottom of the picture, which may have been used to filter the water coming from the flume into the rock reservoir (directly behind the photographer).

Figure 4. A side view of the "uphill" end of a segment of the wooden flume that carried water from the spring to the reservoir. This portion was placed under the lower end of the log above it; the water flowed through the hollowed log downhill to the next log.

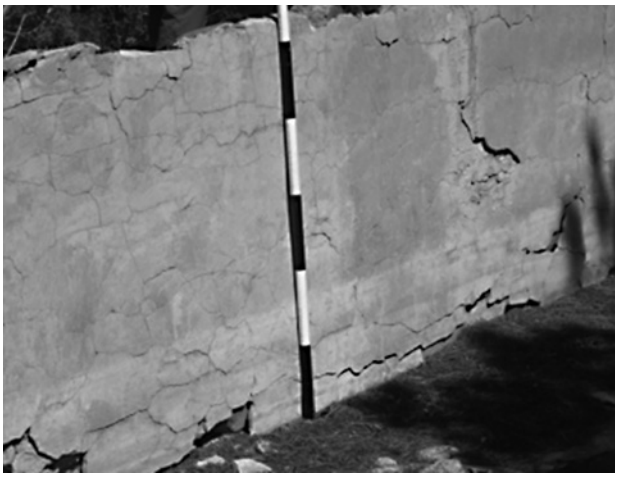


Figure 5. The interior of the rock reservoir. The measuring stick is 6 ft long. Calculations based upon measurements indicate the full capacity of the reservoir was 30,000 gallons.

Figure 6. Pipe extending from side of rock reservoir to supply a drinking trough in the corral.

Figure 7. A portion of the corral fence. The ends of the logs overlapped between the vertical posts. The ends are badly decayed, and the logs now lie on top of the logs below, making the fence much lower than the original would have been.

The original roads were traced as much as possible. The most prominent road runs through the camp site to the spring, where a turn-around looped across a loading dock below the spring. Vance Haynes had identified the loading dock during his visit to the site earlier in the year. A trench had been dug to open up the source of the water in the spring. The wooden supports for the loading dock are still in place near the lower end of that trench. Beside the spring is the remainder of a rock building 14 ft square with solid walls 2 ft thick, a short hallway, and a lower floor in the remaining room, much like an early-day root cellar.

Segments of the wooden flume are still visible between the spring and the reservoir (Figures 3 and 4). Water marks on the reservoir show that it held a considerable amount of water at times (Figure 5). Pipes extend through two sides of the reservoir (Figure 6), and a portion of a hollowed log used for a drinking trough still exists in one corral. The two large corrals near the reservoir were built from logs laid horizontally between vertical support posts with no nails or bolts used in the construction (Figure 7). Enough of the fence is still visible to define the boundaries of the corrals to investigators willing to crawl through the junipers and oak brush.

Down the ridge from the camp is a small clearing containing the remains of a rock building. According to local lore, the building was used as a ranch house and a school in the early 1900s. The dimensions of the walls match those used in buildings constructed at New Fort Wingate in 1868; all walls, interior and exterior, were 2 ft thick (Giese 1995:3).

A dirt tank dug below the spring and south of the reservoir washed out long ago; its berm now

contains mature pine trees. An obscure narrow road that connected the spring and the dirt tank continued up to the rock house. Portions of the military road into the site from both the south and the north can be seen coming into the area of the rock house and to the corrals.

Research continues into the military use of this site, and upon the significance of the site to the settlement of the Corona area. No graves were located in this survey, and the specific locations of the encounters with Indians have not been identified. Because events at this site influenced the Confederates' change of plans for New Mexico and later convinced the Mescalero Apaches to surrender to life at the Bosque Redondo, it is our feeling the that area is worthy of recognition on the State Register of Historic Places. ☙

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Why Did We Do It That Way? The University of Utah Glen Canyon Project in Retrospect[1]

WILLIAM D. LIPE

INTRODUCTION

Carol Condie and I both worked on the University of Utah portion of the Glen Canyon Archeological Project[2]—she as de facto editor of the University of Utah Anthropological Papers (though her actual title was Associate Editor) and I as a field crew member and crew chief. Carol spared not the red pencil when she worked over my attempts at reporting our field results. I learned much about how to write from her—lessons I have relied on throughout my career.

My colleague Don Fowler, with whom I worked in the field for three seasons, has just published his "personal memoir" of the Glen Canyon Project (Fowler 2011) set in a larger history of exploration and research in the Glen Canyon region. Don presents a wonderfully clear, informative, and entertaining account of how the University of Utah Glen Canyon Project was organized, how it worked, and what it was like to be a part of it. My contribution here is a minor embroidery on Don's history, focused on trying to shed a bit of light on the academic, cultural, and personal contexts that shaped the structure and conduct of what I'll hereafter refer to as "the UGCP" (thus distinguishing the Utah portion from that administered by the Museum of Northern Arizona).

The primary answer to the question in the title is "Because Jess Jennings said to do it that way." But it is a bit more complex that that. The obvious next question is why Jennings thought about it the way he did, and so forth. But the project also evolved during its lifespan, and the people who participated in it shaped it in various ways.

I'll provide my "take" on the intellectual context in which the project was developed, and on what seem to me to have been some of the assumptions that were built into it. I also want to talk about some of the organizational and methodological aspects of the research, and then close by considering a few of its deficiencies as well as its lasting contributions. My comments are based to a considerable extent on my reading of the project's published reports, but also on perusal of some of the archival materials held at the University of Utah. However, much of what I offer here is derived from my memories of discussions with Jennings and other project personnel, and from personal perspectives on why we did what we did, developed during the project and subsequently.

I'll start with a few words about how I came to get involved in the UGCP, and what experience I brought with me when I came on board. That last part won't take long. My first archaeological field experience was in 1956 at the University of Arizona field school at Point of Pines, directed by Emil Haury and Ray Thompson. While there, I demonstrated the ability to move a lot of dirt while not digging through masonry walls and still finding an adequate number of artifacts. That experience helped me land an assistantship at the Museum of Northern Arizona in the summer of 1957, working for Dave Breternitz. Toward the end of the summer, some of us drove to the Pecos Conference in Globe, Arizona, and it was there I heard Bob Lister of

the University of Colorado describe the first UGCP surveys, which he had led in the Escalante River canyons and adjacent areas. Lister's talk convinced me that I needed to become part of that project if I could.

My chance came in the late fall of 1957, as I was nearing the end of my first semester of graduate school at Yale. The American Anthropological Association meetings were in Chicago, and I and several other graduate students drove over in my 1949 Chevy with the broken window, and stayed on the floor of a dormitory at what was I think the University of Chicago Divinity School. I got an introduction to Jesse Jennings from my graduate advisor, Ben Rouse. Jennings had in fact included Rouse on a list of colleagues to whom in February 1957 he had sent a letter asking for names of students or others qualified to fill supervisory field or lab positions on the Glen Canyon Project (Jennings 1957a). I must have followed up with an application letter, and a few months later I was hired as a crew chief, to start in the summer of 1958. That summer job stretched to two and a half years, and gave me the opportunity to write two substantial fieldwork reports (Lipe 1960; Lipe et al. 1960). I returned to graduate school in the fall of 1960, and then came back for one more summer in 1961 (as a crew member, working for Floyd Sharrock). In 1962-63, after finishing classes at Yale, I returned to Salt Lake to assemble data for a dissertation on the Red Rock Plateau (a portion of the Glen Canyon area), which I finally completed in 1966 (Lipe 1966, 1970).

WHAT WAS THE INTELLECTUAL CONTEXT FOR THE PROJECT?

1) The taxonomic approach to culture history was still very strong in American archaeology, and what passed for a theoretical literature was dominated by often labored discussions about the right way to define types and whether they did or did not represent ideas once held in the minds of their makers (e.g., Ford 1954; Krieger 1944; Rouse 1960; Spaulding 1953). Definition of cultural phases also occupied a good deal of conceptual space (e.g., Rouse 1955), as did arguments about the definition and boundaries of large-scale archaeological "cultures" such as (in the Southwest) Anasazi, Mogollon, Hohokam, Fremont, Sinagua, and Patayan.

2) The "new archaeology" (e.g., as represented by Binford 1962, 1964) was still a few years away from conception, let alone birth. But there were a number of publications and currents of thought that were beginning to move American archaeology away from a predominantly cultural taxonomic/typological approach to one more concerned with function and environmental adaptation, not to mention the social characteristics of the communities and societies responsible for the cultural complexes that archaeologists were attempting to define and date. Still largely unexamined was the assumption that cultures were made up of widely shared norms, and that our job as archaeologists was to establish types of artifacts, structures, and sites that would capture the prevailing norms of a particular period and locality; this would serve to characterize the culture that occupied that particular chunk of time and space.

3) Walter Taylor's *Study of Archeology* had been published in 1948, nine years before the start of the UGCP, and although his attacks on the some of the leaders of the field stimulated most of the discussion, his ideas about a "conjunctive approach" (Taylor 1948) that would yield something like a cultural ethnography of the past, were quietly infiltrating the field. Analysis of animal bones, plant remains, and even pollen was also becoming more common in the archaeology of the American West, as sources of both paleoenvironmental and economic inferences. The same year the UGCP started, Taylor edited the report of an influential meeting sponsored by the National Academy of Sciences entitled "The Identification of Non-Artifactual Archaeological Materials" (Taylor 1957). Don Fowler (2011:226-227) discusses Lyndon L. Hargrave's earlier advocacy for using ecological information in archaeological research

and credits Hargrave for influencing Taylor's interests (Fowler 2011:229).

4) In 1938, Julian Steward had published a landmark study of how the sociopolitical organization of aboriginal groups mapped onto environmental variation in the Great Basin and adjacent portions of the Columbian and Colorado Plateaus (Steward 1938). In the 1950s, articles and a book setting forth the concepts of cultural ecology appeared (Steward 1955). Gordon Willey's pioneering Viru Valley settlement pattern study and his edited volume on New World settlement patterns were published before the start of the UGCP (Willey 1953, 1956). The concept of settlement pattern had begun to register in Southwestern archaeology, although notions about how one would study such patterns in the field and what one could infer from settlement data were still pretty undeveloped. In particular, excavation was still viewed as the principal, if not only, way to obtain data useful for interpreting the past, and survey remained largely relegated to the role of finding sites worthy of excavation.

5) In the middle 1950s, Gordon Willey and Philip Phillips published two articles in the *American Anthropologist* that were the basis for their book *Method and Theory in American Archaeology* (Willey and Phillips 1958). Their broad stages (Archaic, Classic, etc.) emphasized recurrent functional complexes of characteristics, rather than historically distinctive "cultures" (although they hedged their bets by calling the scheme "historical-developmental"). They also suggested that archaeologists needed to try to understand the social aspects of archaeological units (Willey and Phillips 1958:48-56), but still couched the question in taxonomic terms (e.g., whether "phase" could be equated to "society").

6) Jennings' "Desert Culture" concept was first published in a journal article (Jennings 1953) and his classic monograph on Danger Cave (Jennings 1957c) came out the year the Glen Canyon Project started. The Desert Culture was seen not as a taxonomic entity, but as a widespread "lifeway" that represented a persistent cultural and social adaptation to the harsh environments of the interior West. Interregional similarities rather than differences were stressed. The Desert Culture eventually was folded into the Western Archaic, but Jennings' culture-ecological insights helped ensure that later attempts to characterize the Archaic went beyond just listing artifact types.

I think all of this work was known to Jennings, though the field archaeologists who made many of the decisions on the project were for the most part less well grounded in the literature. I'm sure that I had never read Willey's work on settlement patterns, and was only vaguely familiar with Steward's approach to cultural ecology. Within a few years of leaving the Glen Canyon Project, however, I had produced a dissertation based on a cultural ecological analysis of settlement patterns in the Red Rock Plateau portion of the Glen Canyon area. If I had been better grounded in the hot new theoretical literature of the late 1950s, I might have made some more insightful decisions in the field and as I was writing up my field reports, but I see that my battered copy of Willey and Phillips is signed "Lipe 1960" and my copy of Steward's book shows "Lipe 1961"—indicating that they were purchased after I had left the project and returned to graduate school.

WHAT WERE THE THEORETICAL AND EPISTEMOLOGICAL ORIENTATIONS OF THE UNIVERSITY OF UTAH WORK?

The intellectual climate that prevailed in American archaeology in the 1950s surely influenced the way the UGCP did its work, but these influences generally seem to have operated implicitly, as a kind of "normal science" that structured the choices the UGCP archaeologists made regarding what sites to work on, and how to interpret the evidence.

Insofar as I can tell, there never was a detailed research design for the UGCP organized around substantive questions about what had happened in the past in that region. In a brief introductory history of the project, Jennings (1959a) does provide what he labels as a research design, but it is one that focuses largely on the organizational aspects of the project, and on very broadly stated objectives, with the most important ones being:

> 1) The project is aimed at an intensive, thorough, mile-by-mile search for sites in the area; leading to a
>
> 2) complete sampling of cultures represented in the area, by test and extensive excavation, resulting in
>
> 3) a flow of descriptive reports kept current with field work…. (Jennings 1959a:13)

"Scientific problems" were to be of secondary focus, with the "salvage aspect" of the project to be primary.

Jennings (1963b) was quite positive about the value of "salvage" archaeology, and believed that the primary job of the salvage archaeologist was to collect the full range of data from the full range of sites that were going to be lost. He saw problem-oriented research as necessarily being more selective in the choice of what sites would be excavated or what classes of specimens and information would be collected from them.

> Any salvage operation has special objectives [as compared with problem-oriented research]. It is impossible for the technical staff to concentrate work on one problem, or one time period, or in some special aspect of a problem or a time period. Total recovery is the objective; this means total sampling of all cultures, and all time periods, to be found in the area. (Jennings 1959b:681)

Jennings was somewhat ahead of the times in referring to "sampling" at all, but what this might mean in practice remained obscure, and certainly did not refer to an application of statistical sampling theory. Although Jennings saw the primary obligation of a salvage project as acquiring as unbiased a sample as possible of all the cultural manifestations present in a given area, he recognized that those responsible for the fieldwork must be aware that the ultimate value of their work would depend on the usefulness of their data in addressing general problems of historical or scientific interest.

> It seems necessary, at all costs, to plan and execute the project so that the immediate salvage problems are adequately dealt with and simultaneously the scientific implications of the data are correctly understood, assessed, and interpreted. (Jennings 1957e:1)

> It is not enough that competent craftsmanship in data collection be achieved (although this would satisfy the technical aspects of contract fulfillment but every precaution should be taken to make certain that the scientific implications of the mountain of crude data being accumulated through routine field operations be recognized, assessed and (if possible) interpreted. For example, the sites selected for excavation or any areas chosen for resurvey or revisitation or sites chosen for excavation which are peripheral to the major search areas, should be selected in part because they appear to be capable of contributing to clarification of specifically identified problems as they develop. (Jennings 1959a:10)

In his progress reports to the National Park Service (e.g., Jennings 1957d, 1957e), Jennings repeatedly refers to some broad substantive problems that needed

to be addressed. These focused on the chronology of occupation in the region and on the kinds of cultures that were represented. Included were the search for solid evidence of preceramic occupations, defining the spatial and temporal relationships among major Formative-level "cultures" (Mesa Verde, Kayenta, and Virgin Branch Anasazi, as well as Fremont), and using archaeology to expand knowledge of later cultures in the area—Paiute, Navajo, and historic Anglo. He frequently mentions the difficulties posed by the lack of comparative data from surrounding areas, due to a lack of previous archaeological research, especially to the west and north.

Jennings understood that "facts are derived from observations, and observations are always made to some scale and within some context, either explicit or assumed" (Jennings 1963a:16). In practice, however, there was little explicit discussion of what observations the fieldwork should be expected to yield, and the concepts of "collecting raw data," "complete recovery," and "descriptive reporting" come up repeatedly in his progress reports and, as I recall, in conversations. These contributed to an implicit expectation that archaeological data could be generated from the physical archaeological record in a fairly straightforward and even self-evident way, if appropriate field methods were used and archaeologists kept their eyes open.

Jennings knew that given the scope and logistical difficulties of the UGCP, much of the day-to-day fieldwork decision making (usually including which sites to test and, in some cases, which sites to dig) would depend on the knowledge and judgment of the field supervisor.

> In reservoir salvage work there is a heavier burden on the field supervisor than in any other kind of archeology, since the completion of the dam and the filling of the reservoir precludes further opportunity to check, verify or add to the information acquired through fieldwork. (Jennings 1959b:681)

Jennings necessarily relied on the judgment of the field archaeologists (and more generally, on the "best practices" for survey and excavation that were current at the time) to dictate how the archaeological record would be investigated at particular sites, and in fact, how sites would be selected for what level of investigation. He expected that an understanding of the prehistory of the project area would flow from the direct encounter between perceptive archaeologists and the archaeological record, both in the field and in the lab. And he hoped he had hired archaeologists who were perceptive enough to get the most relevant information for the time and money expended.

If we look at some of the choices and interpretations that were made during the project, there is evidence for an underlying theoretical framework that could be considered culture-ecological, though this was seldom explicit. The inclusion of several kinds of biological and geological studies as part of the project indicated a concern with obtaining data both from present-day and past environments in order to help place the prehistoric cultural manifestations in ecological and adaptive contexts. In the interpretive papers he published on the project, Jennings (e.g., 1963a, 1966) clearly takes a generalized functional and ecological approach rather than the taxonomic one that was still dominant in Southwestern archaeology. That is, he spends more ink on how people lived in relation to their environment than on whether the sites in question were Pueblo II or III or Kayenta or Mesa Verde Branch, or whether the pottery in them was Mancos or McElmo Black-on-white.

Another indicator of a broadly culture-ecological approach is that, unlike projects today, the fieldwork—including excavation—was not confined just to the area directly impacted by development—in this case, by reservoir construction. Rather, a broader regional approach was taken, with surveys and excavations conducted well outside the reservoir boundaries. This approach was explicitly encouraged by Charlie Steen, the Park Service's archaeologist in charge of overseeing the project:

> You should not concern yourself only with the land which will actually be flooded, but should extend your surveys, wherever possible or desirable, to and somewhat beyond the canyon rims. We shall not attempt to set a specific limit, or to ask you to search for sites to a line a certain distance from the rims, for there can be no closely followed procedure in such terrain. I believe you should instruct your Chiefs of Party to look over all the lands adjacent to the canyon which give promise of being of importance in any field of investigation. We are very anxious to know just what archeological, historical, and biological resources exist in the Glen Canyon region. (Steen 1957; also quoted in Jennings, 1959a)

One assumption that seemed to underlie this approach was that the small Puebloan sites located in the canyons that would be flooded needed to be understood in relation to larger sites in the higher surrounding areas or at least in terms of the cultural influences that presumably emanated from more populous areas. This assumption was more explicitly developed by the Museum of Northern Arizona's project team, but I believe it structured the University of Utah approach as well, at least at the outset. Jennings was able to implement work in neighboring upland areas (e.g., excavations on the Kaiparowits Plateau and in Harris Wash in the upper Escalante [Fowler 2011:294]) by obtaining non-project funds from the Wenner-Gren Foundation, the University of Utah Research Fund, and the National Science Foundation, and by arranging for the University of Colorado's 1959 field schools at the Coombs site. Today, we would more explicitly use the concept of regional settlement system to relate the highland and canyon sites.

WHAT WERE THE DISTINCTIVE METHODOLOGICAL AND ORGANIZATIONAL ASPECTS OF THE UGCP?

In contrast to the broader intellectual underpinnings of the project, its organizational and methodological aspects were more explicit. The UGCP field manual, published in 1959, is quite instructive, as are several papers Jennings wrote in the 1960s on the administration of contract salvage projects.

Jennings welcomed the definitiveness of the contract salvage format, because it required that a certain amount of money would be allocated to achieve certain results or produce certain products within a specific amount of time. This approach is one he was familiar with from his varied work career in the non-academic world, and he saw no reason why such common-sense requirements would not apply to a research project as well. Jennings is probably the finest research administrator I have ever encountered, bar none, and a large salvage project gave him the opportunity to show what he could do. It is also clear to me that he saw some of the academic research of the time as little more than undisciplined dawdling, and felt that some of his colleagues were more wedded to particular techniques or to achieving spurious levels of precision than to figuring out how to learn the most from the archaeological record with the least expenditure of time and money.

Several principles seem to me to have structured Jennings' approach to successfully carrying out an archaeological salvage project under contract.

1) "Use the coarsest tool which will do the work—i.e., recover the data" (Jennings 1966:7).

2) "My preference is to get 95% of the data from ten sites instead of 99% from one" (Jennings 1963b:263).

3) Although troweling and screening are sometimes appropriate, routinely excavating with trowels and screens instead of shovels and no screens is usually a waste of time, in terms of the amount of information obtained per unit of effort expended. (In his summary report on the UGCP, Jennings unleashes a final rant against the "spurious accuracy" achieved by "the slow brushing away of a site with trowels and the plotting of each scrap…" [Jennings 1966:6]). Not envisioned was a role for screening in promoting comparability of the artifact samples collected during excavations.

4) A well-coordinated team of full-time workers (in both field and lab) is better than a single individual working the same total number of hours over a longer period (see especially Jennings 1963b:284).

5) Maintaining data quality is essential, but the level of quality sought should be appropriate. Perfection is not achievable and the attempt to achieve it is a waste of time.

6) In the area of data quality, the field record is preeminent. A researcher can always reclassify the artifacts that have been collected, but he or she cannot not go back and re-excavate the portions of a site that have already been dug. (On a reservoir salvage project, this was, of course, true even for the unexcavated portions of sites).

7) If you want to achieve data comparability among several research teams and over the life of a multi-year project, it is essential to take explicit steps to ensure that this happens; otherwise, it won't. Thus, most of the pages in the project's operational manual (Jennings 1959b:687-707) are devoted to discussing forms and procedures for record-keeping in the field and lab.

8) Fieldwork unreported is equivalent to fieldwork never done; furthermore, it has resulted in the destruction of a site with no resulting gain in information about the past. "Unpublished data don't exist" (cited in Fowler 2011:272).

9) Report deadlines are essential—"This puts the burden of completion in sharp focus from the very beginning of the project. Thus it…establishes [a]… schedule to which the work must be geared" (Jennings 1963b:284).

10) The principal product of a salvage project will be descriptive reports, produced in a timely fashion, that put basic data on record. "…preoccupation with extensive comparisons, synthesis or interpretation must be deferred or held as incidental until the gathering of data is curtailed by filling of the reservoir…" (Jennings 1959a:9).

11) Archaeological sites and artifacts don't belong to the individual archaeologist, and neither do the archaeologist's records. Records of archaeological field research must remain available for future use.

Jennings was also strongly committed to not prejudging the evidence—and wanted to avoid setting up a conceptual structure that would determine conclusions in advance. This was expressed, for example, in the feature system he designed for taking notes (Jennings 1959b:692-693). Features were numbered so they could be tracked, but otherwise, they were "empty" containers for observations. A feature was anything in the archaeological record about which you wanted to record observations—it could be a stratum, a possible intrusive pit, a discolored area where a fire might once have been built, a structure, an area of the site, or whatever. Interpretive names, such as "hearth" or "kiva" were assigned only after all the observations had been made, and the archaeologist had had the time to reflect on them. The point was to separate observation from inference. The notes on a particular feature were kept open and could be added to until work on that feature had been completed.

I believe this approach implicitly operated at the project as well as at the site level, and was designed to keep project archaeologists from jumping to premature conclusions about the chronological, cultural, and functional relationships of the sites

they were excavating. The first job was to encounter the archaeological record by excavating and studying an unbiased sample of whatever was in the project area. On the other hand, in some cases it provided an excuse to attempt no interpretations beyond those necessary to give artifacts and features descriptive or typological labels.

Jennings' approach to excavating and to taking field notes also reveals a concern for what would now be called site formation process. In his instructions to field supervisors, he urged designing the excavation to efficiently explore horizontal and vertical relationships between cultural and stratigraphic features. He abhorred rote excavation by grid square and arbitrary level, seeing it as an excuse for not thinking about how best to expose and understand the relationships observable in the archaeological deposits. He preferred trenching to pit excavations, because trenches allowed one to connect different areas of the site and also to expose vertical relationships. He ridiculed the then-common Southwestern practice of publishing cross-sections of structures that showed the architecture, but not the strata that had once filled them, so there was no way to infer how they had filled. I recall him remarking "But what happened to the dirt?"—whether he actually said just those words or not, that critique came through loud and clear.

Crew chiefs were expected to regularly write summaries of what they were finding out about the archaeological record of the site they were working on. This was done in "Feature 1," a running journal in which all the features recently excavated were mentioned, and their stratigraphic and spatial relationships discussed. The purpose was to force archaeologists to think about how the archaeological record had been formed, while they were still in the field. If the work was far enough along, interpretive names could be tentatively assigned to features, and the chronological and functional interpretation of the archaeological record could begin.

As a result of the above-mentioned concerns with data quality, there were fairly elaborate systems in both field and lab that were designed to ensure that the provenience controls assigned in the field were maintained through analysis and into curation. One of Jennings' signal contributions to data quality was to read all the field notes, copies of which his crew chiefs sent in every two weeks. If you have ever managed a large project, with multiple crews in the field at the same time, you know what that took in terms of discipline on his part. But it was very effective—receiving a succinctly stated note from Jennings pointing out some inconsistency or outright blunder in your field notes certainly got your attention. On the UGCP, steps taken to promote data comparability included having standardized recording forms in both the field and the lab; having one person (ordinarily the crew chief) take most or all of the notes on the site; using standard artifact classifications across all sites; and labeling artifacts individually and classifying them without taking provenience into account, so that locations and associations would not bias assignment to classes. This allowed all the artifacts from a site (or even several sites) to be spread out on a table, then pushed into piles representing classes or types, and then tabulated by provenience.

Jennings placed prompt analysis and the timely publication of descriptive site reports on an equal footing with the fieldwork itself, in keeping with the above-mentioned principle that if the results are not published, all the other steps may as well never have occurred. He successfully fought for funding of analysis and reporting (including publication of the reports) in an era when salvage contracts often supported only the fieldwork, on the frequently unfounded presumption that academic researchers or their students would eventually do the follow-up work. Jennings did not believe, however, that it was the federal government's responsibility to pay for problem-oriented studies that went beyond descriptive reporting.

In order to carry out the reporting obligation, a rigorous schedule was set up to produce and disseminate inexpensive descriptive reports. While requiring the reports to have thorough (by the standards of the day) descriptions of the archaeological contexts, Jennings recognized that

he had to forestall what he saw as the reluctance of some archaeologists to leave anything out. This was particularly clear with regard to the issue of lumping versus splitting proveniences when reporting the horizontal and vertical distribution of artifacts at sites. Jennings was clearly a lumper, and promoted the principle that if two proveniences did not differ enough to indicate a chronological or functional separation, they should be lumped when reporting the artifacts that came from them. This resulted in a remarkable simplification of data tables.

The previously mentioned concern to ensure that archaeological collections and records be preserved for the future resulted in a significant investment, during the life of the project, in seeing that field and lab notes, maps, photos, other records, artifacts, and other specimens were ready for long-term curation. And in fact these materials have been preserved and are accessible for research and educational purposes. The collections are housed in the Utah Museum of Natural History, but some of the paper records are at the University of Utah archives and the University Archaeological Center.

IN HINDSIGHT, WHAT WERE THE MAIN DEFICIENCIES OF THE UGCP?

It is, of course, easy to critique procedures instituted and work done on an archaeological project carried out 50 and more years ago and often in very difficult logistical contexts. Some of the problems that the UGCP faced "came with the territory"—that is, were the result of events beyond control of the project personnel. Others, however, were at least in part the result of some of the assumptions and overall philosophy that undergirded the project—factors that, in many cases, also contributed to its successes.

The UGCP began before even the 1960 Reservoir Salvage Act was in place, let alone the 1966 National Historic Preservation Act or the 1974 Historic and Archeological Preservation Act. The only legislative authorities for the project were the Antiquities Act of 1906 and the Historic Sites Act of 1935. Because of the scale of the project, it was dependent on annual appropriations by Congress; hence, there was considerable insecurity from year to year regarding how much money would be available, and there was always the threat that the project would be terminated mid-course. Jennings (1963b:285), however, saw that a long-term contract could overcome this disadvantage by allowing funds not expended one year to be rolled over into the next. Thus, as a prudent project manager, he was able to build up a cushion against the vicissitudes of the annual appropriation process.

A significant operational problem present from the outset was the lack of systematic survey data for many parts of the project area prior to the start of full-scale work. Thus, the contracted schedule for the project demanded that in some locales, survey, testing, and excavation had to be undertaken in the same field season. There had been some prior survey, mostly in the Glen Canyon proper (see Adams 1960; Adams et al. 1961; Fowler 1959a, 2011). However, coverage of the areas to be impacted was very incomplete, especially in the tributary canyons and even more so in the adjacent areas outside the full pool level. For example, Don Fowler spent most of the summers of 1958 and 1959 surveying (with one helper) the main stem of the Glen Canyon and the lower parts of the tributary canyons (Fowler 1959a, 1959b). At the same time, I was directing the "river crew" in excavations. We worked out of the same field camp to the extent that Fowler's survey locations permitted. The same pattern of survey concurrent with excavation was repeated when we moved to portions of Moqui and Lake Canyons that were too far upstream to access from the river and had to be accessed overland. Away from the canyons, some of the UGCP surveys were clearly designed as exploratory reconnaissance, although the survey of portions of the Kaiparowits Plateau (Gunnerson 1959a) was intensive (by the standards of the day).

Would it have made a difference if the survey of the project area had been completed in advance?

Possibly, but only if the survey data had been subjected to more intensive chronological analysis and if it had been used more systematically as a way to develop hypotheses about site types and settlement patterns for particular periods and locales. As noted earlier, this role for survey data was still in its infancy in American archaeology in general, not just on the UGCP. As it was, we were usually able to plan excavations for a particular season around survey data obtained the previous season or earlier (e.g., Lipe 1959). The surveys I am most familiar with are those carried out by Fowler and later Kent Day in the Glen Canyon Main Stem and the Red Rock Plateau. The records and collections from these surveys are superior to those of most contemporary surveys in the Southwest, in my opinion. I was later able to make good use of this survey data (along with the excavation results) in developing a phase scheme and settlement pattern interpretations for the Basketmaker II through Pueblo III periods in the Red Rock Plateau (Lipe 1966, 1970).

However, there is no question that the body of survey data generated by the UGCP has gaps and biases. Schroedl and Newsome (2000),[3] on the basis of resurvey of portions of the UGCP area and a review of its records, note that some of the sites missed in Lake and Moqui Canyons

> …were small, open Anasazi structural sites located on small prominences within the canyon. These sites were missed by the GCP inventory teams because the crews were focused on searching for Anasazi sites in alcoves and along the canyon walls. (Schroedl and Newsome 2000:43)

A larger bias was that "the Archaic occupation of the benchlands and uplands surrounding Glen Canyon also went unrecognized" (Schroedl and Newsome 2000:43). This is undoubtedly true, although the presence of such sites was certainly known to the UGCP team. In a memo to the Park Service assessing the results of the 1958 field season, Jennings notes:

> …the consistent presence, from Comb Wash to Wahweap Creek, of a strong lithic complex associated with dunes or rock terraces. All these sites had previously been dismissed (when noted) as chipping areas adjunct to the Anasazi remains. Many, no doubt, are just that. Others, however, can be recognized as having distinctive artifact lists and other traits (location, size, etc.) which mark the existence of a separate complex. Typologically, however, no staff member recognizes the artifact types. No age ascriptions are warranted, but there does appear to exist a non-Anasazi occupation. It seems to be something new; while the complex may be earlier than the dominant Anasazi it could equally well be post-Anasazi in age. (Jennings 1958:3-4)

Unfortunately, the UGCP staff was never able to get much beyond this state of puzzlement. In their defense, most of the surrounding "benchlands and uplands" were above the full pool level, and given the huge area staked out as of interest to the project, they received less attention. I also think that the prevailing approach to doing archaeology overwhelmingly favored excavation, and to the field archaeologists charged with getting the most information for the time available, the lithic sites outside the canyons appeared largely surficial and probably "not worth digging" despite Jennings' repeated expressions of interest in learning more about them. The prevailing emphasis on excavation as the primary source of reliable information perhaps reinforced this attitude.

I think an underlying problem, however, was a largely implicit assumption by the project staff that the occupation of the Glen Canyon area did not have a great deal of time depth. This was a common attitude in the Southwest prior to the 1950s, and by the time the UGCP got underway, only a few papers had appeared reporting on probable pre-agricultural complexes in the Four Corners area (e.g., Bryan and Toulouse 1943; Mohr and Sample 1959).

More importantly, however, the UGCP as a whole did not focus at all effectively on chronology in a methodological sense. For the preceramic part of the record, some expenditure of project funds on radiocarbon dating would have paid substantial dividends. As it was, only one radiocarbon sample was dated (Geib 2006:37) and that one (from the Lone Tree Dune Basketmaker II site [Sharrock et al. 1963]) was submitted as part of my dissertation research after the field phase of the UGCP was completed. Radiocarbon dating was not developed until 1949, but by the early 1950s, it had begun to be used by archaeologists all over the world (Johnson 1951) and had played an important role in acceptance of the early dates from the lower levels of Danger Cave. Dendrochronology was of course an established method for dating contexts of the later periods in the northern Southwest. However, virtually all the sites excavated or tested by the UGCP were at too low an elevation to yield coniferous wood samples suitable for tree-ring dating; samples from a few higher elevation sites were submitted, but did not date. An exception was the Coombs Site (Bannister et al. 1969:12-13; Lister and Lister 1961), but here only a few samples were submitted, even though a number of burned rooms had been excavated.

The UGCP's emphasis on "descriptive" reporting of "raw" data, with the postponement of problem-oriented studies until later, may also have contributed to a lack of focus on chronology. The goal was to get a publishable descriptive report of one season's fieldwork done before the next field season started. This provided an excuse to put off addressing some of the harder issues, of which dating non-ceramic assemblages was one. I also think those of us doing and reporting on the fieldwork took too much to heart Jennings' (1959b:686-687) assertion: "Excavators are further reminded that the data, the sequence of events, and the relationships at any given site are observable. They are in the ground and must be dug for." Thus, we may subconsciously have been expecting to find another Danger Cave to convince us that an Archaic occupation was in fact present.

There was also a tendency—again largely implicit—to favor functional over chronological explanations for differences among assemblages from the same site or from a set of sites in a given locality. For example, in my report on 1958 excavations at Jug Shelter, I conclude,

> The absence of pottery in association with the stone tools and debris in the lower levels of Jug Shelter may be due to the presence of a preceramic distribution below a ceramic one. More probably, however, the distribution of artifacts reflects a changing emphasis from chipping to camping. There is no stratigraphic break between the non-ceramic and ceramic levels. (Lipe 1960:24)

("Chipping station" was a frequently used label for surface lithic sites, and one that was fairly frequently employed to characterize a site type.) In the same volume, I also used the lack of a clear stratigraphic break to question whether the non-ceramic midden levels at Lizard Alcove represented a significantly earlier occupation (Lipe 1960:83). However, when Geib (2006:35-38) much later examined the artifacts from this site, he identified both Basketmaker II and Archaic sandal types from the preceramic levels. Similar examples can be found elsewhere in UGCP reports.

In some cases, interpretations of non-ceramic levels were simply not offered. The report on 1957 excavations at the Alvey Site offers essentially no discussion of chronology, other than to note that excavations had "revealed cultural material to a depth of 12 ft." (Gunnerson 1959b:50), and that the lowest of three levels lacked pottery. Table 10 (Gunnerson 1959b:92) shows that specimens of maize were found in all levels. In an earlier report to the Park Service, Jennings (1957d:8-9) had observed about this site "The non-pottery basal level (if it proves to be actually pre-pottery) will presumably equate with early Basketmaker II or even with the late phases of the Uncompahgre complex." Radiocarbon dates

on samples from the lower levels at the Alvey Site subsequently confirmed occupation in a Basketmaker II time frame (Geib 1996:17).

Post-UGCP fieldwork and reanalysis of collections made by the project has shown, of course, that there was relatively abundant Basketmaker II and not insignificant Archaic occupation in the Glen Canyon area (Geib 1996, 2006). To the credit of the UGCP, several Basketmaker II sites were identified and reported as such (e.g., Honeycomb Alcove, Bernheimer Alcove, and Rehab Center in Moqui Canyon, and Lone Tree Dune and Greenwater Spring in upper Castle Wash) (Sharrock et al. 1963). Geib (1996:27) tabulates a substantial number of dates from the late centuries B.C. and early centuries A.D. for the Glen Canyon region, quite a few of them run on samples of maize. This indicates that an Early Agricultural period occupation was probably more widespread in the UGCP area than was recognized at the time. His conclusions were based in part on restudy and dating of specimens from the UGCP.

I've speculated (as noted in Geib 2006) that the prevailing view of the Glen Canyon area as "marginal" for Puebloan farmers was implicitly generalized to an expectation that it would have been even more marginal for foragers or semi-agriculturalists (which was how Basketmaker II was perceived at the time). However, I could find no specific statement of this in the published or archival materials I reviewed—it's just a hunch based on my recollections of how we tended to think about questions like this. An additional reason for our lack of success in defining Archaic occupations may have been that projectile points of styles clearly predating the Basketmaker II period were quite rare, as a glance through the artifact illustrations in the UGCP reports shows.

I think if Jennings had been doing the fieldwork himself, his encounters with the archaeological record would probably have led him to recognize that in some cases he was dealing with significant time depth, but crew chiefs as inexperienced as I was lacked that perspective. In retrospect, it would have helped if there had been a project design that provided some concrete expectations about how cultural variations due to chronological differences would be distinguished from those due to differences in site function.

A lack of sharp focus on chronology also characterized some of the interpretations offered for the Pueblo period sites. For example, in the report of the 1961 excavations, a number of reasons are given for not providing fine-grained date estimates:

> The problem involved in assigning dates to Castle Wash and Moqui Canyon is much the same as for other areas of the Glen Canyon…no beams suitable for dating have been located; radiocarbon dating latitude is impractical for the relatively short time involved; ceramic dates for several reasons are of dubious value for dating Glen Canyon sites because of a) some presumed lag between their inception at culture hearths and the arrival of the style or actual vessel in Glen Canyon, b) holdover of the style after abandonment at the culture hearth, c) possible reuse of abandoned vessels at a much later time.… (Sharrock et al. 1963:18)

However, I had earlier been able to successfully use Kayenta tradition pottery types for a simple graphical seriation of assemblages from sites excavated in 1959; I also assembled credible date estimates for these sites based on the standard pottery type dates published in the Southwestern literature (Lipe et al. 1960:4-9). If the factors listed by Sharrock et al. (1963) had been heavily in play, this should not have been possible. To my knowledge, this 1960 attempt was the only use of seriation on the UGCP.

I'll conclude this section of the paper by briefly discussing one last self-inflicted deficiency of the UGCP: the strict "guys in the field, girls in the lab" approach to staff assignments. To a great extent, this was a product of the attitudes of the time; the 1950s probably represented the nadir of opportunities for women to work as field archaeologists in the

Southwest. In several of the communications about staffing that I unearthed in the University of Utah archives (e.g., Jennings 1957a, 1957b), Jennings consistently refers to recruiting "men." However, some of his earlier projects had included women at least as field school students or field hands. I suspect that it was the remoteness and potential dangers of the Glen Canyon region that promoted a culturally conservative stance on his part. He and/or his dean probably correctly anticipated that the Department of Anthropology and by extension the university would have been mercilessly pilloried in the local media if a young woman was seriously injured while working on the project. Furthermore, it would then have been revealed that tender young ladies were being sent out to live in the wilds, unchaperoned, with groups of undoubtedly randy young men, some of them perhaps even sporting beards.

Don Fowler (2011:275-276) recalls that at the beginning of the 1959 field season, our lab director, Dee Ann Suhm, accompanied Jennings and his son David to our field camp, and was able to participate in the fieldwork for a short time. Our camp location was accessible by jeep and presumably the supervision provided by Jennings made a difference. Later, as Dee Ann Story of the University of Texas, she became famous for training generations of students in how to do good dirt archaeology. Many of her students—male and female—have gone on to successful careers as archaeologists.[4]

WHAT WERE THE LONG-TERM CONTRIBUTIONS OF THE UGCP TO AMERICAN ARCHAEOLOGY?

1) The project field and lab crews and a number of the crew chiefs and other personnel were graduate or advanced undergraduate students from colleges and universities around the U.S. These students gained experience in field techniques, laboratory analysis, report preparation, and project administration and logistics. Because the project occurred just in advance of an employment boom in both the academic and "salvage archaeology" (later "CRM") fields, many of these students found that their UGCP experience helped them become full-time professionals. Consequently, the project resulted in training and professional development for a rather large cohort of archaeologists.

2) In substantive terms, the long shelf of published descriptive reports that the UGCP produced helped put this area of the Southwest "on the record" archaeologically. Although William Adams (1960) has documented a surprisingly large number of earlier archaeological expeditions to the Glen Canyon basin, virtually none of these produced substantive archaeological reports. The UGCP reports remain a significant research resource in the study of Southwestern archaeology, especially for the Puebloan occupations of southeastern Utah.

3) The abundance of small Pueblo period sites documented by the UGCP provided a departure from the then-prevailing Southwestern focus on large pueblos, and contributed to an emerging understanding of variability in settlement, community, and mobility patterns among Southwestern horticulturalists. By the end of the UGCP, Jennings had recognized that a flexible dispersed settlement pattern of small sites was typical of wide areas of the northern Southwest. He became

> inclined…to view the inhabitants of the Mesa Verde cliff dwellings as no more typically Anasazi than the modern cliff dwellers of New York City are typically American…the idea that the typical Anasazi lifeway falls into a rancheria-scrounger pattern, instead of a pattern of high centers radiating stimuli outward to backwoods farmers, will require examination. (Jennings 1963a:13)

4) Pre-Pueblo components were recognized and reported on in some cases, especially those dating to the Basketmaker II period. These studies did provide some evidence of time depth for the region, and have contributed in various ways to development of the productive field studies of the Basketmaker II and Archaic periods that have been undertaken in recent years in the general Glen Canyon area (e.g., Geib 1996; Jennings 1980; Matson et al. 1988).

5) The extremely variable and episodic history of occupation of various portions of the project area, and in fact of the area as a whole, was an important finding that has contributed to a better appreciation of not only forager but agricultural settlement dynamics in the Southwest. Efforts to link changes in rainfall regimes to episodes of population expansion into and withdrawal from the Glen Canyon region were stimulated by this recognition (e.g., Lipe 1970), and have continued since. Unequivocal evidence that Pueblo agriculturalists were periodically moving into and out of the region also served as an antidote to the tendency of Southwestern archaeologists in the 1970s and 1980s to dismiss migration as a source of cultural dynamics.

6) From the beginning of the UGCP, Jennings (1957e:2) recognized that one of the UGCP's goals should be a search for archaeological evidence of Native American occupations that post-dated the Pueblo III period. After several years of fieldwork failed to produce clear evidence of these occupations, he brought Catherine Sweeney (later Fowler) and Robert Euler on board to implement an ethnographic and ethnohistoric approach to the problem. This was quite successful in documenting occupation of the area by Numic speaking groups, and in discovering some of the archaeological sites associated with these occupations (Euler 1966; Sweeney and Euler 1963). The regular UGCP archaeological surveys also recorded a number of sites with Pueblo IV period Hopi pottery (Lipe 1970:137-138), evidently the result either of occasional trips to the area to visit ancestral sites or resource areas or of activities by Numic groups that had acquired Hopi pottery. In addition, at least one historic period probable Ute site was excavated (Sharrock et al. 1961:123-128), and a Navajo habitation site was recorded on Cedar Mesa, with late nineteenth century tree-ring dates obtained from structural timbers (Day 1964:144-146).

7) Under the leadership of Greg Crampton of the University of Utah, numerous historic "Anglo" sites were recorded, in conjunction with documentary research. This work resulted in a string of publications in the University of Utah Anthropological Papers (see Jennings 1966 for a listing) as well as articles in other scholarly outlets, and several successful popular books (e.g., Crampton 1964, 1986). Although excavations at some of the historic period sites would undoubtedly have yielded interesting information, this seems not to have been considered in the design of the UGCP. Historical archaeology was not a well developed specialty in western North America at the time, and even nationally; the Society for Historical Archaeology was not formed until 1968.

8) Ecological reconnaissances were also carried out under the aegis of the UGCP, resulting in a number of publications (see Jennings 1966 for a listing of these contributions). Various ancillary studies also contributed to understandings of the archaeological contexts and the cultural ecology of the early inhabitants of the region.

9) The UGCP made significant contributions to the design and administration of multi-year, multi-team, multi-disciplinary archaeological projects, both through the training that project participants received, and through Jennings' publications on these topics that utilized what he had learned from the UGCP. The UGCP served as a model for future projects through the development of procedures to ensure rigorous project administration; coordination and oversight of multiple field teams; clear division of labor within lab and field teams; routine use of

standard field and laboratory forms; explicit efforts to produce comparable data; prompt reporting of results; and proper curation of all notes, records and specimens for future use.

10) The University of Utah "feature system" for field recording was carried to other institutions and has evolved in various ways. When the Dolores Archaeological Project began, the University of Colorado archaeologists who were in charge were using a version of the feature system that I speculate had been carried from the UGCP to Colorado by Robert Lister, and transmitted to his faculty colleague Dave Breternitz, who further modified it for use on his own field projects. Breternitz subsequently became Principal Investigator on the Dolores Archaeological Project. When I joined the Dolores project as leader of a subcontract group from Washington State University, I used my UGCP experience with the feature system to help adapt it to the demands of creating a computerized database of field records. Subsequently, I and some others who had worked on the Dolores Project became involved with the Crow Canyon Archaeological Center in Colorado, where shadows of the University of Utah feature system can still be detected in the field recording system used by that institution.

11) Underlying the structure and conduct of the UGCP was Jennings' conviction that salvage archaeology involved a public trust. Public funds were being used to build a reservoir project that would destroy archaeological sites. Public funds were also available to recover and study a sample of the archaeological remains that would be affected, leading to new knowledge about the past. To ensure that this public benefit was realized, however, the work would have to be done efficiently, the basic results would have to be reported thoroughly and promptly, and the resulting collections and records would have to continue to be available for further study in a public repository. I believe that the UGCP was successful in meeting these goals, and that it helped influence the further development of public archaeology in the U.S. in the years that followed. ☙

ENDNOTES

1. This paper had its origins in one of the same title read in 1997 at the Annual Meeting of the Society for American Archaeology in a symposium organized by Don Fowler on "The Glen Canyon Project and After: Archaeology in the 'Place No One Knew,' 1957-1997."

2. The Glen Canyon Archeological Project (technically the Upper Colorado River Basin Archeological Salvage Project) was designed to study archaeological, historical, and ecological sites that would be destroyed or affected by the formation of Lake Powell behind the Glen Canyon Dam. The dam was constructed by the Bureau of Reclamation, but the archaeological salvage contract was administered by the National Park Service. Lake Powell began to fill in 1962, and at full pool extends 186 mi up the Glen Canyon of the Colorado River and into the lower portions of its tributaries, including the San Juan River. The southern part of the reservoir is in Arizona, with the remainder in Utah. The Museum of Northern Arizona was responsible for salvage archaeology on the San Juan River portion of the project area, and on the left bank of the Colorado (facing downstream) from the mouth of the San Juan to the dam site. The University of Utah was responsible for the work in the remainder of the project area. This paper covers only the University of Utah portion of the salvage project.

3. Schroedl and Newsome (2000) fault Jennings' claim that "the precise location of over 2000 sites is now known" (Jennings 1966:43). In their review of the site records of the UGCP, they account for only 1635 sites. However, it is clear from the outset of Jennings' 1966 summary report that he intended to cover the work of the Museum of Northern Arizona portion of the Glen Canyon Project, as well as that of the Utah team.

4. Unfortunately, Dee Ann Story passed away in December 2010.

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That Old Music: A Reproduction of a Shell Trumpet from Pueblo Bonito

RICHARD LOOSE

BACKGROUND

My interest in shell trumpets began while I was making some outdoor recordings at Chaco Canyon. This was part of a research project jointly sponsored by the Navajo Tribe and the National Park Service (Loose 2008; Stein et al. 2007). I had been tasked to record some unusual echoes and sound effects along the cliff face between Pueblo Bonito and Chetro Ketl. I had been using a laptop computer with a tone generating program, a battery powered audio amplifier, and a metal outdoor public address speaker to generate tones loud enough to get a good audio recording of the echoes. Around 80 decibels (dB) was the loudest tone that I could generate. During some background research, I learned that a least 12 shell trumpets had been found during excavations at Pueblo Bonito. I decided to make a reproduction trumpet and was pleasantly surprised to measure horn blasts at over 90 dB. The shell trumpet was much easier to use as a portable sound source for creating echoes when compared to the complex electronic setup.

DISTRIBUTION OF SHELL TRUMPETS

In 1916, J. Wilfrid Jackson published a short article describing the wide spread use of marine shells as horns or trumpets. Dr. Jackson was the Honorable Librarian of the Conchological Society of Great Britain and Ireland. The term "conchological" refers to the study of mollusks. The Conchological Society was founded in 1876 and is still active today.

The paper was published in the Memoirs and Proceedings of the Manchester Literary and Philosophical Society covering the years of 1915 and 1916 (Jackson 1916). The content of his publication was comprehensive and is still quite relevant to current research. He included a world map showing the distribution of shell trumpets in North and South America, across the Pacific in Oceania, New Zealand, New Guinea, Borneo, Indo-China, Japan, India, Mesopotamia (Persia), the Mediterranean, Europe, and Great Britain. The time span of conch shell trumpet use is equally impressive, going back to at least the second millennium B.C. in Minoan Crete (Braun 2002).

ETHNOGRAPHIC ACCOUNTS

Jackson combined descriptions of archeological and modern ethnographic examples. He cited uses to include herding cattle, signaling, sounding alarms, and scaring thieves and birds away from vineyards and gardens. He noted that Triton shells (Triton nodiferus) in particular are still used to announce church services in some communities in Italy. Also in a religious context, the Chank shell (Turbinella pyrum) is used in India for summoning the god Krishna's attention, announcing the commencement of principal rites, and calling the devout to worship. The Chank shell

trumpet can also be used to scare away hostile and evil spirits. In Bengal, Chank shell trumpets are blown during eclipses and earthquakes until the eclipse or earthquake is over. The Chank is also used in South India and Ceylon to summon plantation workers to their duties. In many of the South Pacific islands, the shell trumpet was used as a call to war.

In the American Southwest, the conch shell trumpet has been associated with warfare. E. Brown (2005) quotes Tyler (1975) regarding the use of a "Big Shell" at Zuni Pueblo during their initial contact with Europeans. The Zuni War Chief was the keeper of the Big Shell.

> The Big Shell seems to have been an actual conch shell which was blown as a trumpet in times of dire threat, with the expectation of magically dispelling the enemy. It was probably this or a similar Big Shell that appeared, according to accounts, in the resistance to Coronado's attack of 1540…. On the previous night one group of Spaniards had heard the "trumpet" sounded by a scouting party of the Zuni and had interpreted it as a call for retreat by the Indians. Doubtless it was not this, but the first attempt to drive off the invaders by means of Big Shell's magic (Tyler 1975).

In terms of historic ritual contexts, it has been reported that the Zunis and Hopis associate the sound of shell trumpets with the roar of the Great Plumed Serpent (D. Brown 1971; E. Brown 2005; Mills and Ferguson 2008). The Hopi refer to the Great Plumed Serpent as *Paalölöqangw*, and his Zuni name is *Kolowisi*. E. Brown (2005) provides an informative discussion of the significance of this association including possible links to the Mesoamerican gods Tlaloc and the Quetzalcoatl Plumed Serpent. Mills and Ferguson (2008) mention that the Plumed Serpent controls rain and brings good things. They also report that shell trumpets are thought to have magical powers and can actually take the life of witches and enemies if properly used. Blackhorse et al. (2002) have documented the use of shell trumpets by Navajo *Hataalii* (Chanters) during a ceremonial called *Chiihwojoolye of Tsebiinaholts'a Yalti*. This *Taal* (Chant) is still performed by modern practitioners at a natural amphitheater in Chaco Canyon. The acoustical properties of this amphitheater were recorded using modern electronic equipment as part of the research project mentioned above.

ARCHAEOLOGICAL ACCOUNTS

Although shell trumpets have a world-wide distribution, I will only discuss pre-Columbian shell trumpets from New Mexico, Arizona, and Casas Grandes (Paquimé) in northern Mexico. I will make one exception for a recent and spectacular find at Chavin de Huantar in Peru.

The American Southwest

I was fortunate to find two excellent investigations into shell trumpets from the American Southwest. One is from Barbara Mills and T. J. Ferguson (Mills and Ferguson 2008) and the other is Emily Brown's doctoral dissertation (E. Brown 2005). Mills and Ferguson considered shell trumpets only, while Brown examined all types of pre-Columbian musical instruments from the Southwest. A short summary of their work follows.

Brown's research considered at total of 43 shell trumpets. Mills and Ferguson catalogued 317 examples. The site of Paquimé accounted for 181 specimens, while the Grewe Site in Arizona produced a total of 42. The most common conch trumpets were made from the shell of Strombus galeatus. Other species included Strombus gracilor, Murex nigritus, Melongena patula, Muricanthus negritus, and Phyllonotus nitidus. Shell length ranged from 5.25 cm (2.0 in) to 21.0 cm (8.26 in) with a mean value of 12.0 cm (4.72 in). Diameters ranged from 3.5 cm (1.37 in) to 15 .0 cm (5.90 in) (Brown 2005).

With regard to the term "conch shell trumpet", it

should be noted that true conchs are marine gastropods (univalve mollusks) in the family Strombidae. Many other large univalve shells are sometimes called conchs and include Melongenidae, and Fasiolariidae (horse conch) and the sacred Chank (Shanka) of India from the family Turbinellidae. In fact, any large spiral shell will produce a loud tone if the end of the spire or a side whorl near the end of the spire is modified with a mouthpiece hole.

Mills and Ferguson (2008) report that the majority of shell trumpets have been found in the Hohokam area of southern Arizona. They are also found in the Western Pueblo sites along the Mogollon Rim, at Chaco Canyon in New Mexico, and at Casas Grandes in northern Mexico. Use of the trumpets in the Southwest spans from A.D. 900 to European contact, and they are still in use today.

Seventeen conch trumpets including Strombus and Murex were found in Bonito Phase ruins in Chaco Canyon (Mills and Ferguson 2008). Probably the most famous shell trumpet from Chaco is the one associated with Burial 14 in Room 33 of Pueblo Bonito (Pepper 1909). Room 33 contained thousands of pieces of shell, shell beads, turquoise beads and pendants, whole pottery vessels, and six nearly intact wooden flageolets (flutes). Other objects included a jet finger ring, Haliotis shell disks, turquoise frogs and tadpoles, and turquoise inlay mosaic work. The shell trumpet was found 4 in. from the right knee of Burial 14. The trumpet had holes drilled in it that appeared to be from an attempt to repair a crack in the shell body. Mouthpieces designed for shell trumpets were also found in Room 33. The mouthpieces were made of pitch, gum, or clay. One contained an inlay of turquoise fragments.

I was fortunate enough to receive a digital photograph of the Burial 14 trumpet. It was taken by Dr. Ted Frisbie during a recent visit to the American Museum of Natural History in New York. This photograph was the initial inspiration for me to attempt making a copy of this particular shell trumpet. It is interesting to compare Ted's photo to the plate from Pepper's report on his excavations at Pueblo Bonito (Pepper 1920). Some of the shell in the repair area is now missing.

Chavin de Huantar

Chavin de Huantar is an archeological site in the Peruvian Andes and is located about 160 mi north of Lima at an altitude of 10,480 ft. It is considered to be a Formative Period ceremonial center (Cook et al. 2010) and was occupied from 1500 B.C. to 400 B.C. The site has been designated a UNESCO World Heritage Site.

Figure 1. Recent photograph of the conch shell trumpet associated with Burial 14 from Room 33 in Pueblo Bonito (AMNH H/3653). Photo courtesy of Ted Frisbie.

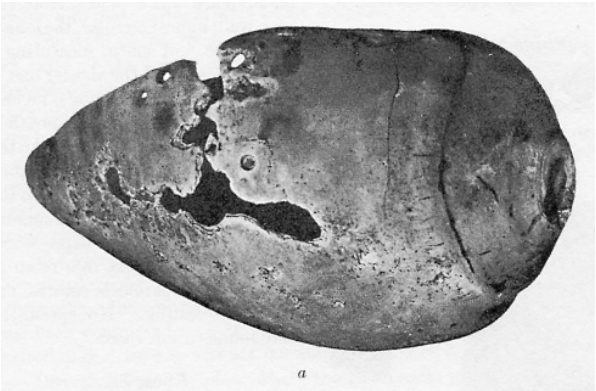

Figure 2. Plate of the Pueblo Bonito Room 33 trumpet (AMNH H/3653) as shown in Pepper's (1920) report on Pueblo Bonito.

In 2001, 20 decorated playable trumpets made from Strombus galeatus shells were excavated at Chavin de Huantar. These shells were polished, painted, and etched with symbols. Iconography at the site included representations of the trumpets and portrayed humans partially transmorphed into animal forms. The investigators suspect that psychoactive plants were processed and ingested by ritual participants to enhance a religious experience (Cook et al. 2010).

As part of their research program, they subjected the trumpets and local architecture to a rigorous and highly technical acoustical analysis (Cook et al. 2010). It was found that the fundamental tones of the trumpets ranged from 272.11 cycles per second (C#4) to 416.87 cycles per second (G#4). The "playability" of each shell was rated from unplayable, unresonant, to highly playable, highly resonant. I found this personally interesting since I have made some shell trumpets that were very easy to sound. Others made in exactly the same way were nearly impossible to play. I suspect that a subtle difference in the internal geometry of the particular shell is to blame.

The research at Chavin de Huantar also included audio recordings of the echoes from computer generated sound impulses in the complex of stone labyrinths at the site. In addition, the trumpets were actually played inside the stone chambers where they were found. Resonances of up to 1 second were observed. The returned drone tone sounded as if it was coming from several directions at the same time. This is an example of an auditory illusion referred to as an acousma. Again, unusual sound effects were possibly used to enhance a cult religious experience.

THE REPRODUCTION

The hardest part of recreating Pepper's famous Burial 14 shell trumpet from Pueblo Bonito (Pepper 1909) was finding a large Strombus galeatus shell. They are not popular with collectors and high end specimens are quite expensive. I found one shell on eBay that was advertised for $800.00. I started thinking about a trip to the Sea of Cortez. Luckily, I found a shell dealer in Mesa, Arizona who specialized in items from the Sea of Cortez and I soon had my shell at the local post office in Organ. Upon opening the package, I was pleasantly surprised to find a nicely preserved shell that was 7 7/8 in (20 cm) long and 4 1/2 in (11.43 cm) in diameter. It weighed in at 2 lbs and 5 oz (1.05 kg). This shell is right at the upper size range of 21 cm reported by E. Brown (2005).

After cleaning the shell, the next task was to remove the end of the spire and create an aperture for blowing the trumpet. A cut was made using a high-

Figure 3. Unmodified *Strombus galeatus* shell, side view. Photo by the author.

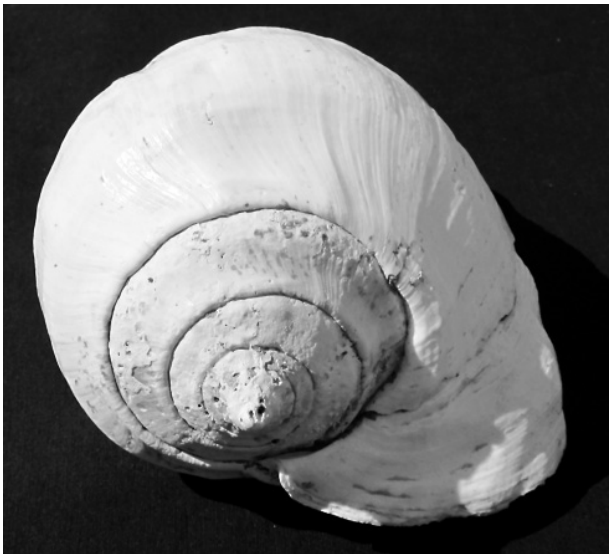

Figure 4. Unmodified *Strombus galeatus* shell, end view. Photo by the author.

speed abrasive disk. The opening was then enlarged using a tapered grinding stone mounted in a small electric drill. I slowly ground out the hole and started doing sound tests when the hole got close to a 1/2 in wide. I have found that between 1/2 in and 5/8 in is the best diameter for me personally. I decided to leave the apical lip (wing) in the natural configuration. This lip had been cut back on Pepper's trumpet from Room 33. I'm not sure why this was done because given its position at the end of the whorls, it should not have significantly affected the tone or "playability." Perhaps this valuable piece of shell was removed and processed into other items. The entire modification process took about 30 minutes, but I imagine it took hours or days to do this by hand with nothing but stone tools. The newly created trumpet had a nice clear steady tone that echoed off the local buildings.

At this point, I was forced to abruptly stop further work because a powerful thunderstorm had come up with wind, heavy rain, and lightning. Local rain gauges recorded that 2 to 3 in. fell in about 2 hours that afternoon. It was the most rain we had received in the local area in over a year. I had to smile and wonder about the association of these instruments with the Water Serpent and bringing rainstorms.

The next day I finished grinding the aperture and then dressed it with some small steel jeweler's files to remove any burrs and get a flat smooth surface for the best lip seal. After the trumpet was finished, I took some photographs and then measured the pitch and loudness of the shell using a digital recording and analysis program called CoolEdit Pro by Scintrillium Software. Although it is over ten years old and has a somewhat flippant title, it is a highly sophisticated sound editing program and provides several different algorithms for spectral analysis of a recorded sound track.

To make the recordings, I first used a QuickStart desktop microphone with my computer's audio card and drivers (Audigy 2 ZS Audio), then made a second recording using a Roland UA-30 digital USB audio interface and a stage quality microphone. Results of the two tests were nearly identical with the fundamental tone at 329.84 cycles per second (E4) and a rich set of overtones with the first harmonic at 650.0 cycles per second and the next one at 974.4 cycles per second. The sound level was measured at 96 dB above the noise floor of the recording system.

Figure 5. New *Strombus galeatus* trumpet with blowing aperture completed. Photo by the author.

The research team that examined the shell trumpets from Chavin de Huantar used a second technique for measuring the natural resonance of the individual shells. They called it an impulse test. It was performed by smacking the mouthpiece end of the trumpet with an open palm to force an impulse puff of air through the trumpet's spiral bore. This produced a "popping tone" that would very accurately portray the natural resonance of the shell and remove effects caused by the particular player. (The museum curator must not have been in the room during these tests.) I gave it a try and got 330.0 cycles per second for my trumpet.

At this point I should add a short explanation of how a shell trumpet actually works. Shell trumpets are often referred to as "lip reed" instruments because the player's lips act like the reed in woodwind instruments such as clarinets or bassoons. The lips are tightened over the trumpet mouthpiece opening and air is blown, causing the lips to vibrate. This is done until the vibration frequency matches the natural resonant frequency of the shell. If the shell is "overblown" hard enough, sometimes the next harmonic tone (higher in

pitch) can be produced. The tone depends mainly on the size of the shell and partially on how it is blown, with larger shells producing lower tones, and smaller shells producing higher tones.

The mouthpieces mentioned earlier that were found associated with trumpets may have had more than just a decorative function. They may have been used in part to adjust the size of the blowing aperture to improve the "playability." Once an aperture has been opened, it can always be made larger, but there is no way to make it smaller other than to add a mouthpiece. I have found that the small shells less than 5 in long are very difficult to play. This seems especially true for Murex shells because their bores expand quickly and are therefore short when compared to Strombus shells. If the mouthpiece restricts the aperture and then bells out on the upstream side, the venturi formed can also assist the player in maintaining a consistent air flow, pitch, and volume. Thus, the applied mouthpiece can help enhance the player's embouchure on the smaller shells and make them easier to play. I have experimented with beeswax and pine pitch to form applied mouthpieces. Both materials work fairly well and improved the trumpet's performance.

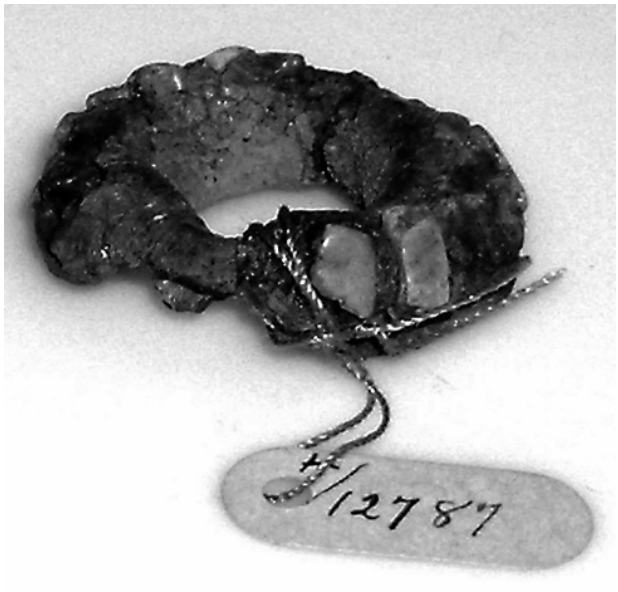

Figure 6. A turquoise encrusted trumpet mouthpiece from Room 33 in Pueblo Bonito (AMNH H/12787). Photo courtesy of Ted Frisbie.

CONCLUSIONS

Both (2006:267) summed up his paper on shell trumpets in Mesoamerica as follows. I think it applies equally well to the American Southwest.

> A number of basic ritual functions remained apparently untouched by cultural changes and could have survived from prehispanic times to the present. One of the most important concepts shared in each of the cultures is the shell trumpet's function as a sacred instrument associated with water, fertility and creation. It is always guarded in sanctuaries and played by expert religious leaders in important ceremonies, in which the shell trumpet's powerful sound is capable of reaching the spiritual realm.

Clearly, the shell trumpets in the American Southwest and northern Mexico were transported to where they were found in archaeological contexts. The nearest possible source for the species that have been found is the Sea of Cortez (Cipriani et al. 2008). These shells were obtained either by sending an expedition out for particular items or by participating in a trade network. Considering the suite of exotic items found at remote locations like Chaco Canyon, being part of a regional trade network would seem to have been the most efficient approach. Ronna Bradley (2000) has produced a detailed study of shell trade in the American Southwest. She proposes that Chaco was in the Hohokam sphere of trade partners.

Shell trumpets are usually found in caches, ritual contexts such as kivas, or in burials. This is true for the greater Southwest as well as Mesoamerica and Peru. It is known that in historic times shell trumpets were associated with the Plumed Serpent at Hopi and Zuni. These instruments were likely considered as powerful objects in pre-Columbian times and considered capable of bringing rain or taking a life. They were probably thought to be dangerous and only certain

qualified practitioners were allowed to use them. These individuals, in turn, obtained high status and power by knowing how to use these devices. They were even capable of effecting actions at a distance because the sound of a shell trumpet can be heard two or three miles away. So, in addition to bringing rain, the trumpets could have been used in ritual practices associated with curing, warfare, and sorcery. ☙

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A Prehistoric Pueblo Trail in the Ojo Caliente Valley

JAMES L. MOORE

INTRODUCTION

In preparation for the widening of U.S. 285 by the New Mexico Department of Transportation, the Office of Archaeological Studies examined a series of Classic period terrace top gravel-mulched fields along the east side of the Ojo Caliente Valley near the small community of Gavilan. As we examined these sites on the terrace top, we noted relatively narrow linear shelves on the slopes below most of the sites being investigated. Closer inspection of the shelves showed that they represented a trail that was broken into numerous segments by erosion and historic construction activities. Designated as LA 118549, the trail was visible from the modern road at the time of discovery, allowing it to be traced for at least 9.2 km, beginning just south of the modern village of Ojo Caliente and running at least as far south as the Classic period village of Ponsipa'akeri. There it was met by two short trail segments that originate at Ponsipa'akeri and end at LA 118549, probably representing access corridors from the village to this thoroughfare.

Our assumption was that construction and use of the trail was related to the Classic period occupation of the Ojo Caliente Valley, which included the large Pueblo villages of Howiri, Hupobi, Nuté, Ponsipa'akeri, and Posi'ouinge, and the smaller village of Hilltop Pueblo adjacent to, and possibly related to, Nuté. However, it was possible that the trail could have been a historic travel route or was related in some way to highway construction along U.S. 285. Before a prehistoric Pueblo affiliation could be assumed, these possibilities had to be ruled out. That process is discussed in more detail later, but first a description of the trail and a discussion of some of our investigations along it are presented.

DESCRIPTION OF THE TRAIL

Most segmentation of the trail was created by the cutting of erosional channels that drain the terrace and form narrow valleys between fingers along its west edge. Smaller channels cut through trail segments in many places, but enough of this linear feature remained intact that it could be easily traced. The trail tends to meander across the west ends of terrace fingers, occasionally ascending to the terrace top and seemingly always curving down to descend to the floors of the small intervening valleys. There it disappears, only to reappear on the other side of the valley where it ascends back up the terrace slope. Though no evidence of the trail was visible on the intervening valley floors, this lack can be attributed to the action of erosion occurring in the centuries since the trail was regularly used for travel. When the trail was in use the various segments that are now visible were parts of an uninterrupted travel corridor.

Segments that were both adjacent to the farming sites and that extended into project limits were examined and described. Each of these segments was mapped along with features on the adjacent farming

sites. The remaining sections of segments within project limits were then examined by pedestrian survey, their physical characteristics were recorded and described, and associated artifacts were collected. Subsurface investigations were limited to two mechanically excavated trenches that examined the trail in cross-section. In one trench, the trail appeared as a shallow swale with a maximum depth of 7 cm and a width of 1.2 m (Figure 1). In the second trench the trail had a maximum depth of 15 cm and a width of 1.65 m (Figure 2). The shelf-like appearance of the trail suggests the investment of considerable amounts of labor to clear its route of cobbles, and there may have been an additional investment of labor by cutting the shelf into the terrace slope. It is also possible that the trail took this form from foot traffic along the corridor after the route was simply cleared of cobbles. A combination of both processes is likely, with the removal of materials from the trail to create a more level pathway, and traffic along the trail incising it further into the terrace slope and creating the shallow swales seen in the two cross-sections. Thus, some level

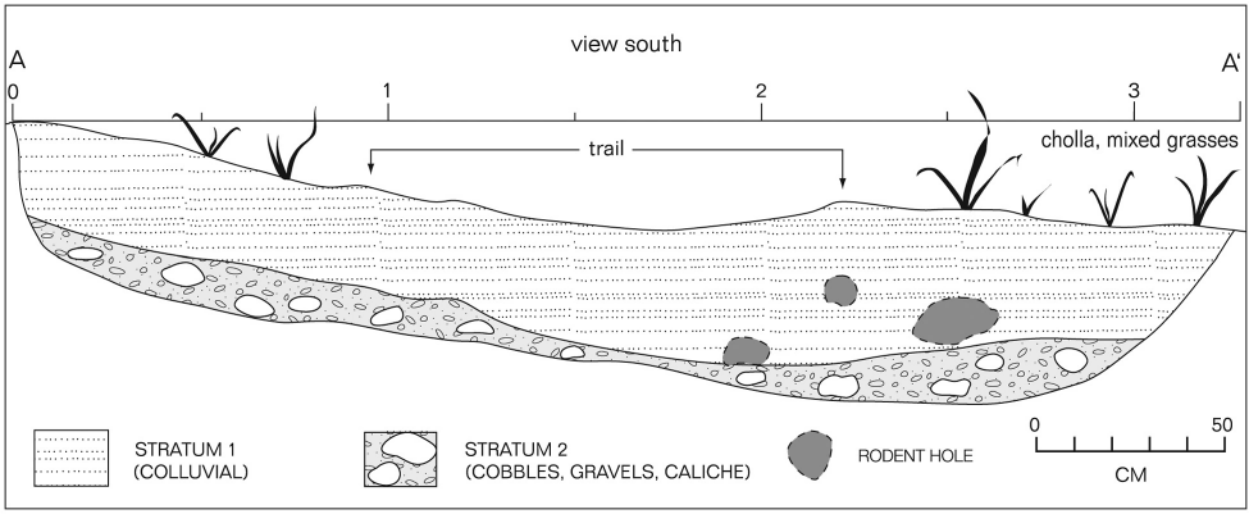

Figure 1. Profile of Backhoe Trench 1 across a section of the trail.

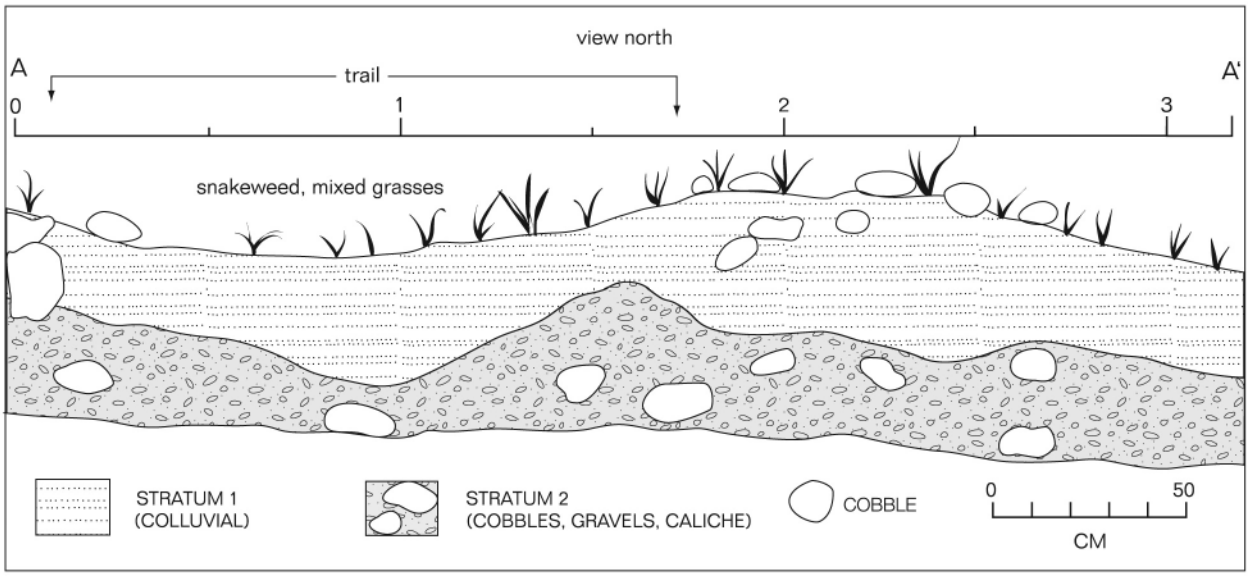

Figure 2. Profile of Backhoe Trench 2 across a section of the trail.

Figure 3. Photo of a segment of LA 105713 showing its shelf-like appearance, cleared of cobbles.

of purposeful clearing and construction of the route is certain, especially considering the elaborations noted wherever the trail ascends to the terrace top, as discussed in the next section. As cobbles and boulders were very obviously cleared from the trail (Figure 3), both processes were probably factors in creating its current form.

In most places the trail occurred as a shelf positioned about halfway between the terrace top and valley bottom. It may be important that the trail was routed along the terrace slope rather than in the valley bottom or on top of the terrace. Farming features occupied the west edge of the terrace top along much or most of the section of trail that could still be traced, and the valley bottom probably contained numerous irrigated fields during the Classic period occupation. By routing the trail along the terrace slope, it avoided potential and active farm lands that would have been controlled by various corporate groups living in the nearby Pueblo villages.

CONSTRUCTION ELABORATIONS

The trail ascended to the terrace top in several places and, as noted earlier, showed evidence of elaborations in each case. In one location, the trail ascended to about four-fifths of the way up the terrace slope and leveled off about 2.5 m below the terrace top, running north-northwest along the west terrace slope. Through this area the trail formed a shallow swale that was about 1.75 m wide, including a distinct berm on the downslope side. The berm was 25-30 cm high, and seemed to be mostly composed of cobbles and gravels removed from the swale and piled along the outer edge of the trail. The bottom of the swale was mostly devoid of cobbles except for those that had washed in from above. In some places the trail widened to about 2 m, and the berm was similarly higher at 30-40 cm. The trail soon began to wind upward, paralleling the terrace edge and ascending to the terrace top. Through

this area the trail continued to be about 2 m wide and the berm was 30-40 cm high (Figure 4). Remaining on top of the terrace, the trail closely paralleled its west edge until it reached a point about 30 m south of an earth navel shrine. At this point the trail began descending from the terrace top, and continued to be paralleled on its downslope side by a berm (Figure 5). Erosion had deepened the trail by 15-20 cm through this area. By the time the trail was below the shrine it had descended about a third of the way downslope. The berm disappeared at about that point and the cross-section of the trail again became shelf-like rather than a shallow swale. The trail continued to descend until it was two-thirds of the way down the terrace slope, where it leveled off.

In another location, a section of trail ran about 75 m north across a shelf that was 5 m below the top of the terrace. The trail was about 2 m wide through this area, and a berm 20 cm high and 1 m wide ran along its west edge. This segment was a shallow swale in cross-section, and was mostly devoid of rocks but was heavily disturbed by rodent activity. The trail began sloping upward at the north end of this stretch; the berm ended at that point, and the trail cross-section became shelf-like. After about 15 m the trail began to more steeply ascend the slope toward the terrace top. It was only 1.2-1.3 m wide through this area, and as the steeper ascent began the berm again appeared and was quite distinct by the time the trail reached the top of the terrace next to an elaborate double terrace-edge borrow pit (Figures 6 and 7). The berm was 25-30 cm high and a meter wide in this section. The trail crossed the terrace top west of the double terrace-top borrow pit and almost immediately began to descend again. At that point the berm disappeared and the trail resumed a shelf-like appearance.

The relationship between the trail and the double terrace-edge borrow pit is interesting to speculate upon. The large outer pit almost certainly represents the original borrow area, which was probably used to build a nearby gravel-mulched field. The purpose of the smaller interior pit is more difficult to explain. While it may simply represent reuse of the borrow pit as a materials source, it may have also had a less practical

Figure 4. Berm along a section of trail as it nears the earth navel shrine at LA 105709.

Figure 5. Descending segment of trail after passing the earth navel shrine at LA 105709.

function associated with the trail. Other borrow pits in the area that displayed evidence of multiple episodes of use were generally simply enlarged by the excavation of a new area adjacent to the original borrow pit, giving it a lobed appearance. Since the double terrace-edge borrow pit has a completely different appearance, it may have functioned differently and a ritual use is possible, though speculative.

A third segment of trail crossed the terrace top adjacent to a gravel-mulched field, but the section that ascended the slope was eradicated during an earlier construction episode along the highway. This section of trail formed a shallow swale about 10-12 cm deep and 2 m wide at the edge of the road cut, and paralleled the right-of-way fence. The downslope edge was bermed, but most of the berm had been removed by the road cut. Where most intact, the berm was about 20 cm high and 1.5+ m wide. After passing the gravel-mulched field, the trail began to descend the terrace slope at a moderately steep angle. This section was only about 1 m wide, and was incised 10-15 cm deep by erosion. There was no evidence of a berm through this area.

The trail reached the terrace top in one other location within project limits. As it approached the terrace top, this section of trail had a shelf-like cross-section, was fairly indistinct, and was only 1.0-1.1 m wide. As the trail reached a point about 2.5 m below the terrace top a berm appeared along its downslope side that was 20-30 cm high and 1.0-1.2 m wide, with the trail forming a swale that was 1.3-1.5 m wide. The berm achieved its maximum height as the trail reached the top of the terrace slope, where it was 40-50 cm high and the trail swale was about the same depth. The trail soon widened to 2 m and became shallower, decreasing to a depth of 10-15 cm. Similarly, the berm was lower through that area, with a height of only 15-20 cm. Near this segment of trail were two small rock piles that may represent shrines, though their association with the trail is uncertain. Soon after this point the trail began to descend the slope. The berm continued for about the first 20 m downslope, then

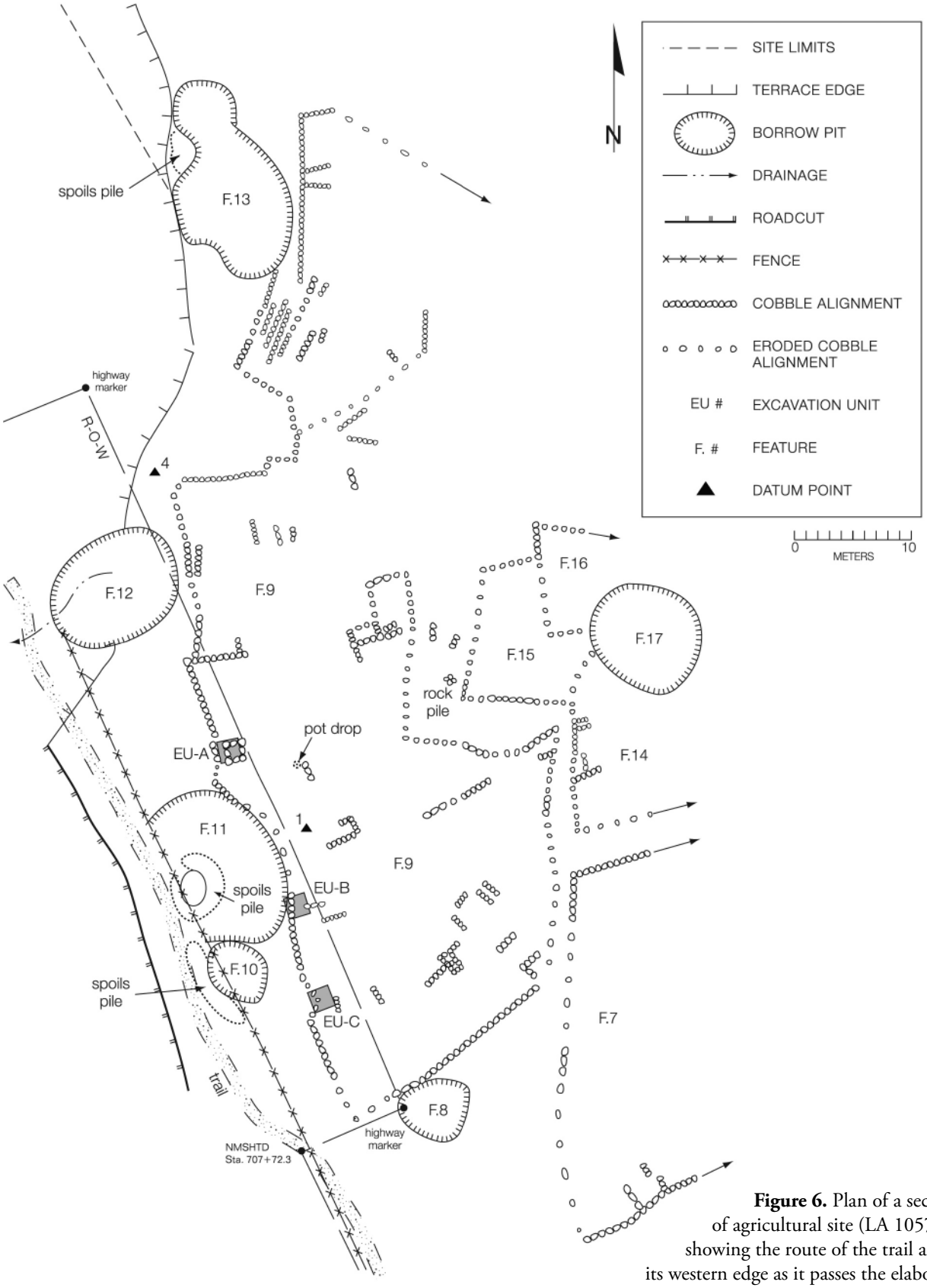

Figure 6. Plan of a section of agricultural site (LA 105708) showing the route of the trail along its western edge as it passes the elaborate double borrow pit (Feature 11 on plan).

Figure 7. Photo of the elaborate double borrow pit at LA 105708 and adjacent section of trail, bermed along its downslope side.

disappeared, and the trail again assumed a shelf-like cross-section except for short areas that were eroded away. Through this area the trail was 1.3-1.5 m wide.

Four characteristics usually occurred when the trail ascended to the terrace top. In all cases a berm appeared along the downslope side, generally disappearing after the trail crossed the terrace top and began to descend the slope. In the bermed areas the trail generally formed a swale rather than a shelf, though this may have resulted from use rather than construction. In all cases the trail remained on the terrace top for only a short distance, and soon began descending the slope again. Finally, in at least two and possibly three cases, definite or likely shrines occurred in the areas where the trail crossed the terrace top, and in the fourth case any potential shrine may have been removed by earlier episodes of construction.

DATING THE TRAIL

We assume a prehistoric, probably Classic period, affiliation for the trail based on several lines of evidence that suggest it represents a culturally-built feature of the prehistoric landscape rather than a historic trail or road-related feature. Interviews with long-term residents of the Gavilan area failed to elicit any information concerning historic use of the trail. Similarly, there is no evidence that a long linear landscape feature of this type was related in any way to earlier episodes of construction along U.S. 285. During a field inspection in which several members of the team that was supervising construction of this highway segment were shown the sites to aid in protecting them, a section of the trail was pointed out. All agreed that it was not related to road construction.

In addition, plans from the initial construction and paving of U.S. 285 in 1939 were obtained and examined (NMSHTD 1939). Nothing remotely resembling the location or configuration of LA 118549 was scheduled for building in those plans.

In a further attempt to provide a minimum date for the trail, two junipers that were growing out of the trail or the associated berm within the existing right-of-way in areas scheduled for removal by slope cuts were sectioned. Two counts at different locations were made for the first sample, yielding totals of 80 and 82 rings, the latter being considered the more accurate. Five counts at different locations were made for the second sample, yielding totals of 70, 65, 64, 57, and 56 rings, with the first three being considered the most accurate. The second juniper sample was very convoluted, and different axes produced different counts because of the way in which the tree had grown. If the trail had been built in conjunction with highway construction, neither tree should predate those building episodes. The samples were obtained in 1998; Sample 1 would therefore have begun growing sometime around 1916 to 1918, and Sample 2 around 1928 to 1934. As both trees began growing before the initial construction and paving of U.S. 285 in 1939, as well as all subsequent road building episodes, construction and use of the trail definitely predates the early years of the twentieth century.

The very structure and routing of the trail argue for a prehistoric affinity. No modern highway construction-related feature would become more elaborate as it approached and crossed the terrace top, nor would it be expected to leave the terrace slope. A historic trail built and used by the Spanish also would not demonstrate these tendencies. Spanish transportation corridors would be expected to occur in valley bottoms because they would have needed to accommodate livestock and wagons as well as foot traffic. A prehistoric affinity is much more likely because the elaborate sections of trail tend to occur near features of probable ritual importance to the prehistoric Pueblo occupants of the region. Thus, we conclude that LA 118549 seems to represent a prehistoric trail linking several large villages and associated fields on the west side of the Rio Ojo Caliente.

Harrington (1916) presents a detailed discussion of Tewa ethnogeography, but while he describes numerous trails in the region, there is no mention of any in the Ojo Caliente Valley. This is an interesting omission, but Harrington does note that it was difficult to obtain adequate information on old trails from the Tewa (Harrington 1916:107). One possible reason for this is that certain ancient trails may have become classified as sacred, no matter how mundane their original nature might have been. This may have been especially true of trails leading to important shrines or that were used for ceremonial purposes. One such feature was recorded near San Ildefonso (Moore and Levine 1987), and was said to be a sacred hunting trail. Interestingly, that trail was similar in cross-section and traversed landforms like those crossed by the trail in the Ojo Caliente Valley.

INTERPRETING THE TRAIL

Trails in the Pueblo region were mainly used as pedestrian corridors to travel from one location to another, but they also at times served as ritual pathways. For example, trail segments have been extensively documented in the Chaco region in northwest New Mexico and, rather than pedestrian corridors, they seem to have been "...cosmological corridors that link ceremonial architecture to various topographic features, horizon markers, and directional-astronomical orientations" (Marshall 1997:71). Most of the known trails in the Northern Rio Grande seem to have functioned mainly as pedestrian corridors, and thus filled a different niche than the Chacoan roads did, though some ritual pathways have also been identified in the Northern Rio Grande.

An example of a ritual pathway in the Northern Rio Grande is at the well-documented directional earth navel shrine on top of Tsikomo Peak, which is sacred to many Native American groups, especially the Pueblos. Douglas (1912, 1917) indicates that

each Pueblo that uses this shrine has its own trail, all radiating out from the eastern shrine entrance. Parsons' (1929:241) drawing of the entrance to this shrine disagrees with Douglas' description, instead showing a single trail leading into the enclosure, with several secondary trails joining it just before the main trail enters the shrine.

Jeançon (1923:70-71) described the landscape around the Classic period village of Poshu'ouinge in the Chama Valley, noting that paths lead out from the village in many directions, all apparently ending at shrines. While the features Jeançon saw at Poshu'ouinge may indeed have been formal trails leading to shrines, it is also possible that he was describing some of the cobble-bordered gravel-mulched fields occurring around that village. However, a similar trail seems to approach the large earth navel shrine south of Posi'ouinge in the Ojo Caliente Valley. As we suggested for Jeançon's formal pathways at Poshu'ouinge, this trail is lined by cobble alignments that appear to border gravel-mulched fields. Thus, the formal-looking trail that approaches the shrine may represent an area that was intentionally left clear during field-building to allow unrestricted access to the shrine.

Harrington (1916) documents several trails in the Tewa Basin. He notes the existence of trails passing through Santa Clara and Guaje Canyons to the Jemez area, one in the El Rito region leading to the Tierra Amarillo area, and several others. The entire Cochiti region (including Frijoles Canyon) is said to be covered by a network of trails (Harrington 1916:421). Two trails were noted for the general Ojo Caliente area, but both cross Canoe Mesa and do not extend into our project area, one continuing into Comanche Canyon and leading into Ute territory (Harrington 1916:199). Bandelier (Lange et al. 1975:86) found a well-defined trail leading to Posi'ouinge, and barely perceptible trails heading north along the mesa that forms the west rim of the Ojo Caliente Valley.

Steen (1982:7) reports that trails are common on the Pajarito Plateau, and are usually associated with habitations. However, near the upper end of Mortendad Canyon he found a trail that is deeply cut into tuff and lacks associated habitation sites (Steen 1982:7). At Bandelier National Monument, Powers and Van Zandt (1999:142) found seven habitation structures associated with well-defined trail segments. Hewett (1906:16) describes a trail at Navajú, a prehistoric Pueblo village on the Pajarito Plateau, as worn hip-deep by foot traffic.

Two basic types of corridors seem to be represented by trails. The most common type is a pedestrian corridor, usually leading from one residential site to another or to an area that is used for farming or contains resources used on a regular basis. The second type is a ritual corridor leading to a location of esoteric importance, usually a shrine or a source for important ritual materials. Some trails were used for both purposes. The Zuni had three trails that led to Acoma, which were used both as trade routes and to access sacred areas (Holmes 1989:18). The Zunis also used at least two trails for their pilgrimages to Zuni Salt Lake, one for foot traffic and a second suitable for burros (Kelley 1988:2-7). Acoma and Laguna had their own separate trail to Zuni Salt Lake (Kelley 1988:2-8). These trails had shrines along them, at which offerings were left by pilgrims on their way to gather salt (Kelley 1988). The Acoma-Laguna trail was marked in places by cairns, and the shrines along it were small earth navels that opened to the east (Kelley 1988:2-8). Another trail led from Zuni Salt Lake to the confluence of the Zuni and Little Colorado rivers, which is where the Zuni say they originated (Kelley 1988:2-8). This trail is sacred to both Zuni and Acoma, and has shrines along it (Kelley 1988:2-8).

If the trail recorded as LA 118549 was used for ritual as well as mundane purposes, it should lead to an important ritual location. The most important ritual location in the Ojo Caliente Valley, so far as we know, is the hot spring located just below the ancestral village of Posi'ouinge that is now part of a resort in the village of Ojo Caliente. All springs are considered sacred by the Tewa, but some springs may have been more important than others. For example, the Tewa place of emergence is a small brackish lake located in the

sand dunes north of Alamosa, Colorado (Harrington 1916:564-565; Hewett and Dutton 1945:23). Sacred waters are associated with the four cardinal directions; for example, the sacred spring of the west is located less than a mile southwest of Perage (Hewett 1938), a Classic period village situated across the Rio Grande from San Ildefonso Pueblo.

Harrington (1916:164) indicates that the hot spring at Ojo Caliente is considered by the Tewa to be one of their most sacred places. Besides the usual sacred nature of springs and other bodies of water, this hot spring has important associations with Poseyemu, the Tewa culture hero. Poseyemu is said to have occasionally entered the hot spring when he still lived among the Tewa. The hot spring is also considered the home of Poseyemu's grandmother, and he is said to visit her there once a year (Harrington 1916:164). People at San Juan Pueblo (now known as Okay Owingeh) told Harrington (1916:164) that the Tewas still drank water from the hot spring and, presumably, had done so in the past. The importance extended to the hot spring at Ojo Caliente suggests that visits to that area were probably common and associated with ritual in prehistoric as well as historic times.

The nature of the trail also suggests that it may not have been simply a pedestrian corridor. The route that it follows is certainly not the easiest way to traverse the Ojo Caliente Valley, since it mostly travels along a terrace slope, about halfway up. Normal pedestrian traffic would be expected to take an easier route, though the land tenure system may have helped determine where such corridors could be placed. If most of the valley floor and the rim of the terrace that forms the east edge of the Ojo Caliente Valley were covered with fields, the terrace slope may have been one of the few corridors open to pedestrian traffic that would not cause friction with local farmers. However, the fields that rimmed the terrace edge formed a comparatively narrow band, so a simple ascension of the terrace and a short walk to the other side of the band of gravel-mulched fields would have carried travelers to a fairly flat area where foot-travel was easier and no modifications to the corridor were needed.

If the trail was the main route to the gravel-mulched fields, one would expect it to ascend to the terrace top at fairly regular intervals. It does not do this. Indeed, the trail topped out in very few places within our study area, and when it did there was often a shrine or shrine-like feature at that point. The trail does not even ascend to the terrace top at the village of Ponsipa'akeri. Instead, two trails documented by Bugé (1978) lead down to it. A few faint traces of trails leading up to fields were noted during our study, but none of these was improved or as well-defined as was LA 118549 itself.

When all of this information is combined, our suspicion is that LA 118549 does not represent a simple pedestrian corridor. Rather, it seems more likely that this trail was a ritual pilgrimage route from the south to the hot spring at Ojo Caliente. The berming noted along segments that approach and cross the terrace top is similar in idea to the earthworks that occur along Chacoan roads as they approach great houses (Nials et al. 1987:15). While no connection to the Chacoan system is suggested for the Ojo Caliente trail, in both this case and the case of the Chacoan roads more elaborate construction approaching ritually important locations would have generated spoils—most easily discarded by simply placing them along the road or trail. This would have further emphasized the corridor in those areas, enhancing the more elaborate aspects of construction.

Though the trail does not connect with the earth navel shrine identified at one of the farming sites, a segment located to the south of that shrine contains the longest and most elaborate approach to the terrace top of those that were documented. At the next ascent of the trail to the terrace top it was bermed along its downslope side as it approached and crossed the terrace top. The only possible shrines identified in that area were two rock piles, which may have been used as field shrines rather than representing ritually important locations accessed by the trail, though the latter cannot be ruled out. And, as a reading of Harrington (1916) shows, many Tewa shrines would be invisible to non-initiates. The next point at which

the trail ascended to the terrace top was adjacent to the elaborate double borrow pit, and its downslope side was again bermed through that area. At the final approach to the terrace top (heading north up the valley) within project limits, a short section of trail crosses the terrace top at the edge of a farming site and its downslope side was bermed, though much of the berm, the ascending section of trail, and probably whatever ritual feature occurred in that area were removed by earlier construction along U.S. 285. These were the only places in which the trail ascended to the terrace top within our study area.

However, the descent of the trail and its disappearance in an area between Hilltop Pueblo and Nuté suggests that a strictly ritual use cannot be assigned to this landscape feature. Part of its function must have been as a pedestrian corridor, otherwise, why would it disappear in this area? This idea is supported by the paths connecting the trail to the village of Ponsipa'akeri, suggesting mundane use in addition to its probable ritual function. Like some of the trails between Zuni and Acoma, LA 118549 probably had multiple purposes. As we have seen, trails could be simple pedestrian corridors, they could be paths linking important ritual locations, or they could serve both of these functions at different times. Because the trail seems to have continued south beyond Ponsipa'akeri, it may have linked the Tewa villages in the Ojo Caliente Valley with related villages along the Rio Grande, and was probably used by the inhabitants of those villages for pilgrimages to the hot spring.

CONCLUSIONS

During an archaeological study in the Ojo Caliente Valley we were able to document a trail that is almost certainly of Classic period origin that represents a pedestrian corridor with multiple uses. While the most important function of the trail may have been as a ritual corridor for pilgrimages to the hot spring situated below the village of Posi'ouinge, it also served to link several of the large Classic period villages together, probably extending as far south as the Tewa villages in the Rio Grande Valley. Farmers also undoubtedly used this convenient route to access their fields on the terrace top. But first and foremost, the trail may have represented the main route used to access the sacred hot spring, providing access to other shrines along the way.

While the hot spring is known to have important connections to Poseyemu, a further level of importance may have been assigned to this landscape feature. As in the Tewa view all waters are connected, the Ojo Caliente hot spring could also have at times served as a substitute for the lake of emergence located much further to the north. In this way, the essence of an important ritual location would have been ascribed to a landscape feature that was not as difficult or time consuming to access as the original, especially for village elders who might not have been up to making the actual pilgrimage.

Trails often seem to have led to locations of ritual importance, and this was probably an important if not the main function of this trail that runs along the east side of the Ojo Caliente Valley, though it was undoubtedly also used for more mundane journeys. Whether as a substitute for the lake of emergence or because of its association with Poseyemu, the primary endpoint appears to have been a hot spring that was one of the most ritually important landscape features for the Tewa. ☙

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There's the Site: Further Investigations at LA 33223, The Montaño Bridge Site

JOHN R. RONEY

LA 33223, also known as the Taylor Ranch Site and the Montaño Site or Montaño Bridge Site was an important prehistoric archaeological property located on the western bank of the Rio Grande (Figure 1). Its original extent was from near the intersection of Coors Boulevard and Montaño Road, eastward along both sides of Montaño Road to the Rio Grande floodplain. The site has now been largely destroyed or covered over by commercial real estate development, as well as road and infrastructure construction. However, multiple investigations took place prior to this development (see Raymond 2009b for a summary), and in 1988 the City of Albuquerque required a major data recovery project prior to construction of the Montaño Bridge. Although extensive fieldwork was undertaken at that time, circumstances prevented immediate completion of a final report. In 2008 the City engaged Criterion Environmental Services to finish the remaining analyses and to write a final report (Raymond 2009a). This paper is intended to summarize some of the major findings from earlier components at this site, and to place them in a wider context.

DATING

Twenty years elapsed between major fieldwork at LA 33223 and the final analysis and reporting of the archaeological work. One unfortunate result of this delay was the loss of about 14 radiocarbon samples, probably those that were originally selected for analysis. On the other hand, there have been important advances in radiocarbon dating during those intervening 20 years, and we were able to obtain much more reliable results than would have been possible in the late 1980s. Fortunately, the excavators collected multiple samples, and from the 11 available samples, we selected eight for Accelerator Mass Spectrometry (AMS) radiocarbon dating.

The radiocarbon samples collected in 1988 consisted of multiple pieces of charcoal from identified proveniences. In order to take advantage of the strengths of AMS dating, we first identified the plant species represented in each sample. Almost all of the 11 remaining samples included remains of annual plants, twigs, or charcoal from shrubs that would minimize error resulting from old wood and cross-section effects. We selected eight archeological samples for AMS radiocarbon dating, based on these identifications of the charred materials and on an assessment of their archaeological contexts. The dating results are presented in Table 1.

MIDDLE ARCHAIC OCCUPATION, CA. 2400 B.C.

The earliest occupation at the Montaño Bridge Site is a deeply buried Middle Archaic component, radiocarbon-dated to around 2400 cal B.C. This occupation was in the western part of the site, and was buried approximately 2.5 m below modern grade. Because of this, the Middle Archaic component is

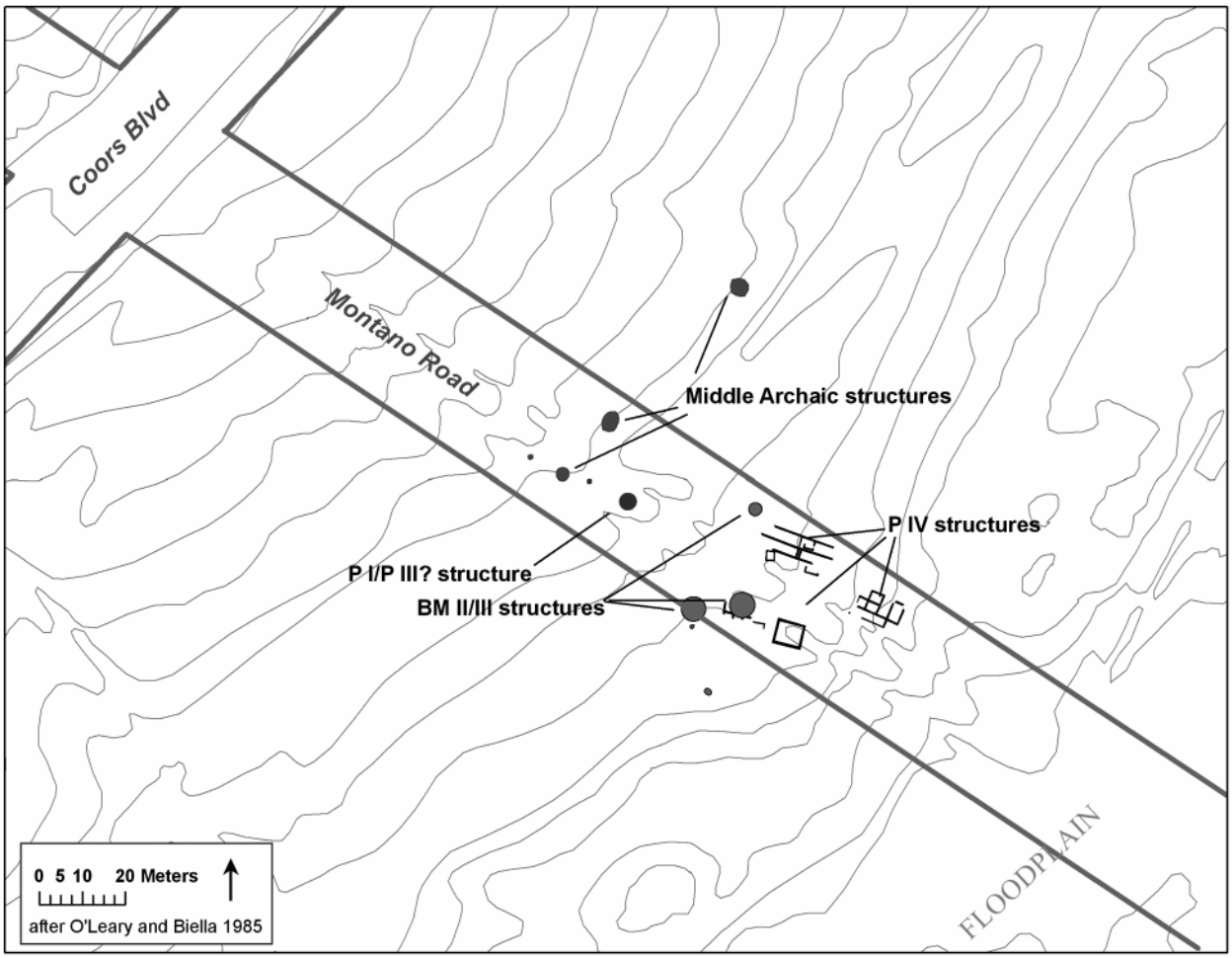

Figure 1. Major components at LA33233, the Montaño Bridge Site.

Table 1. Radiocarbon Results from LA 33233, the Montaño Bridge Site

Sample Number	Provenience	Material	Accelerator Mass Spectrometry Date (radiocarbon years before present)	Mean Calibrated Date
RC-6	Area B, F1[a]	*Atriplex* twig	3910±15	2405 B.C.
RC-12	Area A, F20, 10 cm above floor	*Atriplex* twig	1615±15	A.D. 465
RC-21	Area A, F11	*Zea mays* cupule	1610±15	A.D. 470
RC-20	Area A, F9, 15-30 cm above floor	*Zea mays* cupule	1595±15	A.D. 480
RC-16	Area A, F22, hearth in wattle-and-daub room	*Atriplex* twig	635±15	A.D. 1342
RC-14	Area A, Kiva 1, lower floor hearth	*Zea mays* cob	485±15	A.D. 1430
RC-10	Area A, Kiva 1, upper floor	*Salicaceae* twig	470±15	A.D. 1435
RC-3	Area A, F15, adobe roomblock, Room 1	*Zea mays* cob	395±15	A.D. 1462

Source Roney and Raymond 2009 [a] F = Feature

effectively isolated from later components, with a very low probability of contamination.

The component included two and possibly three shallow pit structures, as well as an outdoor activity area (Marshall 2009:4-8—4-11). These features resemble other excavated Middle Archaic components on the Albuquerque West Mesa. The three pit structures are shallow basins between 3.0 and 4.8 m in diameter and 20 to 40 cm deep. No postholes or direct evidence of a superstructure were found, but it is assumed that these structures were made of brush and logs. The activity area was located at the same depth as the two excavated pit structures, just a few meters to the west. It was a 4.0 m by 3.5 m area, with charcoal and a few lithic artifacts mixed into a 10 cm thick lens. At least two of the pit structures included internal features in the form of shallow, basin-shaped pits, all of which lacked oxidation or other evidence of use as hearths (Marshall 2009).

A single radiocarbon date was obtained from one of the structures. The sample, a twig of salt bush (*Atriplex*) (RC-6), was taken from a dark, charcoal-laden, 20 cm thick deposit resting on the floor of the structure. AMS dating of this sample yielded a date of about 2400 cal B.C. (Table 1). The other pit structures and the activity area are assumed to be of equivalent age based on their proximity and similar stratigraphic positions. The radiocarbon date places this component in the San Jose Phase of the local Archaic sequence, or the Middle Archaic in more general Southwestern terms.

Only three pieces of debitage from this component were available for analysis, so little can be said about lithic technology. However, the collections did include a large unnotched biface and a Northern Side-Notched projectile point (Okun 2009). Both of these tools were found within the structure that provided the radiocarbon date. Consequently, association between these two artifacts and the radiocarbon date is especially strong. Several quartzite manos and a sandstone metate fragment are also reported from this component (Arena 1987; Mattson 2009).

A single flotation sample from the dated pit structure provided several charred goosefoot seeds (Toll 2009). The faunal assemblage attributed to the Middle Archaic component consisted of 30 bone fragments, of which only five were identifiable to genus. Predictably, the identified species consisted of cottontail (*Sylvilagus* sp.) and jackrabbit (*Lepus californicus*) (Duncan 2009).

BASKETMAKER II/III OCCUPATION, CA. A.D. 500

The second documented occupation at the Montaño Bridge site took place at a crucial point in the culture history of the Albuquerque Basin—right at the transition between Archaic and Early Formative times. The component includes three pit structures, an apparent outdoor activity area, an infant burial, and an isolated hearth (Marshall 2009). Three AMS radiocarbon dates place the occupation at or slightly before ca. A.D. 500.

The most impressive of the three pit structures was 5.9 m in diameter and 75 cm deep, with both floors and walls covered with a 5 to 10 cm thick layer of adobe. A second pit structure at the site was similar in size, but walls and floor were unlined. The third pit structure attributed to this occupation was smaller (3.2 by 2.85 m), and was shallow, making it comparable to pit structures of the preceding Archaic Period. However, its floors and walls were also lined with adobe. Each of the three structures had one or more interior hearths, as well as interior pits and basins presumably used for storage. One featured a bell-shaped pit, a more specialized storage facility often associated with early dependence on agriculture. There was no evidence of a roof support system, and no evidence of entryways at any of the three structures.

The apparent outdoor activity area was a shallow basin that included three small pits and a small concentration of fire-cracked rock. A small number of intrusive Glaze A red and yellow slipped ceramics were found in this group of features, but a maize cupule yielded a radiocarbon date of about cal A.D. 470 correlating closely with the Basketmaker II/III

occupation. In addition, O'Leary and Biella (1987) attribute a burial found just outside one of the pit structures (and left in situ) to this occupation based on stratigraphic relationships.

Okun (2009) analyzed over 200 pieces of lithic debitage from this component, and found that in technological characteristics they closely resembled the much larger sample from later Pueblo IV contexts. However, the Basketmaker II/III lithic assemblage was notable in the number of formal and expedient tools it contained. Expedient tools included eight retouched flakes, eight used flakes, one hammerstone, an expedient drill/perforator, an early stage biface, and an informal uniface. Formal chipped stone artifacts included three bifaces and 17 projectile points. Two of the projectile points are unnotched, and could be classified as Cottonwood Triangular; 16 are broadly similar to the Trujillo type.

The groundstone assemblage from this component included several unusual artifacts, such as two fragments of a pipe made from pumice, a stone bowl also made of pumice, and an unworked piece of sandstone with yellow pigment on one surface. Other groundstone tools included two handstones and a quartzite mano (Mattson 2009).

In the three structures attributed to this component, only one sherd (a Plain Gray utility ware) was found in floor contact. A small amount of ceramic material identified as Lino Gray and Fugitive Red was found in other contexts on the site, but cannot be confidently associated with the features attributed to this component. This could indicate that little or no pottery was being used at the Montaño Bridge at ca. A.D. 480, despite the appearance the bow and arrow, plaster-covered walls, and other characteristics usually associated with early Basketmaker III components.

An assemblage of 102 faunal elements was available from the Basketmaker II/III component. The most common identifiable bone was rabbit, both jackrabbit and cottontail. Other identified species included rock squirrel, pronghorn antelope, and Cervidae (either deer or elk). One possible piece of worked bone seemed to be part of a Bitsitsi whistle, although this is not certain. (A Bitsitsi whistle consists of two small, identical, rectangular segments of long bone shafts, usually bird bone, bound together with the concave sides facing each other and a piece of grass or other material between them to serve as a reed [Beach and Causey 1984:210]. The whistle, still in use today in the Southwest, is carried between the lips of a ceremonial personage—and is invisible to an onlooker.) The faunal assemblage from this component also included several pieces of worked and unworked shell. The two worked pieces were both ornaments. One was a fragment of clam-like bivalve mollusk shell (possibly *Mercenaria*) worked into a bracelet or pendant with a drill hole. The other was a spiral shell drilled in the center (Duncan 2009).

Flotation samples were processed from the large pit structure and the smallest pit structure. Both included corn, but the most exciting discovery was a charred bean cotyledon (*Phaseolus vulgaris*), an important cultigen that is seldom preserved in the macrofossil record. Other economic species included charred remains of tansy mustard, goosefoot, purslane, juniper seeds, and ricegrass. According to Toll (2009) seeds of these species are available from early through late summer. Pollen was also analyzed from these proveniences. Extremely high frequencies of Cheno/Am pollen suggested gathering and use of these plants for food. Other potential economic plants that were well represented in the pollen record include sunflowers, mormon tea, sage, cholla, and four o'clock (Dean 2009).

LATER COMPONENTS

The focus of this paper is on the earlier components at LA 32233, but brief mention should be made of the subsequent occupations represented at this site. There is a substantial pithouse that dates sometime prior to Pueblo III times. Architecture and pottery apparently associated with the structure (a predominance of plain gray with traces of corrugated-indented, plain fugitive red, and Red Mesa Black-

on-white) suggest Pueblo I affinity (Marshall 2009), but three radiocarbon dates suggest a Pueblo II-III temporal affiliation (Harper 1988).

The most intensive occupation at the Montaño Bridge Site was during Pueblo IV times, which can be divided into several episodes. The earliest may be two semi-subterranean wattle and daub structures, one of which yielded a radiocarbon date from a saltbush twig of around cal A.D. 1330 (Table 1). In any case, by Glaze A times (ca. A.D. 1460) there was a substantial Puebloan occupation. A large midden and a kiva dating to this interval were excavated, although the residential structures that presumably accompanied these two features were not found. Subsequently, the kiva was abandoned and filled with Glaze C trash. This trash deposit was associated with a roomblock built over the earlier Glaze A midden. The onset of Glaze C ceramics is bracketed by radiocarbon dates between ca. cal A.D. 1435 and 1463 (Table 1). Final abandonment of the site occurred around A.D. 1500, prior to the onset of Glaze D ceramics (Franklin 2009).

COMPARATIVE DATA

In recent years there have been a number of archaeological excavations in the Albuquerque area, especially on the West Mesa. In an effort to place results from the Montaño Bridge Site into a wider context, I reviewed literature from testing and excavations in the Albuquerque area, defined loosely as the area covered by 16 USGS 7.5 minute topographic maps roughly centered on the Montaño Bridge Site. This corresponds to the area covered by Connell's (2008) recent detailed geologic map of the Albuquerque-Rio Rancho Metropolitan Area. It is an area bounded on the west by the Cejo del Rio Puerco and Rio Puerco itself. Its northern boundary reaches to areas near San Ysidro and San Felipe Pueblo, taking in the lower reaches of the Rio Jemez. On the east it includes the Creston de Moctezuma and the easternmost slopes of the Sandia Mountains, and on the south it ends just a little beyond Tijeras Arroyo.

Over 300 radiocarbon dates are available from archaeological excavations in this area. I reviewed this data emphasizing associations between chronometric dates and cultigens, diagnostic artifacts, and architecture. The radiocarbon dates were calibrated using OxCal 4.1, employing the IntCal 09 curve (Bronk Ramsey 2009). As most readers know, a radiocarbon date is actually a range of possible dates. For ease of presentation, I frequently use the mean calibrated date, which is the calendrical date calculated by averaging the upper and lower boundaries of the one standard deviation interval.

When Does Corn First Appear in the Albuquerque Area? It is not surprising that no evidence of agriculture was found in the Middle Archaic component at the Montaño Bridge Site. The earliest corn (*Zea mays*) yet discovered in New Mexico was found at the Old Corn Site near Quemado, dating to about 2260 cal B.C. (Huber and VanWest 2005; Merrill et al. 2009). Therefore, the Archaic component at the Montaño Bridge site predates the earliest known maize in New Mexico by about 150 years. Even taking the standard error of the radiocarbon date into account, it is probable that the Middle Archaic component predates the introduction of maize agriculture in New Mexico.

When was maize first used in the Albuquerque area? Despite a surprisingly large number of excavated Middle and Late Archaic sites, we do not yet have a definitive answer. Table 2 tabulates reports of possible early maize in the Albuquerque area and places them in the context of well documented occurrences of corn elsewhere in New Mexico. As the table indicates, there are doubts about all of the earliest possible discoveries of early corn in the Albuquerque area. Some are based on tenuous phytolith evidence (Jones-Bartholomew et al. 2002:67-71), and in other cases identification of corn cupules is not definite (Jones-Bartholomew et al. 2002; Seymour et al. 1997:278). Several reports document maize in questionable contexts, where associations between maize remains and the radiocarbon date are not

Table 2. Possible Early Occurrences of *Zea mays* throughout New Mexico and in the Albuquerque Area

New Mexico		Albuquerque Area		
Site	**Date**	**Site**	**Date**	**Evidence**
		LA 133528	3210 B.C.	Phytolith?
Old Corn Site	2260 B.C.			
Bat Cave	2150 B.C.			
		LA 64724	1825 B.C.	Cupules?
		LA 107577	1642 to 1250 B.C.	Pollen?
Las Cruces	1510 B.C.			
San Luis de Cabezon	1400 B.C.			
		LA 131379	1195 B.C.	Cupules?
		LA 64741	785 B.C.	Pollen?
		LA 65515	500 B.C.	Cupules

secure (Fletcher 1997:73; Kovacik 1998:73-81).

Seymour et al. (1997:134-173) make a good case for pollen evidence in contexts dating between 1642 cal B.C. and 1250 cal B.C. at LA 107577, a site at the Sandoval County Landfill. In one instance the pollen sample that included *Zea mays* was taken from beneath a metate fragment; in the other two cases the samples were taken from cultural features. There was some ceramic period activity at LA 107577, but for the most part later occupations are spatially separated from proveniences yielding corn pollen. Still, pollen grains are microscopic objects that move readily through sediments, and for that reason I am nervous about accepting the presence of early maize based on pollen evidence alone. The best pollen evidence comes from pollen grains or clumps of grains trapped on working surfaces of manos or other tools. Short of that, the evidence from this site is about as solid as pollen evidence can be.

At this time, the earliest reliably identified maize macrofossils in the Albuquerque area appears to be from LA 65515, where Elyea (1995:4-46 ff.) reports corn from hearths and storage features in two superimposed pit structures. Juniper charcoal from the floors of the two structures yielded two radiocarbon dates right around 500 cal B.C. After 500 B.C., corn is common but by no means ubiquitous in macrobotanical assemblages from the Albuquerque area.

The Projectile Point Sequence

Archaic age projectile points are especially important in Southwestern archaeology because they are often the only means of dating pre-ceramic surface assemblages. Changes in projectile point morphology provide one of the major underpinnings of local phase sequences such as those proposed by Reinhart (1967a, 1967b), and by Irwin-Williams (1973). Because of this, documented associations between projectile points and chronometric dates are especially important. Table 3 tabulates those associations in the Albuquerque area which seemed most secure.

These data corroborate our current understanding of changes in projectile point morphology. Large side-notched dart points, including one from LA 33223, are associated with radiocarbon dates of 2400 B.C. and 1780 B.C. The Late Archaic Period (1500 B.C. to A.D. 500) is represented exclusively by Medio and San Pedro points, with seven examples occurring between 500 B.C. and A.D. 500. At the Montaño Bridge Site, 14 Trujillo points were found in a component dating slightly before A.D. 500. The small corner-notched Trujillo points are thought to be the earliest indicators of bow and arrow technology in the Albuquerque area.

When Are Ceramics First Used in the Albuquerque Area?

In the culture historical framework widely accepted for the Albuquerque area, the Archaic Period (including Basketmaker II) ends and Basketmaker

Table 3. Secure Associations between Projectile Points and Radiocarbon Dates

Site	Provenience	Radiocarbon Date	Mean Calibrated Date	Point Type	Reference
LA 33223	Area B, F1[a]	3910±15	ca. 2400 B.C.	Sudden Side-Notched	Okun 2009:8-31
LA 65522	F40	3450±80	ca. 1780 B.C.	San Rafael Side-Notched	Elyea 1995:4-119
LA 65515	F2(2)	2380±60	ca. 500 B.C.	En Medio	Elyea 1995:4-46—4-57
LA 65515	F2	2540±130 and 2380±60	ca. 500 B.C.	En Medio	Elyea 1995:4-46—4-57
LA 109100	FA1	2190±60	ca. 270 B.C.	En Medio	Brandi and Dilley 1998:17-33
LA 65536	single component site	2240±60 to 2050±60	ca. 200 B.C.	2 San Pedro	Elyea 1995:4-86—4-98
LA 109100	FA	2130±60	ca. 200 B.C.	En Medio	Brandi and Dilley 1998:17-33
LA 133525	F3	1600±80	ca. A.D. 450	Trujillo	Jones-Bartholomew et al. 2002:42-47
LA 33223	F20	1615±15	ca. A.D. 460	8 Trujillo	Okun 2009:8-33
LA 33223	F9	1595±15	ca. A.D. 480	6 Trujillo, 2 Cottonwood Triangular	Okun 2009:8-34
LA 32795	F1(1)	1870±70 to 1600±70	ca. A.D. 500	En Medio	Elyea 1995:4-57—4-75

[a] F=Feature

III begins with the introduction of pottery at around A.D. 400. Identifying the earliest occurrence of pottery in the Albuquerque area is a little tricky because it requires high resolution dates and because old wood and cross-section effects are often problems, especially with conventional radiocarbon dates. Both old wood and cross-section effects result in radiocarbon dates that are older than the cultural activity they are supposed to date.

The earliest reasonably well dated ceramics in the Albuquerque area seem to be from LA 60839, a site located on the eastern side of the Rio Puerco floodplain, west of Albuquerque (Gerow 1998). It is a large and complex site that includes Late Archaic, Basketmaker III, and Pueblo I occupations. Structure 1 was a 2.5 m diameter pithouse with a four-post roof support system and a roof entry. A total of 149 sherds were recovered from the structure, including Lino Gray, Lino Fugitive Red, and San Marcial Black-on-white. Two Lino Fugitive Red jars were found in floor contact, providing a secure archaeological context for the pottery. One radiocarbon date from a juniper post was rejected as too old, but a second sample taken from the outer rings of a juniper post provided a date of cal A.D. 336. We can assume that an unknown number of rings have been lost from the sample, so actual use of the pit structure and its associated ceramics is somewhat after this date, very close to the traditional estimate of A.D. 400 for initial adoption of pottery. However, the Montaño Bridge Site provided a date from a corn cupule of A.D. 580 found in a substantial pit structure that yielded little or no pottery. This seems to imply that use of pottery, like maize, did not become ubiquitous immediately after it was introduced.

How Does Architecture Develop in the Albuquerque Area?

Table 4 tabulates 42 excavated Archaic Period pitstructures with associated radiocarbon dates. The earliest suspected pit structure is at LA 133528, radiocarbon dated to ca. 3120 B.C. (Jones-Bartholomew et al. 2002: 67-71). With radiocarbon

Table 4. Radiocarbon Dates for Pit Structures in the Albuquerque Area

Site #	Provenience	Radiocarbon Date	Mean Cali-brated Date	Max. Dia(m)	Depth (cm)	Unusual Characteristics	Reference
3500 B.C.							
LA 133528	F2[a]	4410±40	3120 B.C.	6.4?	50		Jones-Bartholomew et al. 2002:67-71
LA 139284	F3	4130±30	2693 B.C.	3.45	28	clay floor	Higgins and Lundquist 2004:98-99
LA 131379	F13	3950±40	2457 B.C.	2.5	10		Jones-Bartholomew et al. 2002:31-42
LA 65522	F7(1)	3950±100	2452 B.C.	4.0	11		Elyea 1995:4-98—4-121
LA 33223	BF1	3910±15	2400 B.C.	4.8	40		Marshall 2009
LA 65519	20-3	3770±120	2230 B.C.	3.6	19	2 posts	Kovacik 1998:180-190
LA 103048	F11	3710±40	2114 B.C.	3.3	30	3 posts	Higgins and Lundquist 2004:43-73
LA 65522	F3	3610±180	1985 B.C.	5.0	—	3 posts	Elyea 1995:4-98—4-121
LA 64741	20-10	3490±90	1810 B.C.	2.4	19		Fletcher 1997:56-79
LA 65522	F1(1)	3470±80	1790 B.C.	4.0	10	superimposed floors	Elyea 1995:4-113—4-116
LA 65522	F40	3450±80	1783 B.C.	5.2	12	rectangular floor plan	Elyea 1995:4-119—4-121
LA 109109	AF2	3200±70	1504 B.C.	2.8	19		Brandi and Dilley 1998:137-150
1500 B.C.							
LA 107577	F4	3050±50	1329 B.C.	3.0	10		Seymour et al. 1997:134-173
LA 107577	F4	3000±50	1250 B.C.	3.0	10		Seymour et al. 1997:134-173
LA 109117	F1	2670±80	851 B.C.	3.3	13	20 posts around circumference	Brandi and Dilley 1998:248-261
LA 64741	20-9	2650±90	785 B.C.	3.75	5		Fletcher 1997:73
LA 100420	F2	2470±70	592 B.C.	2.0	12		Acklen et al. 1995:67-76
LA 100420	F2(4)	2530±60	670 B.C.	2.0	12		Acklen et al. 1995:67-76
LA 109109	BF1	2460±80	585 B.C.	3.9	23		Brandi and Dilley 1998:137-150
LA 137832	F4	2370±80	561 B.C.	2.6	50		Higgins and Lundquist 2004:74-99
LA 65515	F2(1)	2540±130	523 B.C.	4.0	11	3 posts, vertical edges	Elyea 1995:4-46—4-57
LA 65515	F2(2)	2380±60	493 B.C.	3.9	25	2 posts	Elyea 1995:4-46—4-57
LA 64741	20-12	2240±90	297 B.C.	3.5	8		Fletcher 1997:77
LA 109100	FA1	2190±60	273 B.C.	3.8	—	3 posts	Brandi and Dilley 1998:17-33
LA 109105	F4	2190±70	271 B.C.	2.5	20		Brandi and Dilley 1998:43-67
LA 109110	F1	2160±60	234 B.C.	3.5	30		Brandi and Dilley 1998:151-163

Site #	Provenience	Radiocarbon Date	Mean Cali-brated Date	Max. Dia(m)	Depth (cm)	Unusual Characteristics	Reference
LA 65536	F1	2130±60	201 B.C.	3.7	50	entry ramp, possible antechamber, internal 4-post roof support, vertical edges, described as true pithouse	Elyea 1995:4-86—4-98
LA 65534	F2	2100±70	191 B.C.	4.0	12		Elyea 1995:4-75—4-85
LA 54632	Dw1	2108±206	140 B.C.	6.35	30	6 possible post holes around perimeter	Reinhart 1967b; 1968
LA 65522	F53	2000±60	8 B.C.	3.2	23	vertical edges	Elyea 1995:4-98—4-122
LA 32795	F1(4)	1810±70	A.D. 151	5.0	—	3 posts, entryway	Elyea 1995:4-57—4-75
LA 32795	F1(4)	1770±80	A.D. 241		—		Elyea 1995:4-57—4-75
LA 51581	20-4	1860±50	A.D. 153	3.1	12	1 post	Fletcher 1997:27
LA 65534	F1	1820±80	A.D. 203	4.0	15	1 post	Elyea 1995:4-75—4-85
LA 109114	F1	1810±70	A.D. 225	4.55	15	11 posts around circumference	Brandi and Dilley 1998:200-247
LA 109114	F2c	1920±70	A.D. 106	4.55	15		Brandi and Dilley 1998:200-247
LA 60840	F2(1)	1790±50	A.D. 229	3.5	10	3 posts	Gerow 1998:47-53
LA 32791	F9(1)	1760±90	A.D. 262	4.0	11		Elyea 1995:4-5—4-10
LA 32795	F1(3)	1750±70	A.D. 303	5.0	—	3 posts, antechamber	Elyea 1995:4-57—4-75
LA 32791	F9(u)	1710±100	A.D. 304	4.0	9		Elyea 1995:4-5—4-10
LA 60836	F1	1750±50	A.D. 306	3.35	12		Gerow 1998:37-49
LA 60840	F2(u)	1710±70	A.D. 328	3.5	—	2 posts	Gerow 1998:47-53
LA 60839	F38	1690±80	A.D. 336	2.5	42	internal 4-post roof support, roof entry, ceramics	Gerow 1998:55-69
LA 32795	F1(2)	1600±70	A.D. 468	5.0	—	2 posts	Elyea 1995:4-57—4-75
LA 32233	F20	1615±15	A.D. 469	6.0	75		Marshall 2009
LA 32233	F9	1595±15	A.D. 478	3.0	30		Marshall 2009
LA 65522	F39(1) F39(u)	1560±50 1620±70	A.D. 488 A.D. 447	2.5 2.7	60 26	unusual split-level structure with two chambers, 9 post holes around perimeter, entry ramp	Elyea 1995:4-98—4-122
A.D. 500							
LA 109129	ST2(F8)	1510±60	A.D. 526	7.5	40	likely antechamber, likely 4-post roof support	Walth 1999

[a] F = Feature

dates slightly before A.D. 500, the Basketmaker II/III component at the Montaño Bridge Site is among the latest. Throughout this long interval, residential architecture is remarkably consistent. Pit structures are shallow, circular or oval depressions ranging between 2.4 and 4.0 m in maximum dimension. They are usually described as basin- or dish-shaped, between 5 and 30 cm in depth. Often, but not always, they have interior features, such as hearths and basins. Postholes are found in only about a third of the excavated examples. When present, there are almost always between one and three postholes inside the structure near the perimeter. When definable walls are present, they are not plastered, and floors are defined by use compaction.

Within this broad pattern, however, there is significant variation. For example, the floor of a pit structure at LA 139284, dubbed the Clay Liner Site, was intentionally plastered with clay almost 3000 years before this became a common practice. A pit structure at LA 65522, dating to around 1800 B.C., seems to have had a rectangular rather than ovoid floor plan. At least three pit structures built during the Archaic Period have multiple post holes around their circumference: 20 postholes at LA 109117, 11 at LA 109114, and six at LA 54632.

Among Archaic Period pit structures in the Albuquerque area, multiple living surfaces are not uncommon, with as many as four different floors superimposed over one another. This suggests a cyclical pattern, perhaps seasonal movement, on the landscape. Extramural pits and roasting features are also sometimes present at Archaic Period settlements.

Significant architectural change becomes apparent by about 200 B.C., first evident in this data set at LA 65536. Here Feature 1 is a pithouse similar to other Archaic pit structures in overall dimensions. However, it is 50 cm deep with vertical walls, an internal four-post roof support system, an entry ramp, and a possible antechamber. Its excavators describe it as the first true pithouse. These innovations reflect a greater investment in materials and construction, and probably reflect increased permanence of occupation. However, the innovations were not adopted universally, and most subsequent Archaic pit structures follow the usual pattern. Another structure with an antechamber is documented at ca. A.D. 300 (LA 32795), and a structure with an internal four-post roof support system and a roof entry was built at about A.D. 330.

SUMMARY AND CONCLUSIONS

This paper describes archaeological evidence for the Middle Archaic component and the Late Archaic/Basketmaker III component at LA 33223, the Montaño Bridge site. These two components are then considered in the context of other chronometrically dated archaeological materials in the wider Albuquerque area. From this perspective, the Middle Archaic component at LA 33223 is fairly typical of occupations throughout the Archaic Period in terms of architecture, material culture, and subsistence-related evidence.

The Late Archaic/Basketmaker III component, radiocarbon dated to slightly before cal A.D. 500 falls within a period of major cultural change. After the long and stable Archaic Period, a number of innovations appear. These include the introduction or adoption of pottery and the bow and arrow, construction of deeper, larger, and more permanent pit structures, the use of a four-post roof support system in pit structures, the more frequent use of adobe plaster in pit structure interiors, construction of antechambers and other novel entry arrangements. Reliance on agriculture seems to increase, and bell-shaped storage pits become common. Although not discussed in this paper, settlement patterns also change at about this time, with decreasing use of interior portions of the Albuquerque West Mesa, and increasing emphasis on locations along the Rio Jemez, the Rio Puerco, and the Rio Grande.

Cumulatively, these changes are very pronounced, and there are at least three broad models describing how they may have progressed: 1) They may have

resulted from rapid internal change within the local Archaic tradition; 2) They may have resulted from population replacement, with new peoples introducing new technologies and a different economic emphasis; 3) Finally, they may have resulted from ethnogenesis, a combination of the preceding two scenarios resulting in a new and greatly altered cultural adaptation.

Each of these models has different implications, and more precise chronological control is an important tool that can help us distinguish among them. Recent advances in radiocarbon dating provide dates with tight confidence intervals and make it possible to minimize other unquantified sources of error such as old wood and cross-section effects. These steady improvements in the precision of radiocarbon dating promise much more precise chronological control and should allow us to address these issues in a convincing way. ☙

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Situating "Others": Historic Pueblo Migrants— Some Implications for the Archaeological Record[1]

DAVID H. SNOW

HISTORIC MIGRANTS

Timothy Pauketat (2008:241) reminds us that what we call Pueblo communities in the Southwest,

> are wracked with tensions and inequalities, may be variously engendered, and can be composed of people with dramatically different histories, memories, origin stories, and affiliations. In a very real sense, some Southwestern "communities" were not shared identities at all!

Indeed, the ethnographic and historical records underscore Pauketat's observations and, in some cases, such tensions and inequalities appear to have their roots in the assimilation of "others" into existing communities. In his Foreword to Elsie Clews Parson's study of Jemez Pueblo, Kidder noted that the Pecos emigration to Jemez "is of exceptional interest, as it reflects a process which has been going on in the Southwest for many centuries" (Parsons 1925:xi-x), a process that continues to reverberate. Both Parsons and Joe Sando (1982:149), of Pecos heritage, commented on tensions between Jemez people and those of Pecos descent, and noted that recent efforts by the latter to have returned to them a sacred cave in the upper Pecos River canyon are a reminder of their separate history and identity (and see Levine 1999:156-157).

Conservative Laguna migrants to Isleta, ca.1880, consisted of "seven male heads of family," including 12 men and women. These were followed by others "with the children" (Parsons 1932:349), to form a "colony" of some 30 to 40 persons. Following negotiations with Isleta's religious leaders, the migrants were provided land for residences and farming at the place Isletans called "Oraibi." Among the migrants were the "principal ceremonialists" who brought their "mothers" with them, and they were met outside Isleta by the cacique, war chief, and governor, who informed them of conditions for settling there, in return for the ritual and ceremonial "power" of the Laguna religious leaders (Euler 1954:371).[2] In the early 1920s, the Indian agent noted that Isleta's "reactionary element" was in the majority, "but there is a Bull Moose ["progressive"] faction, led by such men as Pablo Abeita...." (Crane 1928:315). "Progressive," Pablo Abeita, according to Parsons, was said to "hate the Laguna people," that he wanted to "cut their ways," and "keep up his own ways," and that "he wants to destroy the others." "In general," Parsons wrote, "Laguna descendants are bilingual, but Keres had not been learned by the Isletans," and her informant said the Laguna people still provided their own secular offices of governor and *teniente* (similarly, at Pecos [Levine 1999:156-157]).

Hopi people at Oraibi recounted to Mischa Titiev (1944:155) the story of the arrival there of the Kokop clan, who

> reached Oraibi after they had formerly lived in a home situated far to the northwest. At first Matcito refused to admit them to his pueblo, so they circled to the south of the mesa and made a temporary settlement there.

In return for aid in defeating Oraibi's enemies, Matcito spoke to the Kokop people: "Now I'll let you come up to my mesa to stay" (Titiev 1944:155-156). Many will recognize this same theme as a condition for the acceptance of Rio Grande Tewas by the Hopis at First Mesa. Negotiations with Hopi leaders resulted in the refugees serving as a buffer community against Hopi's enemies; but the Tano-Tewa of Hano relate their initial ill-treatment by the Hopi—treatment that resulted in the Tewas "burying" their language so that Hopis might never learn it (Walker 1983:60-61). Walker continued, noting,

> It is virtually certain, for example, that the first generation of Tewa immigrants lacked a matrilineal kinship system, which is the cornerstone of Hopi social organization. Their rituals and religious beliefs were, no doubt, similar to those of the Hopis, but their emphasis on curing rituals, rather than weather control, must have made their religious beliefs and practices appear to orthodox Hopis as, at best, deviant and, at worst, perverted travesties of the eternal truths of Hopi orthodoxy.

Sandia Pueblo, resettled in 1748 by Tiwas who fled to Hopi during the Revolt, included an unknown number of Hopis (whose home villages remain unidentified). Bishop Tamarón wrote in1760 that Sandia was a pueblo of Moqui and Tigua Indians who "live apart in their tenements" (Adams 1953:202-203), as was subsequently described in 1776 by Fray Dominguez (Figure 1). It consisted, he wrote, of two small plazas: the Tiwa people "somewhat south of the church," and the "Moquis in some small, badly-arranged houses above the church to the north"

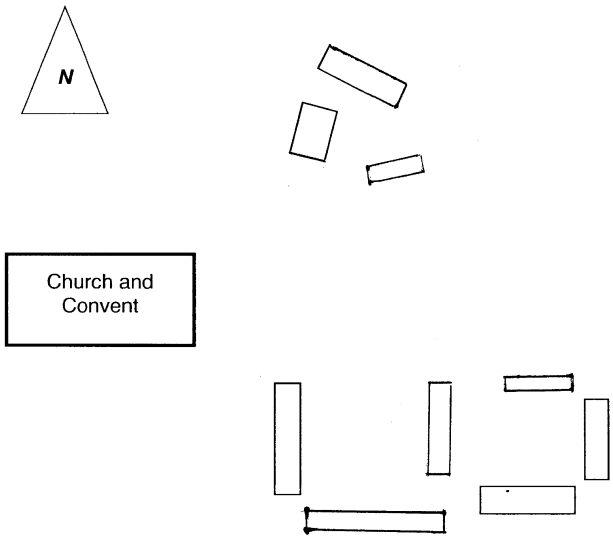

Figure 1. In 1776, Fray Domingues described Sandia Pueblo as being "arranged and built in three small blocks, or buildings, up in a northerly direction and two small plazas down somewhat to the south...the Moquis live in some small badly-arranged houses above the church to the north...." (Adams and Chavez 1956:89, 143).

(Adams and Chavez 1956:143). Sandia's plaza today, only some 200 ft from north to south, is an intimate space that, nevertheless, was considered something of a social barrier in 1970. Sandia, said one informant, consisted of "true Sandias," or Tiwas, and a "colony of other Indians who resided in a now-abandoned group of homes just north of the pueblo." The ethnographer was told that in the past, "one just didn't take a walk to the opposite side of the plaza to visit, as it was "too far." Said one individual of another, "He lived way on the other side of the pueblo. It was too far to walk"—even though people "might walk several miles to tend their fields" (Simons 1970:274). Intermarriage between the "true Sandias," or Tiwas, and "others" there, was said to have been disallowed formally (Simons 1970:45). Clearly, there is here tension resulting from the physical integration of "others" into the dominant social and organizational structure.

Re-locations of Pueblo people are well-documented, beginning with the 1540 occupation of "Alcanfor" pueblo by Coronado's army among the Southern Tiwas, and the establishment of Oñate's

colony at Yungueowingue in 1598. How many were involved in each case, and how their "integration" with neighboring villages might have affected the subsequent architectural configurations (as well as the social and organizational structures of both groups) we likely will never know. Individual ties to Yungue and its lands, however, remain strong at San Juan today (Richard I. Ford, personal communication, 2010). Earlier still, it must be assumed that the inhabitants of one or more of the pueblos destroyed by Vasquez de Coronado's army in 1540-41 took refuge with neighboring villages, although the possibility that some Southern Tiwa peoples simply left the Rio Grande for other regions as a result has not been recognized archaeologically.[3] We lack details of the Franciscan's seventeenth century *reducción* efforts to congregate the potential mission field into a "manageable" number of centers, but it is clear that such efforts were largely successful[4] (Hackett 1937:108; Hodge, Hammond, and Rey 1945:64).

The Revolt Period and its aftermath resulted in major re-locations of people across the Pueblo world, in addition to the Tano-Tewa migrations to First Mesa, and Tiwas to Payupki. Laguna Pueblo, established ca. 1696-97, was founded by refugees from different communities; according to Escalante:

> Most of the Queres Indians, originally from the pueblos of La Cieneguilla, Santo Domingo, and Cochiti, who had taken refuge at the Peñol of Acoma during the revolt of the year 96, came down, and at the end of the year 97 they established themselves four leagues to the north...on a small river they now call Cubero...about a mortar shot from La Laguna (Adams ca. 1963:284).

Accounts also refer to dissident Acomas and refugees from Zia among the founders of Laguna (Ellis 1966; Espinosa 1942:23). Dominguez' 1776 description of the Pueblo's layout might reflect this diverse makeup:

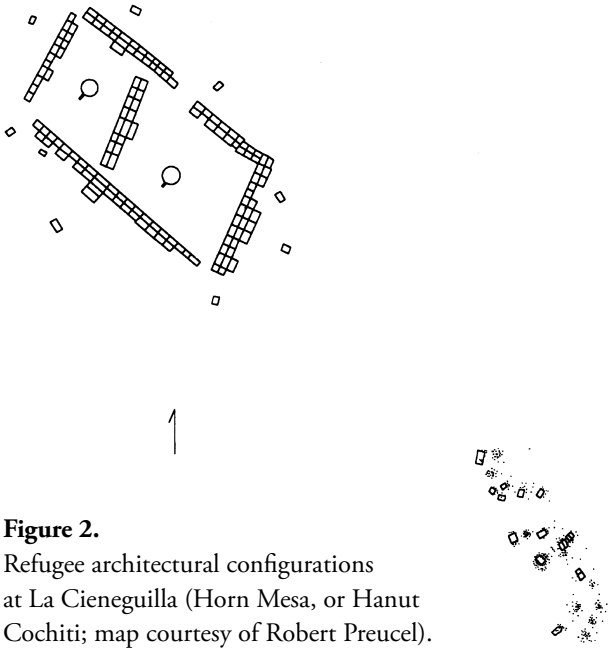

Figure 2.
Refugee architectural configurations at La Cieneguilla (Horn Mesa, or Hanut Cochiti; map courtesy of Robert Preucel).

> The pueblo...consists of seven tenements or blocks, of dwellings, four of which form two intricate little plazas in full view of the church....Of the other three, one is next to the convent and extends to the south, facing east; another is farther down facing the plain to the south; and the third is behind the little plazas more or less to the north, rather far from the convent and facing south (Adams and Chavez 1956:187).

Refugee locations on the Potrero Viejo above Cochiti (called by the Spaniards, La Cieneguilla),[5] and on Black Mesa are illustrated in Figures 2 and 3. On Zuni's Dowa Yalanne were harbored disparate groups of Pueblo refugees whose habitations, albeit temporary, are perhaps indicated by upwards of 30 and more discrete architectural units over an area of roughly 130 acres (Ferguson 1996:48, 54; Mindeleff 1989, Plate LX), almost certainly including Revolt Period construction by refugees from the Rio Grande (e.g., Kessell et al. 1998:754, 969; Kessell et al. 2000:227). At La Cieneguilla were predominantly Rio Grande Keresans, but a passing comment by

Figure 3. Black Mesa north of San Ildefonso Pueblo. Some five or more architectural complexes are visible in addition to the large central community.

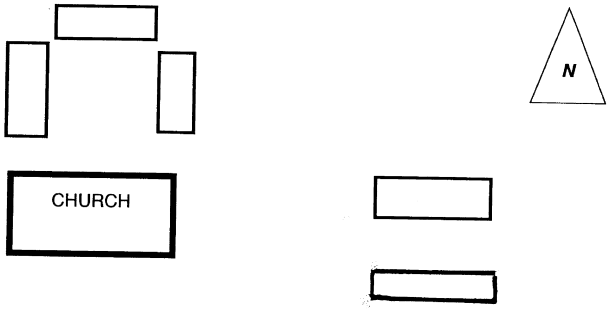

Figure 4. At San Juan Pueblo, "the buildings of the pueblo are to the north," enclosing a plaza on the right side of the church; and "opposite the church and about a pistol shot from the facade are two small tenements that make a kind of street, for one is back of the other and both face south" (Adams and Chavez 1956:143).

Vargas suggests the presence of an unidentified contingent of Tewas, possibly from Tesuque (Kessell et al. 1995:410, 432). Perhaps this accounts for the high frequency of Tewa pottery at the "outlier" unit (LA 85 [Capone and Preucel 2002]). On Black Mesa, in 1696, were counted not only Tewas from most of their pueblos, but also people from Taos, Picuris, Jemez, San Marcos, and Cochiti (Kessell et al. 1998:200, 230, 235). Vargas reported, in March of 1695, that "the house block and dwellings at San Juan Pueblo, constructed by people of San Cristobal and San Lazaro, are today abandoned and standing since only a few years ago they went to settle the lands and haciendas" of the Spaniards (Figure 4). Subsequently, he ordered them to "re-occupy their communal dwelling at San Juan...." (Kessell et al. 1998:230, 607). Fifteen children of "Tigua" families resident at Oke Owingue were baptized there between 1726 and 1729, but the origins of those Tiguas is not provided (Montoya and Martinez 1994).

In 1703, a "majority" of Zuni families fled to the Hopi Mesas in fear of reprisals from Spanish authorities. There they "built their houses and remained for some time on the Peñol de Gualpi, between the pueblo of the Tanos and of the Gualpis" (Sichomovi [Adams ca. 1963:297; Fewkes 1973:18]).

Some Tewas (and possibly Hopis as well) returned from Hopi to the Rio Grande and settled at Abiquiu to form the "plaza of Moqui" where a delegation of Hopi-Tewas recently paid a visit (also see Ebright and Hendricks 2006:251). Twenty-seven Zunis sought baptism, in 1760, at Isleta (Adams 1953:299), but whether any remained there is not stated (Zuni, however, is a prominent surname at Isleta today). By 1792, the few surviving Galisteo Pueblo natives took refuge at Santo Domingo where their descendants were noted by Whipple in 1852 (1956) and, later, by Bandelier (Lange and Riley 1966:142).[6] The 1750 Spanish Colonial census of Taos Pueblo counted a separate "section" (of seven) inhabited by seven households of Apaches (16 persons), but the location of the seven "sections" are not described (Olmsted 1981:46).

SITUATING OTHERS

Efforts to identify prehistoric Southwestern migrants have resulted in a substantial body of literature in recent years (Anthony 1990; Bernardini 1998; Cameron 1995; Clark 2001; Cordell 1995; Duff 1998, 2002; Herr and Clark 1997; Lyons 2003; Neutzil 2008; Spielmann 1998). Jeffrey Clark's definition of migration emphasizes the movement of social units with the intention of "long-term residential relocation beyond community boundaries...as a result of a perceived decrease in the benefits of remaining residentially stable or a perceived increase in the benefits of relocating in prospective destinations" (Clark 2001:2). Such moves must also include the dispossessed and refugees from strife or catastrophe, no matter their "social" composition. While a household, or an extended family group, might have been the most common prehistoric decision-making and migrating units (Cordell 1995; Habicht-Mauche 1993; Neutzil 2008:91-92), the recognition of their footprints in the archaeological record, in most cases, is doubtful (e.g., as a "site-unit intrusion" [Cordell 1995]). Nevertheless, one goal of current Southwestern archaeologists is the recognition of "socially-bounded groups" embedded in larger communities, whose identities might account for historical and contemporary Puebloan social, ethnic, and linguistic complexity" (Duff 2002:29), as well as the tensions and inequalities that persist. Migration might result in the masking of one group's history and identity by the other, but it might not, and the historic footprints and contemporary ethnographic accounts of Pueblo migrants indicate that a closer look might reveal possible analogs for the recent prehistoric past.

In a discussion of prehistoric Pueblo architectural arrangements, Potter (1998:137) noted that "space structures the social interaction of people," and he suggested that "the ability of open space to define, confine, include, and exclude social interaction above the level of the household makes it one of the single most important structuring elements affecting the degree to which social units within a community are well-integrated." The emphasis here is on *integration*, a process that implies a desirable end, leading from the aggregation of diverse social (or other) migrating entities with the host community that result in mutually satisfactory residential and social organizational arrangements. The arrival and acceptance of migrant "others," however, requires a preliminary consideration: negotiating a place (space) to accommodate the newcomers. While migrant nuclear or extended family units might be offered abandoned living quarters within the community or provided space within the existing configuration, larger parties might not be so easily accommodated within the host community's existing architectural structure. In some cases, new rooms and room blocks might simply be appended to existing house blocks, or situated in such a way as to form new, or expanded plaza space—particularly, where the migrants and hosts are "related" in some socially compatible or recognized manner. Where such ties are absent, or tenuous, other residential arrangements and conditions might be negotiated as a precondition to the integration of the migrants into the community's spatial "fabric," as at Isleta.

George Kubler (1940:18) pointed out that

acceptance of the Franciscan missionaries and their architectural space requirements involved negotiating "the admission of the church to the indigenous layout," and he suggested that this was achieved by "mutual concessions, [and] determined on the spot." The negotiations between the Laguna migrants and Isleta authorities emphasize this point. Kubler reviewed several solutions that characterized such negotiations, noting in particular the peripheral "emplacement" of the mission complex at Pecos some distance outside the pueblo's architectural complex (also see the "lost church" of Pecos [Stubbs et al. 1957]). As conditions warranted, particularly following the return of the Spaniards, the mission complex might be situated immediately adjacent to, or incorporated within, the pueblo's structural configuration. Review of several historic Pueblo migrations discussed above indicates that, in some cases, their "footprints" might be visible as discrete, perhaps temporary, living quarters situated outside the host community, much like the Franciscan's structure at Pecos. Such negotiated loci for accommodating others—what I refer to here as "co-residential" units—are also visible, I believe, in the archaeological record of the prehistoric Pueblo Southwest (also see Clark 2001).

Variability among the Hopi communities is the result of migration and the integration of disparate peoples, an amalgam of diverse clans, with their unique heritage; and the reputed Keresan (and other) origins of some Hopi clans and villages, seemingly attest to earlier migrations. Bernardini's (2005) model of "staging areas," from which migrants approached and, ultimately, were accepted into the Hopi milieu is an appropriate one, and the arrival of the Kokop clan at Hopi is, perhaps, a storied case in point. Small prehistoric sites in the vicinity of the large Pueblo IV Hopi Mesa villages are believed to represent ethnographically documented "colony," or "daughter" villages adjacent to "mother" pueblos (Adams et al. 2004). Alternatively, it is equally possible that, like the Kokop and later Tewa migrants to Hopi, those smaller sites reflect the negotiated settlement loci of migrants seeking residence among the Hopi Mesa communities. Contemporary with the larger sites on and below the mesas, most of those smaller villages, based on ceramics, were abandoned between about 1400 and 1500, suggesting the possibility that their inhabitants ultimately moved into the principal historic Hopi villages.

Investigations at late prehistoric migrant sites in the Safford Valley of Southeastern Arizona, suggested that the size of migrating groups influenced the choice of where to construct their new residences (Neutzil 2008). Where household-level migrants have kin, clan, or other ties to their host community, they might be provided space within the existing configuration of the village. Neutzil suggested that larger migrant groups might not simply move lock, stock, and barrel into an established community—we might particularly expect this when migrants and hosts were of different linguistic, religious, or socio-cultural persuasions, as at First Mesa, Isleta and, initially, at Sandia (also see comment on the proximity of the Price Site to Pueblo Santiago [Turnbow 2011:203]).

The use of historic documents to identify the situating of migrants vis-à-vis their host community is one thing. Efforts to identify their "situating" from prehistoric archaeological remains, however, is quite another. Establishing contemporaneity between "outlier" and suspected "host community" clearly is a necessary first step in evaluating the presence of migrants. Ceramic assemblages suggested by the examples that follow might be indicative of the emplacement of migrants.

The presence of non-local ("imported") ceramic assemblages in outlying units, contemporary with those in the adjacent "nuclear" community, might be evidence for the presence of migrant "others" and their material belongings. Thus, examples of such "outliers," reflecting what I believe were discrete residential quarters for migrants adjacent to existing communities, might be indicated by the differential distribution and frequencies of ceramics between "outlier" and the adjacent larger community (Figure 5). Figure 6 illustrates occupation loci tested at the 3-Up Site on Mule Creek in southwestern New

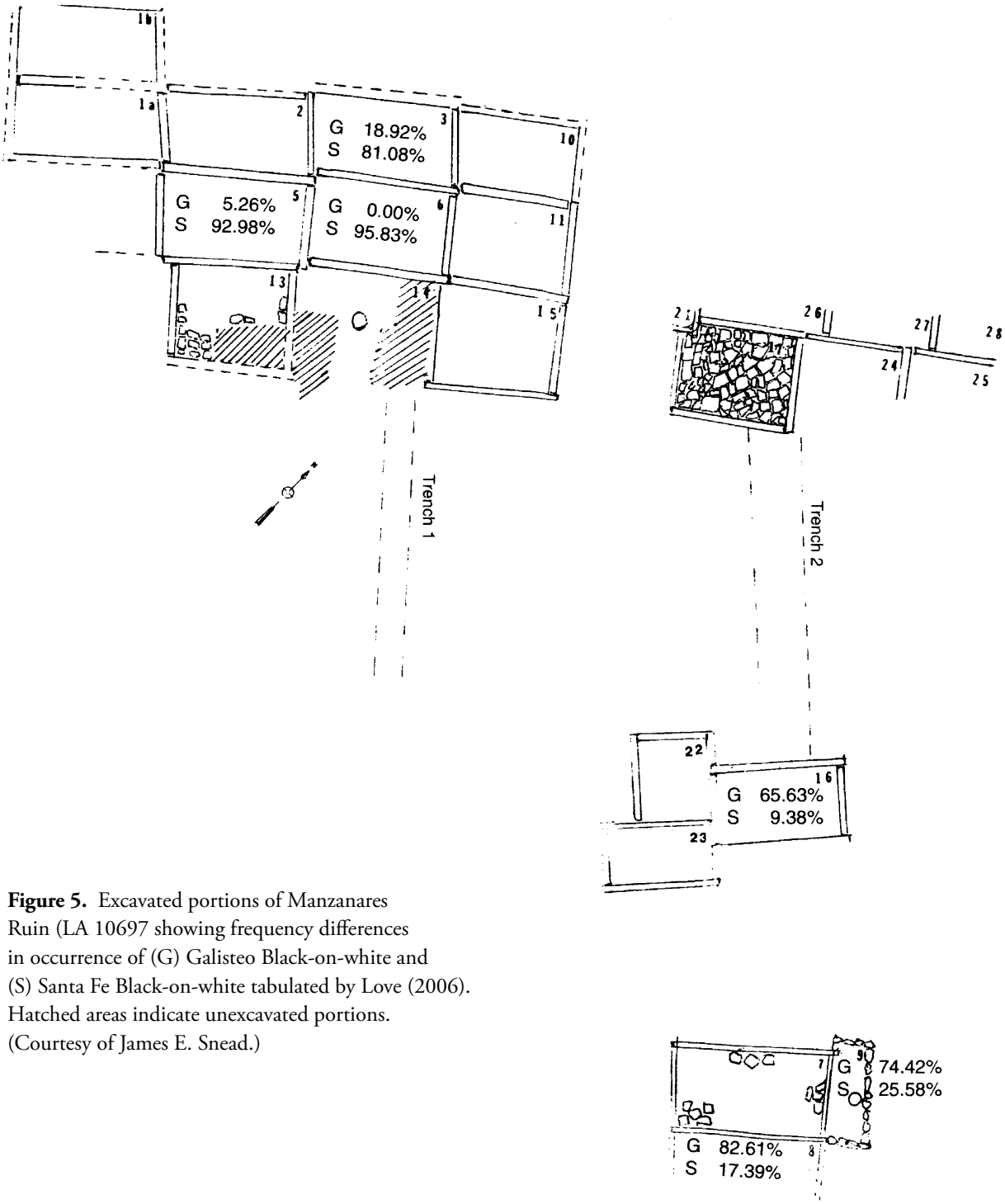

Figure 5. Excavated portions of Manzanares Ruin (LA 10697 showing frequency differences in occurrence of (G) Galisteo Black-on-white and (S) Santa Fe Black-on-white tabulated by Love (2006). Hatched areas indicate unexcavated portions. (Courtesy of James E. Snead.)

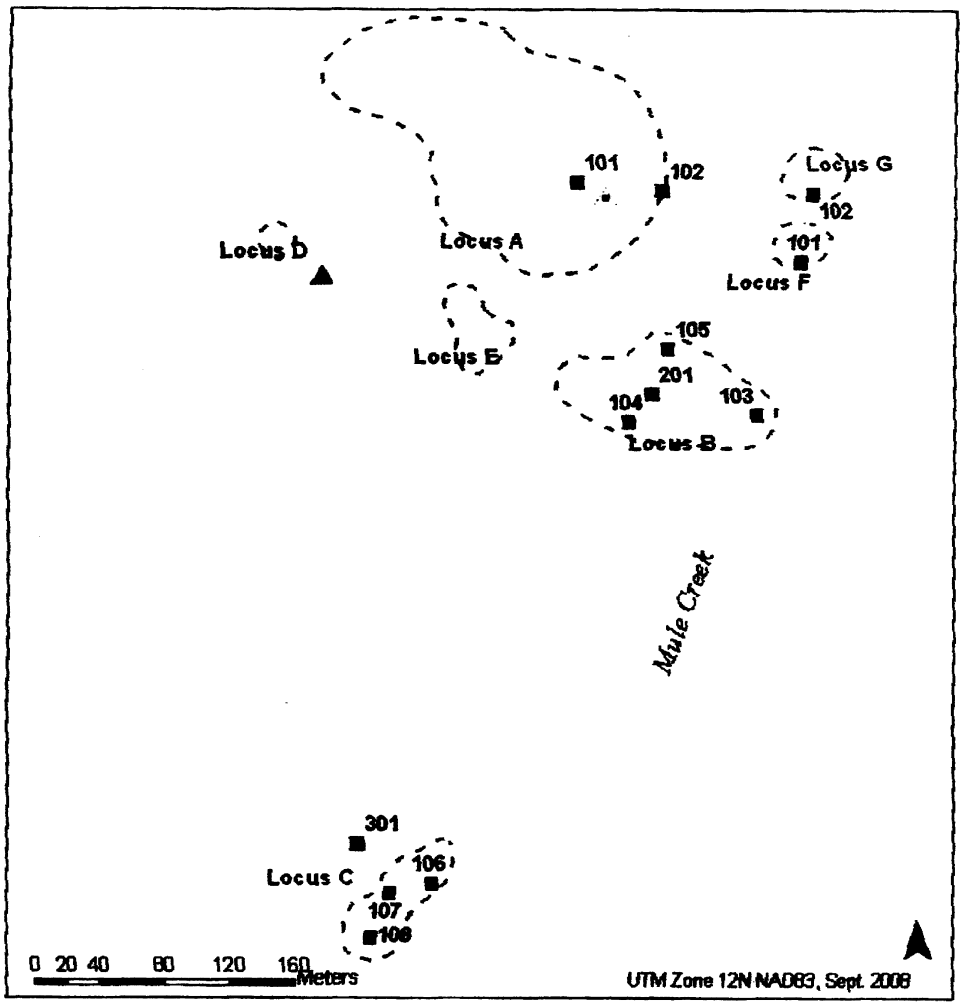

Figure 6. Mule Creek Archaeological Testing, Mule Creek, New Mexico (LA 150373) with the main complex (Locus A), and surrounding "outlier" units (after Dungan [2009]; courtesy of Deborah Huntley).

Mexico (Jones et al. 2009). Although not provided in the preliminary ceramic analysis, ceramic percentages from among the various loci are presented in color-shaded pie-charts, and reveal quite significant differences in proportions of Maverick Mountain series ceramics (about 75 percent for all Locus A Units), and Salado polychromes and Roosevelt redwares (less than about 15 percent at Locus A Units; about 90 percent from Locus C, and about 65 percent Salado and Roosevelt series from Locus F Unit). Locus B Unit yielded comparable numbers of Maverick Mountain and Salado series types (about 50 percent of the total), and roughly 25 percent unidentified Cibola White Wares, which occur in trace amounts at the other loci (Dungan 2009:16-23). The outlying units around the main mound at Tijeras Pueblo (Judge 1974, Figure 1) yield the same ceramic types (not necessarily in the same frequencies) as those recorded from excavations in the main mound (e.g., Sundt and Bice 1989).

These ceramic data from several sites and associated, presumably, "co-residential outlier" units are, as yet, based on very preliminary archaeological investigations, but, for the most part, archaeologists have tended to ignore these small "outlying" residential units often present adjacent to the larger pueblo community ruins. I suspect that such outliers, in many cases, do represent temporarily negotiated occupations by small migrant groups prior to their integration (or not) with the larger adjacent community. If this is a reasonable assumption, we might, in fact, be looking at "site unit intrusions" at a finer-grained scale than originally proposed by Willey and Lathrap (1956), analogous to those identified from the historical records. ☙

ENDNOTES

1. Revised and expanded version of a presentation at the 2010 Annual Meeting of the Archaeological Society of New Mexico, Santa Fe, entitled: "Migration, Residence Patterns, and the Persistence of Identity in the Puebloan Southwest."

2. Fray Morfí, writing in 1782 (Thomas 1932:159-60) noted that friars had brought down some families from Oraibi in 1742, for reason of "discord over the election of a chief in Oraibi," resulting in two sides taking "arms against one another." The least powerful, he said "… (which was composed of said families and others....) being hated because of its temerity or fearful of other encounters took refuge on the little mesa on which the Tiguas were already, *these also previously indisposed with the rest of that province*" (my emphasis). Presumably, the presence of Payupki people (as Sandia Pueblo is called today by Hopis [Hodge 1910:218, 429]) on Second Mesa is referred to, and their presence, seemingly, was not universally well "disposed" by some Hopis. The place at Isleta called "Oraibi" might recall Tiwas among the Oraibi immigrants brought down in 1742 to the Rio Grande (Hackett 1937:390-91).

3. The interrogation of Jerónimo, in 1682, native of Puaray Pueblo, revealed to the Spaniards that "the Tiguas of Alameda, Puaray, and Sandia, because their pueblos have been burned, have discussed going to settle at Sima *and* in the jurisdiction of La Canada...." (Hackett and Shelby 1942:361; my emphasis). Brandt (1979:345) mistakenly cited this as "Sima *in* the jurisdiction of La Canada" (my emphasis). Thus, "Sima" lay elsewhere, possibly the Hopi Mesa region, where Tiwas from Puaray, Alameda, Sandia, and Isleta took refuge against the return of the Spaniards following the Pueblo Revolt. A 1909 manuscript by J. P. Harrington refers to a Sima dialect of Sandia, "words of which were collected at Ysleta del Sur," but no other information was available (Brandt 1979:345). Lujan's account of the 1582 Espejo expedition to New Mexico (Hammond and Rey 1966:153-212) named 13 pueblos of the province where the friars had been martyred previously (the "province of Puaray"). In addition to "Guagua" (almost certainly, Kuaua), is "Simassa" (Hammond and Rey 1966:203), written in Oñate's 1598 list as "Tziymatzi," a Tiwa pueblo (Hammond and Rey 1953:346), next to "Paurai" in that list. I surmise that the people of "Simassa" either were reduced early in the seventeenth century, or fled to parts unknown—perhaps to the Hopi Mesas.

A section of Cochiti Pueblo identified as the "Butterfly group" (butterfly, *bekarana ~ bu·raika* in Keresan [Davis 1964:165; Father Hillaire, personal communication, 2010], lies south of the plaza and church. Informants gave no particular significance to the name (Lange 1952:End Map 2), but possibly reflects descendants of former migrants. A sketch map in Leslie White's Santo Domingo ethnography (1935:180; reproduced in Armstrong et al. 2011:38) depicts a series of numbered villages said to have been destroyed by Plains enemies in the past. Number 9 on the map is labeled Peña Blanca, and identified as "Be'rakana." Another migrant group, cited by Sandia elders, is a 10-room ruin with Glaze E pottery some 2 mi north of the pueblo, said to have been settled by migrants from "Abo" [!] (Marshall 2008:74).

4. Perhaps hyperbole on the part of Governor Diego de Peñalosa in 1665, he claimed that "he reduced to peace two heathen nations, the Cruzados and the Coninas, and made them settle by assembling them in two large pueblos in the province of Moqui" (Hackett 1937:264). If true, where were they, and how might archaeologists recognize them?

5. La Cieneguilla, the 1693-96 refugee pueblo on Horn Mesa (Potrero Viejo), does not refer to its situation in a "little marshy area," but rather, almost certainly was so called by the Spaniards as a result of the establishment thereon by the inhabitants of the abandoned pueblo of La Cienega. This lay some 15 mi below Santa Fe on a tributary to the river of that name, and was a *visita* of San Marcos mission (e.g., Snow 2008)

6. That some of the San Marcos people remained with those who returned to re-establish Santo Domingo is evident from their efforts to gain possession of the Cerrillos Hills (e.g., Schroeder 1975) and the fact that their descendants recall their ancestral heritage (personal communication to the writer 2008; name withheld on request).

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Inferences About Prehistoric Subsistence in the Pecos Country and the Sierra Blanca Region of Southeastern New Mexico

REGGE N. WISEMAN

It is not often that archaeologists have access to large numbers of metates for the purposes of study and comparison. Through the years the author has taken the opportunity to measure and describe five large collections of complete metates from four counties (Chaves, Eddy, Lincoln, Otero) in south-central and southeastern New Mexico (Figure 1). The importance of a data base of this size is that we can now compare certain aspects of grinding equipment across this large region.

At the present time, the Late Prehistoric societies of Lincoln and Otero counties are demonstrated to have engaged in farming to as yet an unspecified degree. Suffice it to say, corn remains and sometimes other cultigens are fairly commonly recovered from village sites in this, the Sierra Blanca region as defined by Jane Kelley (1984).

On the other hand, the prehistoric inhabitants of Chaves (excepting the Roswell oasis) and Eddy counties are only *presumed* to have been farmers on the basis of the presence of pottery and rare instances of structures. More recent studies, however, suggest that most of the groups in Eddy and Chaves counties were hunter-gatherers, *not* farmers.

Thus, we would expect to find differences in the metates used by each group. Because farmers were growing corn and consuming more grain than hunter-gatherers, they are believed to have ground more plant foodstuffs on their metates. In general, the farmers' metates should have larger grinding surfaces for greater efficiency in grinding larger amounts of corn in a given amount of time. Conversely, hunter-gatherer metates are generally known to have had fairly small grinding surfaces reflective of the fact that they evidently used smaller amounts of seeds at a given meal. Are size differences reflected in the grinding surfaces of the metates from the study collections?

It is also pertinent to note that farmers elsewhere in the Southwest developed specialized forms of metates, and in some cases, increased grinding performance by stabilizing them in permanent facilities called mealing or grinding bins. Did the farmers in Lincoln and Otero counties use more specialized forms of metates, and did they place them in mealing bins? These questions pertain not only to the comparisons to be made with the metates from Chaves and Eddy counties but also pertain to comparisons that can be made between the Sierra Blanca region and other areas of the Southwest.

As mentioned earlier, several forms of metates are known for the Southwestern field, and Southwestern archaeologists have devised names for each of these types. The same is true for archaeologists working elsewhere in the United States. The trouble is, the same word is sometimes used in adjacent regions but in different ways. Accordingly, archaeologists working in southeastern New Mexico have to be aware of the differences in terms used by Southwesternists versus Texas and Southern Plains archaeologists. The reader should note that this paper follows Southwestern usages and, in particular, those applied to Ancestral Puebloan ("Anasazi") and Historic Puebloan cultures of the Four Corners region. There, three general types

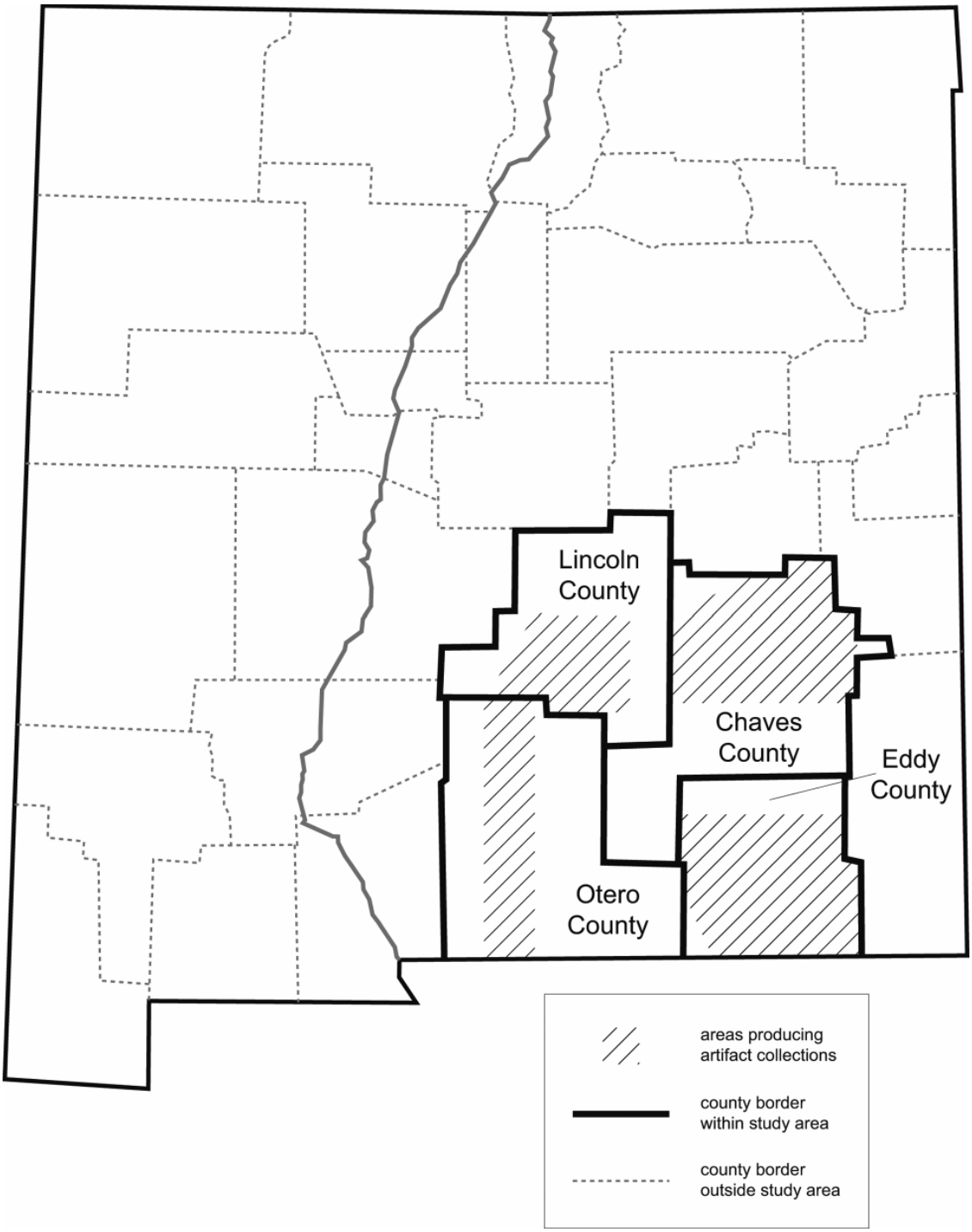

Figure 1. New Mexico counties that produced the five study collections of metates. Metates are mainly from sites in the hachured areas in each county.

of metates are recognized—basin, trough, and slab (or "flat"). Generally speaking for the Southwest, these metate types were developed and used sequentially through time.

Basin metates, being the smallest in grinding surface area, are believed to have been used for grinding small amounts of materials, whether they were foodstuffs, medicines, paints, salt, or other materials. They were most common in the early periods but became less common as the reliance on corn increased through time. Small numbers of basin metates were used well into the Historic period, but usually for grinding materials other than corn (Wiseman 1969).

As corn became more important in the diet, trough metates were developed to process the larger quantities needed on a periodic (daily?) basis. Trough metates could be taken from storage and propped at an angle using stones to create a temporary work station. Or, they could be installed in permanent mealing bins. Adams (1993) discusses the physical and energy benefits involved in grinding and how these change with the type of metate.

The final step in the commitment to corn was the development of slab or flat metates that provided maximum grinding surface confined in the most stable of positions, the mealing bin. Slab/flat metates could be used only when mounted in mealing bins because the stones lacked raised areas to keep the meal gathered for grinding; the sides of the mealing bin served this function. The regular appearance of mealing bins signals the ever increasing commitment to corn in terms of time, dedicated work space, and quantities ground. Before the arrival of the Spanish in the mid A.D. 1500s, some Native American groups used several types of stone to make slab metates. Some stones are more grainy or coarsely textured and others are less grainy. Placed in adjoining compartments of a multi-compartment bin, each successive grade of stone ground the meal into finer and finer particles. By the time multiple-compartment grinding bins appeared, corn was clearly a major foodstuff that was ground and consumed on a daily basis.

TYPES OF METATES

Basin metates have oval grinding surfaces that curve gently and evenly in all directions from the rim of the stone. Basin metates occur on stone slabs, chunky rocks, boulders, and exposures of bedrock. The working position of most, if not all, basin metates was probably flat on the ground in a horizontal position. It is important to note that, to Texas and Plains archaeologists, a slab metate is *not* the shaped artifact just described. To them, a slab metate is a relatively large slab of rock bearing a small grinding surface on one face. (Southwestern archaeologists commonly know this type of metate as an Archaic period metate.)

Trough metates have grinding surfaces that are long compared to width. Across the width, the curvature of the grinding surface is slightly concave until it reaches the vertical side walls. The side walls, which deepen with use, serve to confine the grinding to a forward/backward motion and to keep the material being ground from spilling laterally. The ends of the grinding surface of the trough metate can be of one of two types: "one-end-open" and "through." In the one-end-open type, one end curves up fairly abruptly to the rim of the stone (similar to the sides of the trough) and the lower end is left open to allow the meal to be moved out of the metate and into a receptacle (often a pottery bowl) placed there for the purpose. The "through" trough metate has both ends "open."

On the slab or flat metate, the grinding surface covers the entire top of the stone. Quite often, the sides, ends, and even bottom of the stone have also been shaped to remove excess rock and facilitate installation in grinding or mealing bins. A mealing bin consists of one or more contiguous rectangular compartments formed by vertical rock slabs. In each compartment, a metate is propped up at an approximate 45 degree angle and plastered into place with copious amounts of mud. Compartments per bin range from one to eight. Archaeologically, two to four compartments per bin appear to be the most common. Ethnographic accounts suggest that in historic times three is the more usual number.

SUMMARY OF THE PREHISTORY OF SOUTH-CENTRAL AND SOUTHEASTERN NEW MEXICO

In 1948 Don Lehmer (1948) published his seminal definition of the Jornada branch of the Mogollon culture. He defined the territory of the Jornada Mogollon as a diamond-shaped region that stretches from Carrizozo, New Mexico on the north to below Villa Ahumada, State of Chihuahua, Mexico on the south. The east-west dimension extends from west of Deming, New Mexico to Carlsbad, New Mexico, placing El Paso, Texas, at the center of the region. Then, in 1965, John Corley of the Lea County Archaeological Society (LCAS) in Hobbs proposed extension of the Jornada Mogollon concept to include the archaeological remains lying east of the Pecos River in southeastern New Mexico. This proposal seems to have become universally accepted and is known as the Eastern Extension of the Jornada Mogollon. Since then, archaeologists have simply "connected the dots" and included the territory between these two regions within the concept. Given the dearth of actual archaeological data at the time, this idea seemed sensible.

And thus, for the past 60+ years, archaeologists have accepted this vast region as essentially constituting one culturally homogeneous region during prehistoric times. Over the years, however, data have accumulated to the point that it has become necessary to reevaluate this thesis and to reformulate the cultural relationships in this part of the state (Wiseman, in prep.). These findings are summarized here in order to provide a background for the discussion of the metates.

Lehmer (1948) proposed two sequences of phases for the Jornada Mogollon: 1) the Mesilla, Doña Ana, and El Paso phases for the lowland basins around El Paso, Texas, and north to at least Alamogordo; 2) the Capitan, Three Rivers, and San Andres phases in the northern sequence for upland areas to the north and east. He initially believed that both sequences started about A.D. 900 and ended about 1400. These dates have since been adjusted to about A.D. 200 to 1450 by Miller and Kenmotsu (2004).

In the El Paso region, the early Jornada Mogollon people had a pithouse-dwelling, pottery-making, hunting-gathering culture with low-level corn farming. The first houses were brush huts or wickiup-like structures with shallowly excavated floors. These were later replaced with larger structures that began to take on characteristics of true pithouses in terms of interior floor features and somewhat deeper excavations of the floors. Corn was farmed, but it was only a minor part of the diet for several hundred years. About A.D. 1000 the people began building surface structures of multi-cellular pueblo form, but not until somewhat later (about A.D. 1250 or 1300) did they significantly increase their reliance on corn (Miller and Kenmotsu 2004). By this later date, some of the pueblos were as large as 100 or more rooms arranged as parallel blocks of rooms or as linear roomblocks arranged in squares around open spaces or plazas. The primary indigenous pottery at these late villages is El Paso Polychrome, but numerous imported pottery types are also found in small numbers. Descendant populations and their cultural remains have not been firmly identified. Lehmer, on the basis of pottery assemblages and the surface characteristics of a few sites, boldly assumed that the sites in the upland regions were essentially the same as those of the lowlands.

Corley's (1965; see Leslie 1979 for amplification) proposal for the Eastern Extension involves a sequence patterned on Lehmer's sequences but uses the terms Querecho, Maljamar, and Ochoa for the phase names. Also, like Lehmer, he suggests dates based on associated pottery types, many of which are the same as those on El Paso sites, but offsets them 50 years later to allow for "diffusion" from the Jornada Mogollon heartland. The LCAS data base for the Eastern Extension includes several excavated sites and observations and surface collections at dozens of other sites scattered between the Pecos River on the west, U.S. 380 (Roswell to Tatum highway) on the north, and the two Texas state lines on the east and south. The excavations at

the Merchant site near Hobbs, New Mexico, were the most productive in terms of useful data (Leslie 1965). This site, the most thoroughly investigated by the LCAS, produced Ochoa Indented Corrugated pottery, small amounts of Jornada Mogollon pottery types made west of the Pecos River, two deep pithouses (only one completely excavated), and several small, above-ground surface rooms (rectangular outlines or cimientos ["foundations"] of rocks forming individual and multi-room units). Merchant and a few other sites bearing cimiento surface structures were assigned to the Ochoa Phase, the latest prehistoric phase and the one thought to be equivalent in time to the El Paso Phase of the Jornada Mogollon proper.

Earlier Eastern Extension sites are characterized mainly by brown ware pottery, occasional Jornada types (all made west of the Pecos), and rare structural remains *presumed to be pithouses* but much more like wickiups. Corley assigned these sites to the Querecho and Maljamar phases and believed them to be cognates of the Mesilla and Doña Ana phases of the El Paso region. Dates for the Querecho and Maljamar are suggested on the basis of the associated imported pottery types, many of which are also found on Mesilla and Doña Ana sites. It is no coincidence, then, that the form of the Eastern Extension sequence (Querecho, Maljamar, and Ochoa phases), along with their suggested dates, greatly resemble the Mesilla/Doña Ana/El Paso sequence and its dates in the El Paso region. Although recovery techniques for micro-remains (for flotation and pollen analyses) have *not* been performed on sediments from any of this original group of investigated sites, no corn or other cultigens have yet been identified. Given the extensive excavations at the Merchant and a couple of other sites, this suggests that the people of the Eastern Extension subsisted on hunted and gathered wild products but not on corn.

It is clear that the archaeological culture in New Mexico east of the Pecos River is very unlike that of the El Paso region, especially the manifestations dating after about A.D. 1000, and should not be considered part of them in any proper archaeological sense.

The first important addition to the regional literature appeared in 1966 as Jane Kelley's dissertation at Harvard University, but it was not made readily available until publication in 1984. The sites described and analyzed in this work lie within the northeastern high country or Sierra Blanca region originally included within Lehmer's (1948) definition of the Jornada Mogollon. However, Kelley's results demonstrate conclusively that the post-A.D. 1000/1200 archaeological remains in the Sierra Blanca country, while similar in some respects to those of the greater El Paso region, are clearly *not* El Paso Phase in character and should not be regarded as such.

Kelley devised two taxonomic sequences to characterize her Sierra Blanca materials—the early and late periods of the Glencoe Phase for those remains situated on the eastern flanks of Sierra Blanca (northern Otero and southern Lincoln counties) and the Corona and Lincoln phases for those remains situated around the Capitan, Jicarilla, and eastern slopes of the Gallinas mountains in Lincoln county. Throughout the Glencoe Phase (ca. A.D. 500 to 1400?), the people lived in large pithouses, made quantities of plain Jornada Brown pottery, hunted/gathered wild plants and animals, and grew corn and beans. Glencoe people evidently were the earliest population that can be culturally identified in the Sierra Blanca country.

Corona Phase peoples lived in small pueblo-style structures that had depressed floors but that appeared on the ground surface as cimiento structures (similar to those described earlier). Later, during the succeeding Lincoln Phase, the people built larger structures of 10 to 100 rooms arranged in linear roomblocks and plaza pueblos situated on the then current grade. Corona Phase sites are characterized by plain Jornada Brown pottery that was replaced during the Lincoln Phase with the indented corrugated ware Corona Corrugated. Chupadero Black-on-white was the primary service ware used during both phases. During both phases, subsistence needs were met by hunting and gathering wild animals and plants and growing corn and beans. During the Lincoln Phase the people traded widely for pottery and other products. A number of characteristics of both the Corona and

Lincoln phases suggest important connections, and perhaps ultimate origins, in the Gran Quivira region of central New Mexico (southwestern Torrance and eastern Socorro counties). The Corona and Lincoln phases thrived between about A.D. 1000 and 1400 or later.

Another important addition to the literature is by Paul and Susana Katz (1985) concerning their work at Brantley Reservoir on the Pecos River, Carlsbad, and next to the eastern foothills of the Guadalupe Mountains. They analyze and interpret their Brantley data in light of the archaeology of the Guadalupes as well as the Pecos Valley. In so doing, they provide a broader, better founded set of interpretations for the Carlsbad region as a whole. The sites at Brantley include a variety of burned rock and artifact locations, including two general types of burned rock middens—mounded and scattered. The only type of prehistoric habitations found on the project are stone enclosures (Katz and Katz 2002), the first such reported for this part of New Mexico (see Wiseman 2002a). Importantly, the Katzes did *not* find pithouses, pueblos, or even the floors of brush structures, though brush structures were later found nearby by Zamora (2000). The small amounts of pottery recovered from some of the Brantley and Guadalupe Mountain sites were clearly made (again on the basis of tempering materials) in the Sierra Blanca and El Paso regions and traded into the Carlsbad region. Subsistence pursuits in the Brantley locale involved hunting and gathering of wild terrestrial and riverine species. No unambiguous evidence of farming or gardening of corn or other domesticates has yet been found. The culture of the Brantley/Guadalupe Mountains region is clearly much more like that of the Trans-Pecos region to the south in Texas and definitely is unlike that of the Jornada Mogollon described by Lehmer (1948).

Several of the most recent studies conducted in southeastern New Mexico have taken place within a 15 km radius of the city of Roswell. Previously, Jane Kelley (1984:Appendix 6) reported on the prehistoric adobe pueblo-style village of Bloom Mound. Excavations at this site by the Roswell Archaeological Society recovered hundreds of stone and shell discoidal beads, seven copper bells, large amounts of El Paso Polychrome pottery, and a number of other unique items that really made this small pueblo stand out. This is especially so because of its small size and its location on the southeastern edge of the Southwestern pueblo world. Kelley postulated that the site was a fourteenth century trading post between the El Paso region and Pecos Pueblo (and by extension, the Southern Plains).

Subsequent work at other Roswell sites such as Henderson (Speth 2004), Rocky Arroyo (Wiseman 1985), and the Fox Place (Wiseman 2002b) have resulted in the identification of yet other unique characteristics of the Roswell locality. Because of the nature of the architectural, ceramic, artifactual, and subsistence remains recovered at these and a few other sites in the immediate area, it is clear that these sites represent both unique and familiar cultures. Because of the hydrologic characteristics of the area and the resulting floral and faunal communities, I believe the area immediately around and including Roswell can be characterized during prehistoric times as an oasis in the classic sense of the term.[1]

At least three different prehistoric peoples seem to be represented at sites within the Roswell oasis. The first were the people who probably had inhabited the region for a number of centuries or even millennia (see Rocek and Speth 1986) prior to the arrival of the other two. The inhabitants of the Fox Place and the Henderson site appear to represent late period examples of this original group. The Fox Place was a village of very small pithouses bearing all the characteristics of hunter-gatherers; they ate corn, but whether or not they grew that corn or obtained it from nearby Glencoe(?) farmers has not been established. A large socio-religious structure, bearing Glencoe Phase characteristics, was found among the pithouses, and is interpreted as evidence of religious proselytizing by Glencoe Phase peoples (Wiseman 2002b).

The Henderson site, which may have been built, at least in part, by former occupants of the Fox Place, is a large, poorly constructed pueblo-style building of 75 to 100 rooms. Speth (2004) has defined two periods

of occupation of the site, both of which included corn farming. During the later period, subsistence practices became more Plains-like in character with an emphasis on hunting bison, communal consumption practices, and the like. Also during the later period, exchange relations with surrounding regions intensified. A broader range of Southwestern pottery types made their way into the site, and the village social organization shifted toward an integrated, community-wide configuration (Speth 2004).

The second and third groups immigrated into the Roswell oasis from the Sierra Blanca country to the west. Although their arrival times have not been established except in the most general terms, these groups included Glencoe peoples who inhabited Rocky Arroyo and perhaps other nearby sites, and Lincoln peoples, who apparently built only one site, Bloom Mound.

The social atmosphere created among these three groups apparently varied. The Glencoe people and the indigenous people appear to have gotten along with one another, but the occupants of Bloom Mound experienced violence on more than one occasion. Burned, unburied skeletons and individuals who appear to have been killed and then formally buried (Kelley 1984; Newlander and Speth 2009) have been recovered from Bloom. Newlander and Speth discuss the possibility that the attackers of Bloom Mound were prehistoric Central Texas peoples known as Toyahs. However, other possibilities need to be explored as well, for other examples of violence are also documented in the Sierra Blanca region (Wiseman 1997).

In broad outline, then, the archaeological remains of the four study counties in this paper can be divided into pairs, with those of Eddy and Chaves counties (except for the Roswell oasis) being characterized as hunter-gatherers and those of Lincoln and Otero counties being farmers.

Given the above view of Late Prehistoric cultures in southeastern New Mexico, what are our expectations regarding metates used by hunter-gatherers versus those of farmers? Current thinking suggests the following. Hunter-gatherer metates, especially the grinding surfaces, should generally be smaller because they were used primarily for grinding relatively small quantities of wild plant foods at any one time. On the other hand, the metates of farmers should be larger because they were used more frequently *and* were used to grind larger quantities of plant foodstuffs (especially corn) at a given time. These expectations are in concert with previous studies that have shown a general correlation of increasing mano and mano grinding surface size as being indicative of increasing reliance on corn consumption (Hard et al. 1996). Also, relative dependence on corn is also believed to be reflected in the type of metate used and whether or not it was mounted in permanent mealing bins.

METATES OF THE STUDY COLLECTIONS

Large collections of complete and nearly complete metates from southeastern New Mexico are rare. As is often the case, the collections exist because certain artifact collectors were active over long periods of time in specific areas. The collections considered here include items taken mostly from surface and some excavation contexts at sites in four counties of southeastern New Mexico. The collections reported here represent:

(1) *Eddy County,* the Carlsbad region; the sandhills east of the Pecos River and the Guadalupe Mountains west of the river—47 metates bearing 53 grinding surfaces; collection prior to 1990 by several people, including Quentin Rodgers and Harvey Hicks.

(2) *Chaves County*, the northern half (north, east, and west of Roswell)—37 metates bearing 44 grinding surfaces; collected by Jack A. Ross between about 1950 and 1980.

(3) *Chaves County*. Bedrock basin metates are less common than their portable counterparts. The one example of a bedrock basin metate site included in this analysis is the Los Molinos site (LA 68182) located

on the northern outskirts of the city of Roswell. The 57 basin metates at Los Molinos were documented for the New Mexico Department of Transportation (Wiseman 2004).

(4) *Lincoln County*, the southern half (i.e., centered on the communities of Lincoln, Capitan, White Oaks, and Encinoso)—90 metates bearing 92 grinding surfaces; collected by Clark C. "Duke" Pfingsten between 1920 and 1990.

(5) *Otero County*, the western half (from Three Rivers on the north to the New Mexico/Texas state line on the south)—29 metates (29 grinding surfaces); collected by Virginia Wunder prior to 1972.

The combined study collection for this paper, then, involves a total of 260 metates. Fifteen have two grinding surfaces each, raising the total number of analyzable grinding surfaces to 275. Because a few of the metates are not quite complete and could not be analyzed for all attributes (such as the dimension of length), the numbers presented in the tables and the text do not always add up to the collection totals just presented. The Eddy County metates are held by the Carlsbad City Museum. The Los Molinos bedrock metates are now beneath U.S. 70 at the overpass on U.S. 285 north of Roswell. All other collections are still in private hands.

Records of exact proveniences were not kept by the collectors, so we have no specific temporal or spatial control. The metates from Eddy and Chaves counties were retrieved mainly from surface contexts. Those from Lincoln and Otero counties came from both surface and excavated contexts. For the Lincoln County metates, most of the excavated contexts involved pottery period sites, especially Glencoe and Lincoln phase contexts. However, judging by certain characteristics, eleven of the Lincoln County specimens and three from Otero County appear to represent the Archaic period.

As will be seen, these facts do not appear to have particularly serious consequences. The variability within each collection and among the collections is surprisingly small, suggesting little change in metate characteristics and uses over the time periods represented. It is probably safe to assume that, for most or even all of the collections, that time period probably extends from at least the Late Archaic to the Late Prehistoric, or the period from about the beginning of the Christian Era to about A.D. 1400 or 1450. Evenness of representation of the various sub-periods of time is impossible to judge at this time.

Because of space limitations, this paper is confined to characterization of the basin metates, the most common type of metate in all five collections.

METATE TYPES AND MATERIALS

Overall, the vast majority (95.8 percent) of the metates in the study collections are of the basin type (Table 1). Although grinding basins display considerable range in sizes and depths, these metates conform well to the definitions for the type. It is important to note that only five portable metates are of the true trough type and six others are of the true slab or flat type. It is not surprising that most of the slab metates known for the Sierra Blanca country were recovered from late contexts north of the Capitan Mountains (Kelley 1984:90), suggesting that they were probably introductions from more northerly areas. As discussed earlier, trough and slab/flat metates imply greater commitment to the use of corn than do basin metates, but the small numbers of metates of both types together suggest that a higher commitment to corn in Lincoln County was not widespread. In support of this conclusion, only one *possible* mealing bin has been documented in the region, and that is at LA 2945 in Gallo Canyon (Wendorf 1956) at the far northern limit of the Sierra Blanca region as defined by Kelley. The site is also close to the Gran Quivira region where excellent examples of these facilities have been found (Hayes et al. 1981).

The materials of which the metates are made reflect the geological environment of each county (Table 2). Overall, sedimentary, igneous, and metamorphic

Table 1. Summary Dimensions of Complete Metates by Type and size (in Centimeters).

		Entire Metate			Grinding Surface		
Type	Number	Length	Width	Thickness	Length	Width	Depth
Eddy County							
Basin	53	30-59	15-42	5-11	19-51	10-26	.25-4.5
Mean		39	23	4.7	31	16	1.8
Trough	0						
Slab	0						
Chaves County							
Basin	45	21-56	16-37	2.0-11.5	15-40	10-23	.25-2.75
Mean		30	20	4.4	25	16	1.1
Trough	1	58	37	4.5	39	24	2
Slab	0						
Lincoln County							
Basin	92	33-62	22-51	4-25	18-48	13-40	.5-11
Mean		49	36	14	37	28	5.6
Trough	2	56-60	44-51	15-20	39	35	6.5-9.5
Slab	1	40	27	14	36	26	.1
Otero County							
Basin	29	33-57	21-47	5-15	19-44	2-34	1-9
Mean		45	33	11	34	24	3
Trough	2	39	30	10	38	23	3
Slab	5	40-44	19-30	8-13	30-39	19-23	1-2
Bedrock Basin Metates at the Los Molinos Site (LA 68182), Roswell, Chaves County							
Basin	57	—	—	—	12-30	9-21	1-10
Mean					21	14	3.5

Table 2. Rock Types of Metates in the Study Collection.

	Eddy County	Chaves County*	Lincoln County	Otero County
Sedimentary Rocks				
Siltstone	6% (3)**	6% (3)		
Sandstone	92% (14)	56% (8)	7% (4)	52% (7)
Limestone/Dolomite	2% (1?)	6% (1?)	1% (1?)	
Other (conglomerate, mudstone, caliche)		14% (5)		
Igneous Rocks		12% (2)	92% (4)	45% (6)
Metamorphic Rocks				
Mica Schist				3% (1)
Other Rocks		6% (3)		
Totals	100% (18?)	100% (22?)	100% (9?)	100% (14)

*Collection of portable metates only. The Los Molinos bedrock basin metates are in outcrops of limestone/dolomite (62%) and conglomerate (38%).

**Figures in parentheses indicate numbers of each rock type. Since no tests were made to determine the actual number of limestone/dolomite varieties, this is noted by a ? in parentheses.

rocks are represented among the five collections, with metamorphic rocks being the most poorly represented of all. Sedimentary rocks are the most common among the Eddy and Chaves collections. Igneous rocks are the most common within the Lincoln collection. And, both sedimentary and igneous rocks are about equally represented in the Otero collection.

METATE SIZES

The overall size range of complete basin metates in the five study collections is rather large (21 to 62 cm long), but the ranges are more restricted within each collection (Table 1). However, the *mean dimensions* of basin metates from the (mainly) farming sites in Lincoln and Otero counties are longer (45 to 49 cm), wider (33 to 36 cm), and thicker (11 to 14 cm) than the hunter-gatherer metates from Eddy and Chaves counties (30 to 39, 20 to 23, and 4.4 to 4.7 cm, respectively).

The greater length and width measurements on farmer metates are due in part to the fact that the farmers tolerated more non-functional or excess stone on their tools (i.e., space not used for grinding surface) than did the hunter-gatherers. In contrast, virtually all of the hunter-gatherers chipped and ground excess stone from the edges of their metates, resulting in stones that are essentially lighter in weight, and more portable ("travel metates") (Figure 2). The explanation for these differences is probably straightforward. The farming villagers, once they had carried the stones for their metates to the villages from the collection/quarry locations, probably moved the finished artifacts only short distances after that (from storage positions to use locations and back).

In contrast, hunter-gatherers, especially those living east of the Pecos River, had several problems with which to contend. First, the most suitable sources of stone apparently lay west of the Pecos. After selecting the stone to be used for making a metate, a person had to carry that stone to the camp east of the river. Then, if the metate was not to be left in a specific camp, but was to be moved from camp to camp, it

Figure 2. Examples of "travel metates" from Chaves County, New Mexico.

might be carried to sequential camps every few days or weeks. Under these conditions, the lighter the metate, the easier the carry.

Thus, it is not surprising that the farmers' metates (for the most part) from Lincoln and Otero counties range in thickness from 4 to 25 cm, with means of 14 and 11 cm respectively. And, the hunter-gatherer metates from Eddy and Chaves counties range in thickness from 2.5 to 7.5 cm (mean of 4.7 cm) and 2 to 11.5 cm (mean of 4.9 cm) respectively.

GRINDING BASINS

A number of studies have shown a general correspondence between *mano* length and grinding surface area with degree of dependence on corn (Hard et al. 1996; Diehl 1996). And, as mentioned earlier, archaeologists have noted for decades that there appears to be a corresponding correlation between metate type and mealing facility development with increasing reliance on corn. It therefore seems logical that the sizes of grinding surfaces of metates, like those of manos, should also increase through time and at least partly reflect increased reliance on corn.

For comparison among the five metate collections, the length and width dimensions of the grinding surfaces of each metate are plotted by county/collection in Figures 3-7. An inspection of these plots reveals several interesting things.

For the Eddy County metates (Figure 3), the lengths of the grinding surfaces are highly variable and not tightly clustered. If we disregard the five longest metates, the lengths of the grinding basins range from 19 to 40 cm. Basin widths are much more restricted, being 10 to 21 cm with one outlier at 26 cm. The trend line is very nearly vertical, indicating that the width dimension may be controlled by a static *mano length*.

For the Chaves County *portable* metates (Figure 4), the lengths of the grinding basins range from 15 to 34 cm (with 4 longer outliers) and average slightly shorter than the Eddy County grinding surfaces. The width range of 15 to 21 cm (3 outliers) is very similar to but slightly wider on average than the Eddy metates. And, the trend line is more diagonal than that for the Carlsbad collection, indicating that the mano lengths and/or the grinding strokes are slightly more variable than the Eddy County metates.

For the Lincoln County metates (Figure 5), grinding basin length is longer than those for both Eddy and Chaves metates; the range of the main cluster of points is from 25 to 45 cm with both shorter and longer outliers. Grinding surface widths are much more variable than either Eddy or Chaves metates and range from 17 to 38 cm. Overall, the Lincoln grinding basin dimensions cluster rather tightly and form a fairly distinct trend line that is more diagonal (variable) than the Eddy and the Chaves trend lines.

The Otero County (Figure 6) dot swarm is broad and not tightly clustered, in part probably because of the much smaller sample size. The grinding basin length range of 24 to 40 cm is slightly shorter than that of the Lincoln metates but still averages longer than both the Eddy and the Chaves metates. The width range of 18 to 29 cm is both narrower overall than the Lincoln metates but still wider than both the Eddy and Chaves grinding surfaces.

The grinding surfaces of the Los Molinos bedrock metates near Roswell in Chaves County (Figure 7) form the tightest dot cluster and the most distinct trend line of all collections. The grinding basin length range is 12 to 32 cm, and the width range (with no extreme outliers) is 9 to 21 cm. Not surprisingly, the Los Molinos grinding surfaces are most similar in all respects to the main cluster of the Chaves County portable metates.

SUMMARY AND CONCLUSIONS

The collections that form the basis of this study came from four separate counties in south-central and southeastern New Mexico—Eddy, Chaves, Lincoln, and Otero. Four of the collections consist of portable metates, and one involves a site of bedrock grinding surfaces at Roswell in Chaves County. The total study collection involves 260 metates and 275 grinding

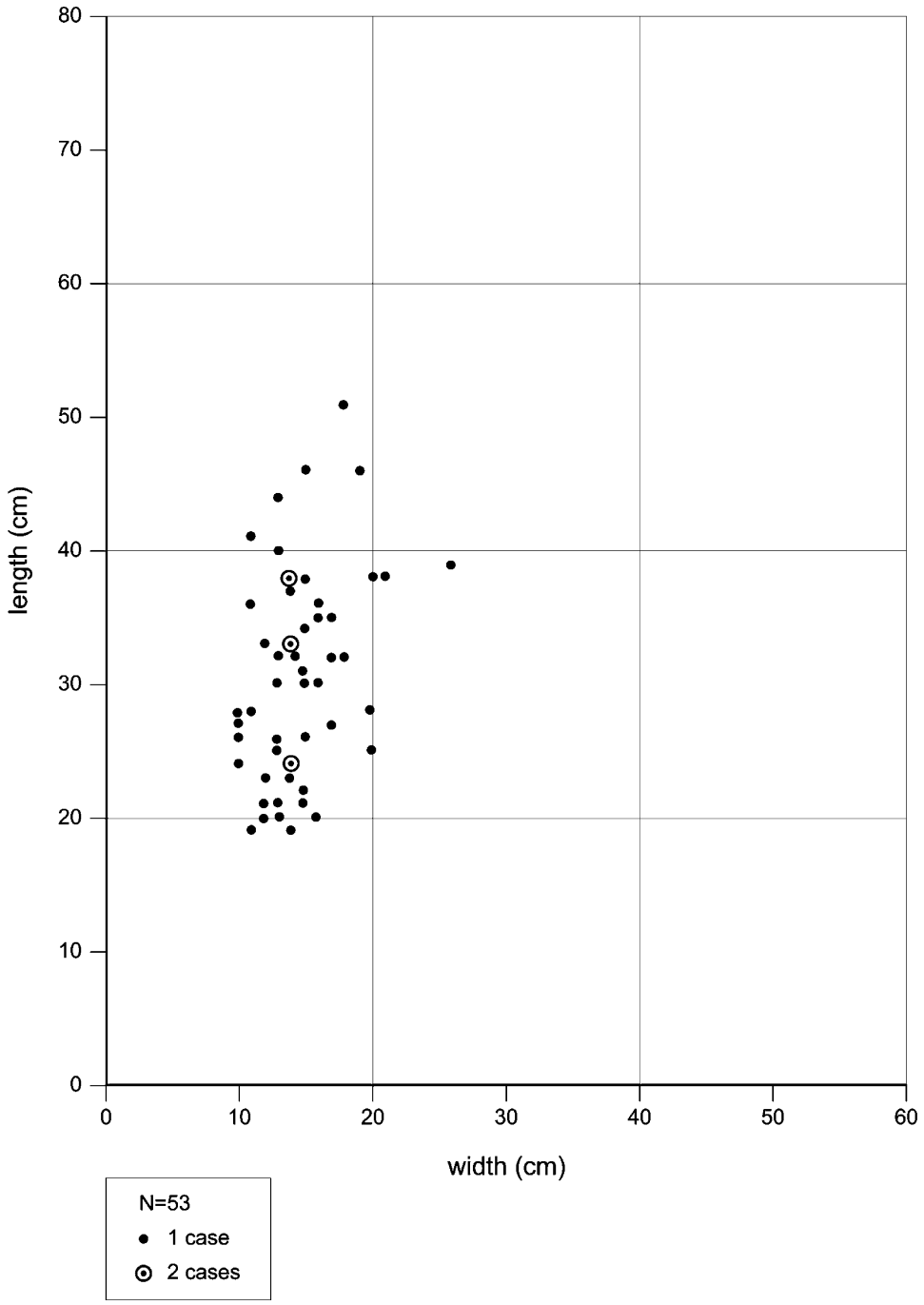

Figure 3. Length/width plots of grinding basins of basin metates from Eddy County.

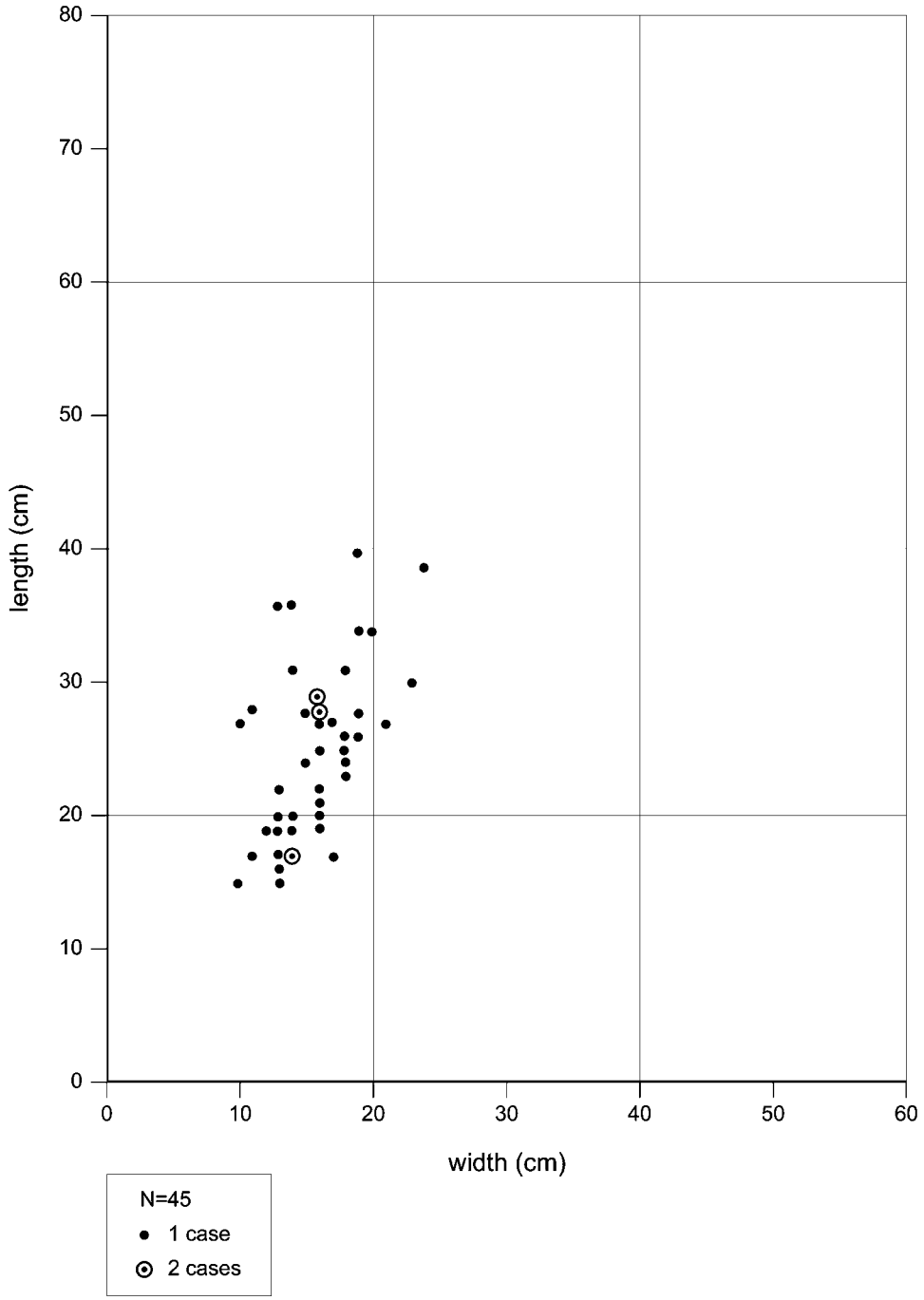

Figure 4. Length/width plots of grinding basins of basin metates from Chaves County.

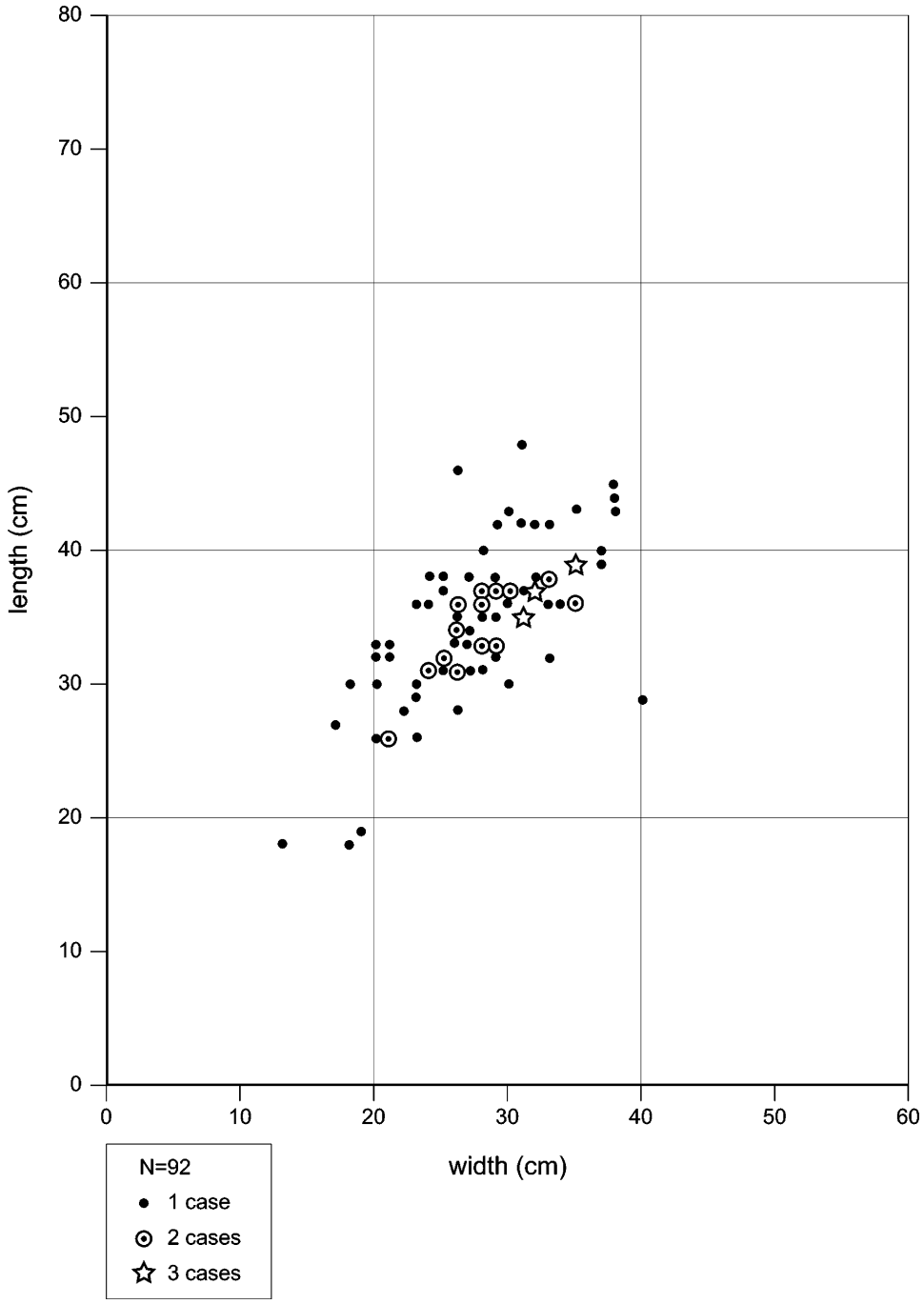

Figure 5. Length/width plots of grinding basins of basin metates from Lincoln County.

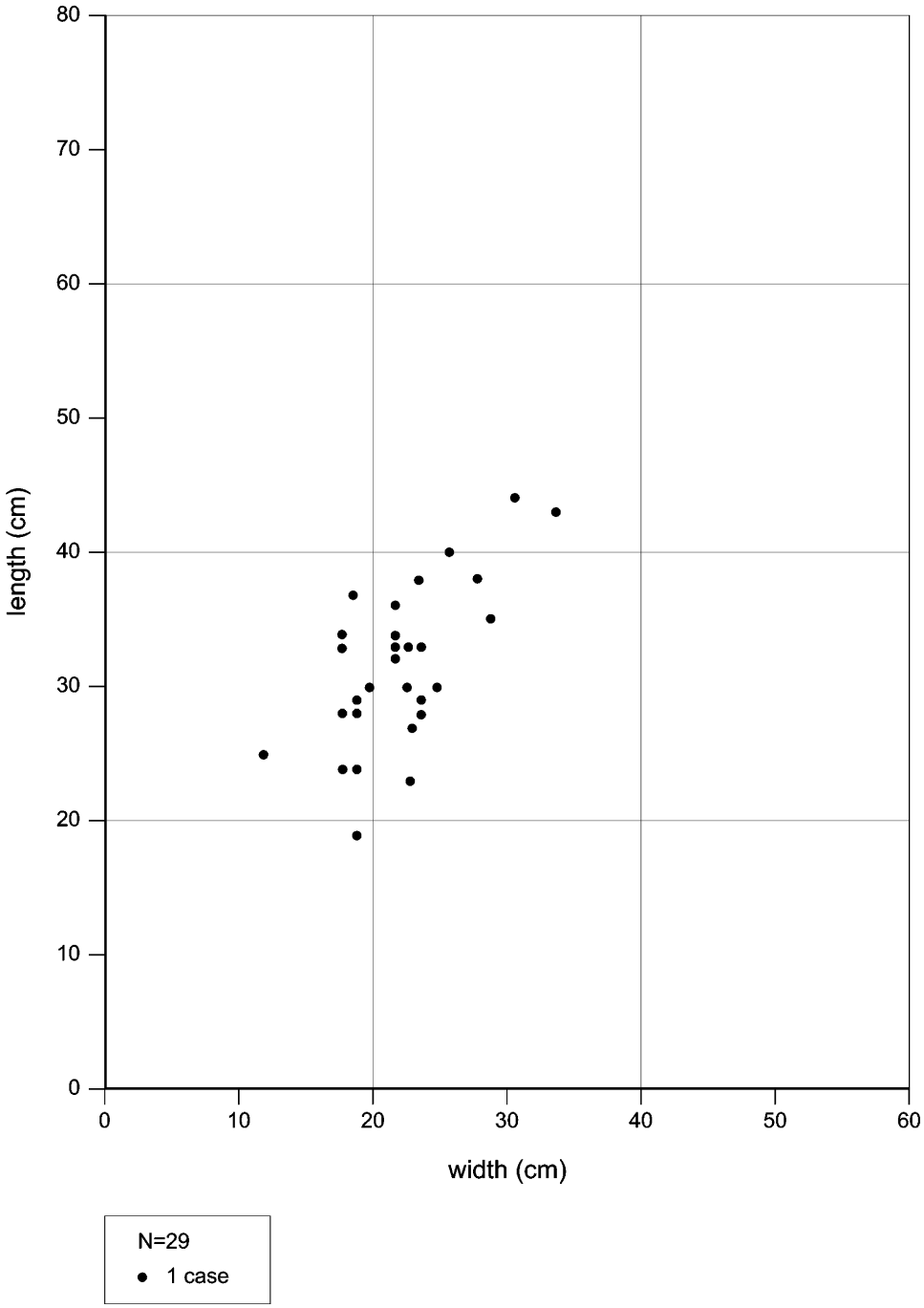

Figure 6. Length/width plots of grinding basins of basin metates from Otero County.

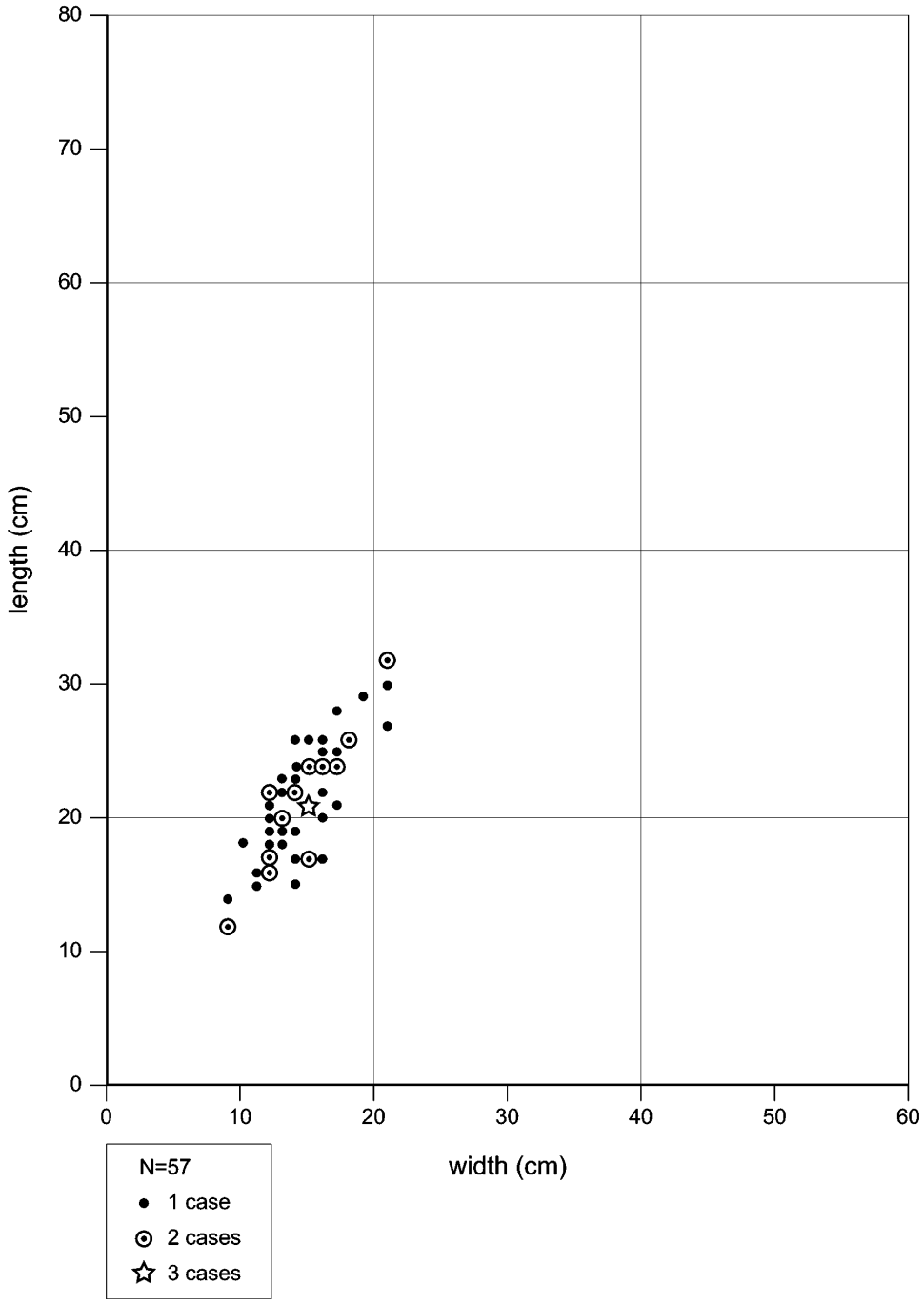

Figure 7. Length/width plots of grinding basins of bedrock basin metates from the Los Molinos site (LA 68182) at Roswell, Chaves County.

surfaces. Except for the bedrock metate site (LA 68182), none of the metates have known proveniences other than the knowledge of the areas habitually collected by the people responsible for bringing the artifacts together.

This study has revealed two surprises. First, all but six of the portable metates are classified as basin type metates. Very few trough metates and slab/flat metates are in the collections, and all but one of those are from Lincoln and Otero counties. A preponderance of the basin type is expected for Archaic and pottery period hunter-gatherer metates, but it is not generally expected for farming peoples because they were reliant to some degree on corn. A search of the regional literature reveals that only one possible mealing bin has been found in excavated sites, and that one is a single-compartment bin in a Lincoln Phase pueblo near the town of Corona in the far northwestern corner of the study region. Together, these two criteria, preponderance of basin metates and virtual absence of mealing bins, suggest that large quantities of corn were not ground in Lincoln Phase sites or any other sites in our study region and that the study area residents relied less on corn than did many of the contemporary societies elsewhere in the Southwest. In contrast, the late prehistoric peoples in the Gran Quivira region immediately west of Corona are known to have used large quantities of corn as evidenced by finds of corn remains in excavations. The majority of their metates are of the trough (38 percent) and slab (47 percent) types (n=916), and multi-compartment mealing bins are occasionally found in their sites (Hayes et al. 1981).

A closer look at the grinding basins of the study metates reveals another interesting facet. In terms of grinding surface area, the Eddy and Chaves county examples are the smallest, as would be expected for hunter-gatherer societies. Also as would be expected, the Lincoln and Otero county farmer metates have the largest grinding surfaces. However, the differences between the two groups of metates are not as great as one might expect; the size ranges overlap to a significant extent, particularly as regards grinding basin length. The primary grinding area discriminator is *grinding basin width*. This factor agrees well with the proposition advanced by Hard et al. (1996), among others, that *mano length* (corresponding with grinding basin width) is an indicator of relative dependence on corn—the longer the mano (and the larger its grinding surface area), the greater the dependence on corn. The same appears to be true of metate grinding surface areas. ☙

ENDNOTES

1. Most archaeologists will probably view the following paragraphs concerning the Roswell oasis and areas east of the Pecos river with much skepticism, if not consider them straightforward heresy. However, these perceptions have formed over a period of nearly 50 years during which I have studied the prehistoric remains of southeastern New Mexico in considerable detail. In these interpretations, I have been influenced to some degree by Art Jelinek (especially his idea about the Middle Pecos peoples giving up farming to become bison hunters, which, at the time [1967], I thought was stretching things a bit) and especially by John Speth with regard to his work at the Henderson and Bloom Mound sites. However, I also am under no illusion that either of these gentlemen will agree with all aspects of my interpretations. Perhaps I have spent too much of my life in the hot sun?!

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