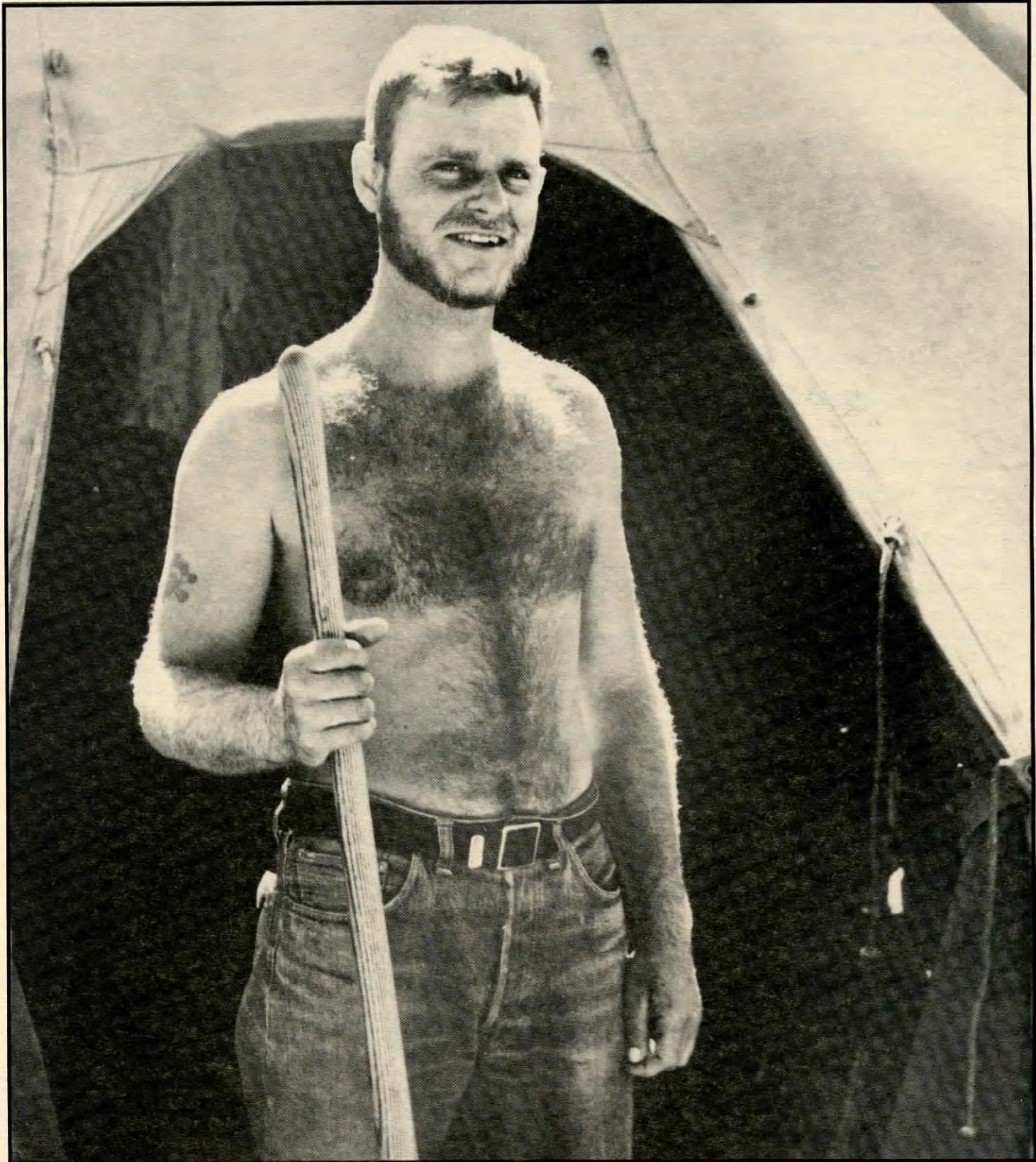


NEVADA ARCHAEOLOGIST

Volume 10, 1992

In Honor of Donald R. Tuohy



ERRATA

The correct volume number and year of publication for this volume of the *Nevada Archaeologist* in honor of Donald R. Tuohy is Volume 10, 1992; the printed cover information is correct, and the header for the individual pages is incorrect.

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Editor's Preface

The papers in this volume were contributed by individuals who claim to be either peers and/or "students" of Don Tuohy. We all, however, consider Don a true friend. Amy Dansie can rightfully claim to be one of Don's top students. Her paper reflects Don's emphasis on sound analysis, data presentation and then interpretation based on the data. Mary Rusco was also associated closely with Don and the Museum through the Nevada Archaeological Survey. Mary's paper presents the legal basis for an important facet of present-day archaeological investigations.

Alvin McLane learned sound field methods from Don during the Pyramid Lake excavations in the 1960s. Don is one of the few individuals that I've heard Alvin compliment as an outstanding field person. Don gave Bill Self access to the Lowe Rockshelter collections and field notes for Bill's master's thesis topic. Don did the same for me with my dissertation research on Falcon Hill.

Dick and Sheilagh Brooks present a classmates' view of Don. The annual Baja trips were a high point for Don, and he would (eventually) return to the Museum refreshed and ready to face a new year. We all have our Don Tuohy stories; Dick and Sheilagh have finally put some of these in print. I, unfortunately, exercised my authority as editor and omitted a couple of the tales. I offer no excuse for my outrageous act of censorship, but you can ask Dick, Sheilagh, Lynda Blair or Hal Turner for the stories.

Bob York echoes the sentiments of many archaeologists who participated in the early days of CRM after passage of the National Historic Preservation Act of 1966. His views are based on 20 years of experience as an agency archaeologist,

and his recommendations deserve consideration.

Finally, Allen Pastron and I initially wrote our paper with the hope of rekindling an ethic in archaeologists to conduct research and publish results of one's work, as Don has always done. We recommended that agency archaeologists encourage and promote publication of contractors' reports. Of note, however, are the recent actions taken against Allen by the Bureau of Land Management, demonstrating the extremes to which a professional archaeologist can be subjected by an agency. Our paper would have read much differently had we known earlier of this new direction for agencies.

Eugene M. Hattori

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A Cache of Bighorn Sheep and Deer Skulls from Northeastern Nevada.

By Amy Jane Dansie (Nevada State Museum)

Introduction

An extraordinary assemblage of six bighorn sheep and two mule deer skulls was found in a rocky talus slope on a Nevada ranch in 1970. Cultural modification of all skulls is intensive and highly patterned. This report summarizes a detailed taphonomic analysis of the chronological and cultural significance of this find (Dansie 1988).

Background

The history of the skull cache was compiled by Tim Murphy (1988), Bureau of Land Management as follows:

A cache of bighorn sheep and deer skulls was found in a talus slope by a Nevada Rancher, when he was getting gravel. The skulls (a minimum of 6 bighorn and 2 deer are represented) were apparently unearthed in 1970.... Mr. Agee (who found the skulls) gave them to Merlin McColm of the Nevada Dept. of Wildlife who in turn loaned them to Bill Wright Jr. of Mary's River Ranch. Bill Wright visited the area where they were found and sent them to Dr. Steve Durrant of the University of Utah for identification.

Durrant's report briefly itemized the elements, aged the ram at about two years, and stated that "no good criteria exist for aging ewes", but noted that there were young and old individuals present. He also noted that the sheep were probably *Ovis canadensis canadensis* Shaw, base on the location of the find.

In 1971, Bill Wright, Jr. submitted a letter to Merlin McColm which contained Durrant's report and comments from a discussion with Jennings regarding the skulls:

Dr. Jesse D. Jennings, Professor of Anthropology, University of Utah... and I have carefully examined the find from an archaeological standpoint and feel it to probably be a "medicine man's" ceremonial cache or burial. It was buried six to eight feet deep in a rock slide with a south exposure and at a height of approximately 10 to 15 feet above the bottom of the slide. The slide is very steep and the burial was reported to be in a small tight pocket.

Mr. Wright recommended dating the skulls, and indicated that he and Jennings interpreted the cut marks to be representative of prehistoric age:

Though impossible to prove without a C14 date all "cut" edges of bone give the more hacked appearance of stone tool chopping and sawing than of steel.

When Murphy examined the skulls in early 1987, he concluded that they were probably chopped with a metal tool. This interpretation implies an historic date, probably after the first white exploration of northern Nevada in 1826 and 1827 (Peter Skene Ogden and Jedediah Smith). The skulls were not radiocarbon dated because of the lack of precision for "whole" bone dates and the 160 year age range for the find. Murphy decided that an accurate identification of the age and cultural association of the skulls required more detailed analysis of the specimens, and arranged for the taphonomic analysis reported here.

My initial observations confirmed Murphy's assessment that the skulls were modified with a sharp metal implement. The basis for this conclusion is described in detail in the original manuscript and summarized below (Dansie 1988).

Bighorn sheep no longer the discovery site and the timing of their local extinction provides an upper date for the skull cache.

Sheep were distributed in scattered ranges over the entire state of Nevada at the time of white contact. In the Elko County area bighorn were the most numerous large game animal reported in the mountains prior to the introduction of domestic livestock.

McQuivey (1978:56) summarizes the data on Elko county bighorn sheep populations and concludes, "These sheep populations were greatly reduced by the turn of the century and have since been completely extirpated. The last documented observation of bighorn sheep in Northeastern Nevada was on the Ruby Mountains in 1921." Hall (1946) also reports that sheep were extinct in Northern Nevada by around 1920-1925. These data firmly support a date prior to ca. 1920 for the skull cache. Therefore, a maximum date range within a 95 year time span, between 1826 and 1920, is indicated based on tool type (metal) and local extinction, although realistically the probable range would be narrower, from 1840 to 1900.

The importance of the skulls in a cultural context lies with their unique modification and dramatic appearance (Figure 1). All ewe skulls are modified in the same manner, producing a visually striking assemblage, stimulating, no doubt, the ritual interpretation.

Elements are noted for each individual in Table 1. The cache is summarized as follows: four intact ewe crania with the front end chopped off across the frontal; one young ewe half cranium modified to the same pattern; one young ram horn with the cranial bone trimmed off by chopping; eight sheep mandibles, all but two missing the coronoid process, and all but one missing the articular condyle; two adult male deer crania with the antlers chopped off and the frontals trimmed as in the sheep; three sheep occipitals and two sheep maxilla portions. The four intact ewe and two deer crania show distinct markings indicating brain case opening and presumably brain removal.

The mandibles were sorted by side and age. Three matching pairs were confirmed from a perfect fit of the complex symphyseal suture. The remaining two mandibles represented two additional individuals. The mandibles were matched to skulls as well as possible, a task made difficult because all joint surfaces were impacted culturally, precluding actual articulation. The oldest ewe mandibles are thought to match with the largest and oldest ewe skull. Likewise, the youngest mandible was attributed to the smallest and youngest skull. The three remaining ewe skulls and mandibles were not as clearly distinguishable on the basis of age. Due to lack of positive association, the mandibles were cataloged separately from the skulls. After cataloging, a systematic analysis of tooth eruption and wear patterns, referring to data in Hansen and Demming (1980), allowed relative aging of the maxillas and mandibles. Age grading the crania by suture closer allowed tentative matching of all ewe elements into an individual list (Table 1).

Of the three loose occipitals, only one fit along a natural suture to the braincase of the larger skull remnant. Another was tentatively assigned to the young incomplete ewe skull represented by a left orbit and horn core portion. The third occipital was notably denser and thicker than the other occipitals. On closer examination it showed the massive tendon attachment scars characteristic of male bighorn sheep. Though not much larger than the old ewe, this ram skull was significantly thicker. It is tentatively assigned to the young ram horn and orbit segment. Both ram elements are heavily modified and do not retain matching adjacent portions. There are only two deer elements (upper crania) representing two individuals.

The skulls were carefully examined and the modification patterns were marked on a standardized bighorn ewe or deer skull

drawing. The deer and ram drawings were modified from female illustrations, and only represent schematic diagrams of modification patterns (drawings from Hall 1946).

Small amounts of periosteum adhere to some mandibles and skulls, but dried connective tissue is virtually absent. Differential staining of the horn cores allowed assessment of which loose sheaths fit on which cores. The actual fit was the determining factor in assigning positive identity of horn sheath attribution. All loose sheaths were attributable to the existing skulls.

Taphonomic Analysis

The general attributes of the cache were initially described in a taphonomic framework, based on Lyman's (1987) overview and synthesis, followed by more detailed description of the skulls later (Dansie 1988).

Although Lyman's review of taphonomic methodology is thorough and useful, the unique nature of the skull cache falls outside his basic analytical structure in important details. Using the patterning and purposiveness criteria together, especially when samples are large and the location of marks can be precisely quantified as in this assemblage, Lyman implies that meanings should be easily derived. In the normal context of butchering for hides, meat, bone grease and tendons, this may well be the case. But as will be shown below, the targeted resource represented by the sheep and deer skulls may not be "normal" carcass products in a subsistence oriented framework of interpretation. The ritual interpretation seems more applicable when the above taphonomic attributes are analyzed, but at this stage, such an interpretation is by default, rather than from specific evidence. Since the more general taphonomic analysis of butchering processes outlined above yields little concrete information in this case, the specific details of the modification become the focus at this juncture.

Mark Morphology

All the skulls in the assemblage studied here have numerous "marks" on the bone. They are straight and v-shaped in cross-section, and often exhibit a high depth to width ratio. Rarely there is minor bone breakage in the vicinity of the mark. In many cases bone is cut cleanly through in one stroke, leaving a flat surface at approximately a right angle to the original bone surface. Some of the deep, v-shaped marks appear to be incomplete efforts to sever bone, as they occur immediately adjacent to and at the same angle as completely severed bone scars. Based on the criteria in Lyman (1987), most of the scars are chopping marks caused by a metal

tool. Some of them are slicing marks, caused by a metal tool, not necessarily the same tool as the chopping tool.

In particular reference to taphonomic implements resulting in evidence for sharp dynamic loading, Lyman summarizes a debate regarding cleavers which is relevant to this assemblage. Langenwalter (1980:107) discussed the Chinese cleaver in some detail:

The Chinese cleaver is a narrow bladed, straight backed tool sharpened high on its side with a bevel similar to a razor. The cutting motion of this tool results in a narrow cut and normally clean separation of bone which is distinguishable from the cut of a European cleaver. The latter has a wider blade with a distinct bevel extending five to ten millimeters up its side. It produces a wider V shaped cut often subtended by an impact fracture at the point of contact. Occasionally an irregular edge on the Chinese tool will leave several parallel micro ridges following the direction of the cut embedded on dense bone specimens. The European tool with its broader blade cannot leave similar markings.

Close examination of the skull cache marks show exactly the same micro-morphology described by Langenwalter, with an incomplete cut morphology showing no trace of a bevel. Although other tool types have been carefully considered, only a Chinese cleaver offers the combination of traits indicated by the mark morphology and force indicators. In addition to mechanical forces represented by marks on bones, each mark has a *location* somewhere in respect to animal anatomy, and each has an *orientation* which can be specified to clearly document the mark (Lyman 1987).

The distinctiveness of the steel tool chopping marks on the skulls, their high frequency and tight clustering in terms of location and orientation leave no doubt as to the taphonomic agent in general—a human being modified these skulls. It is a different matter altogether to identify what "kind" of human performed the modification (ethnic identity), and the "reason" why they were modified and carefully stored.

Even if the taphonomic implement is identified as a Chinese cleaver, the taphonomic agent is not automatically a Chinese person, for any human can use a cleaver (Lyman 1987). A Native American could easily obtain a Chinese cleaver in Elko, as could a white rancher, or a Basque sheepherder. Thus, although identifying the implement is of considerable importance in understanding

Table 1. Individual List

Bighorn Sheep Ewes:

- Individual 1. Skull (10a) left frontal; left mandible 5b, left; maxilla 12, left; occipital (10b). No horn sheath, right frontal missing, possibly split with cleaver. 2 to 2.5 years old.
- Individual 2. Skull (7) frontals; right and left mandibles 1a and 1b. Both horn sheaths present. 3 years old.
- Individual 3. Skull (8) frontals; mandibles 3a and b. No horn sheaths. 3.5 to 4 years old.
- Individual 4. Skull (6) frontals; mandible 4a, maxilla 13. Left horn sheath attached, partially weathered. 4 to 5 years old.
- Individual 5. Skull (9) frontals; mandibles 2a and b. Right horn sheath fits. 6 to 9 years old or older.

Bighorn Sheep Ram:

- Individual 6. Ram horn (11a) (36cm long) and occipital (11b). Ca. 2 years old. No sign of first rut ring (2nd year growth ring), but a ring possibly representing the first year (winter?) growth interruption six inches from tip, blade very thin. It could be just at the 18 month point, based on horn length and thickness of base. Hansen and Deming (1980)

Deer:

- Individual 7. Mule deer brain case, antlers chopped off.
- Individual 8. Mule deer brain case, antlers chopped off.

the skull cache, it actually raises more questions than it answers. Specific taphonomic attributes encompassed in this assemblage provide additional cultural clues to human behavior in the following sections.

The Skulls and Mandibles

Taphonomic Descriptions

Individual 4. This ewe has the most distinctively modified and most complete set of elements in the assemblage (Figure 1a). Modification will be described in detail for this skull. The other skulls will reference this skull, as they are remarkably similar in most details.

The cranium has seven incomplete straight cut marks, indicating perpendicular dynamic loading by a very sharp metal edge, oriented across the frontal 3 to 7 mm above a line connecting the lower orbits. The skull was cloven along this line across the frontal through a nearly horizontal plane which angles back through the proximal nasal nares region, exposing the frontal sinus cavities. This resulted in a triangular cross section through the region between the eyes and the base of the skull. This plane is slightly (7 mm) anterior to the modified plane defined by the trimmed base of the skull near the occipital.

The result of the trimming is that the remaining portion almost sits with the horns upright when placed with the trimmed surface down. A remaining portion of the interior wall of the frontal sinus extends about 3mm out from the general plane, causing the skull to lean to the side a little, pivoting on the projection. The other bone around the projection retains original cut levels. Thus the projection is not a result of attrition to the surrounding bone, but is a product of the trimming. Furthermore, since the trimming of the nasal area is slightly deeper than the more anterior frontal sinus, it appears that the final trimming must have been made from the posterior direction.

Though creating a relatively uniform plane of severed bone, this modification pattern required two distinct types of mechanical movement, performed repeatedly with little error. The frontal was trimmed from the anterior end at a different angle from the delicate and complex proximal nasal nares, which was trimmed from the posterior end of the skull. Although this method of trimming the skull superficially appears to have been aimed at creating a skull which would stand with the horns upright, it clearly does not do so. Only a minor amount of further modification would have been required to level the trimmed area. Also, there is no evidence on the bone that the skull was ever placed that way, as there is no attrition on the delicate bone edges.

The posterior edges of the orbits are also cloven in a separate action from the frontal trimming. Each is severed 14 mm above the surface plane when the skull is placed trimmed side down and held symmetrically upright. The right orbit has a very flat, smooth scar at right angles to the longitudinal surfaces of the postorbital bar, cleanly severing it from the missing zygomatic arch. An incomplete V-shaped scar 12 mm above the severed cut is 7 mm deep, ending in a very narrow apex, with an opening only 1 mm across. This cut is the best evidence for the morphology of the tool blade. It was aimed into the denser bone where the orbit begins to curve into the base of the horn core, rather than through the narrow bar bearing the successful cut. The angle of force application was similar to the complete cut. Other cuts demonstrate that the tool user could easily have penetrated the bone at this location, but they did not apply adequate force in this location to complete the cut. The lack of damage on the temporal in line with the force used to sever the bar indicates a controlled use of force aimed at just severing the

narrow bone on the back of the orbit, but without damaging the braincase. Thus if the goal were to skillfully trim this part of the skull, the correct force was applied in both strokes, but the location and angle were wrong in one.

The left orbit also appears to have been cloven along the posterior border, but post-depositional desiccation and static loading (against a rock?) have bent and frayed the bone posterior to the orbit. The horn sheath on this side of the skull also shows considerable deterioration, indicating weathering. However, weathering on the bone is slight, and two additional small cuts into the orbit are clearly visible. The first is 15mm toward the horn from the frayed cut in the postorbital bar, 7 mm deep and angled toward the horn. The second one is 3 mm deep and would have cut out a v shaped notch in conduction with the former cut, had it penetrated 3 more mm. Both cuts are too narrow to measure the opening, indicating a razor sharp edge used with slight impacting force.

The posterior ends of both zygomatic arches have been truncated, but the mechanism is not clear. Other projecting parts of the skull near the zygomatic base are trimmed off with the cleaver, but show some breakage which could represent using the back of the cleaver as a hammer. These modifications lie on a plane with the base of the skull, surrounding a hole chopped and broken into the brain case. Two small cuts occur on the right zygomatic base (temporal).

This skull exhibits the most distinctive brain exposure actions in the series. There are no less than 10 individual chopping scars, all about 25mm long, oriented parallel to the sagittal plane, running from the foramen magnum to the hole broken into the skull base. The edges of the hole are irregular, appearing broken rather than cut, but a few areas are straight enough to perhaps represent use of the blade edge to expand the hole slightly. The hole is roughly square, 35 mm by 33 mm, with a narrower extension into the occipital 10 mm by 10 mm in size. The opening begins 17 mm from the edge of the foramen magnum. This is the smallest "brain hole" in the series, just barely large enough to work the brain out in pieces. If the brains were scooped out with a tool, one would expect to find some traces, such as striae, abraded hole edges, or sliced-off interior projections, on the inside surface of the braincase. However, no such clues were found indicating the method of brain removal.

The right mastoid process is intact, while the left one has several small cut marks with a scar representing the removal of a slice of bone diagonally along the lower edge. The external auditory meatus is missing on both sides but the dense bone of the ear is intact on both sides. The occipital is firmly attached, with faint suture lines, indicating a fully adult, but not old, animal. The cartilage pad on the occipital condyles remains as a hard, dark yellowish brown material, some of which has cleaver marks through it.

In addition to the skull trimming and the brain case opening, this skull, like all the others, has several distinctive slicing marks around the horn bases. There are 9 marks, all in pairs on the right and 17 in more irregular groupings on the left. Some extend down onto the brain case, and all appear to represent the careful skinning of the head around the horns. The marks are very fine v shaped incisions often angled sharply, but some are perpendicular to the surface. It is interesting that the deepest scars nearest the horns show that the tool was angled down as though the cuts were initiated from the outside around the horns, and not from under the skin as the skinning proceeded toward the horns. The angle of these cuts intersects the horn within a distance which seems

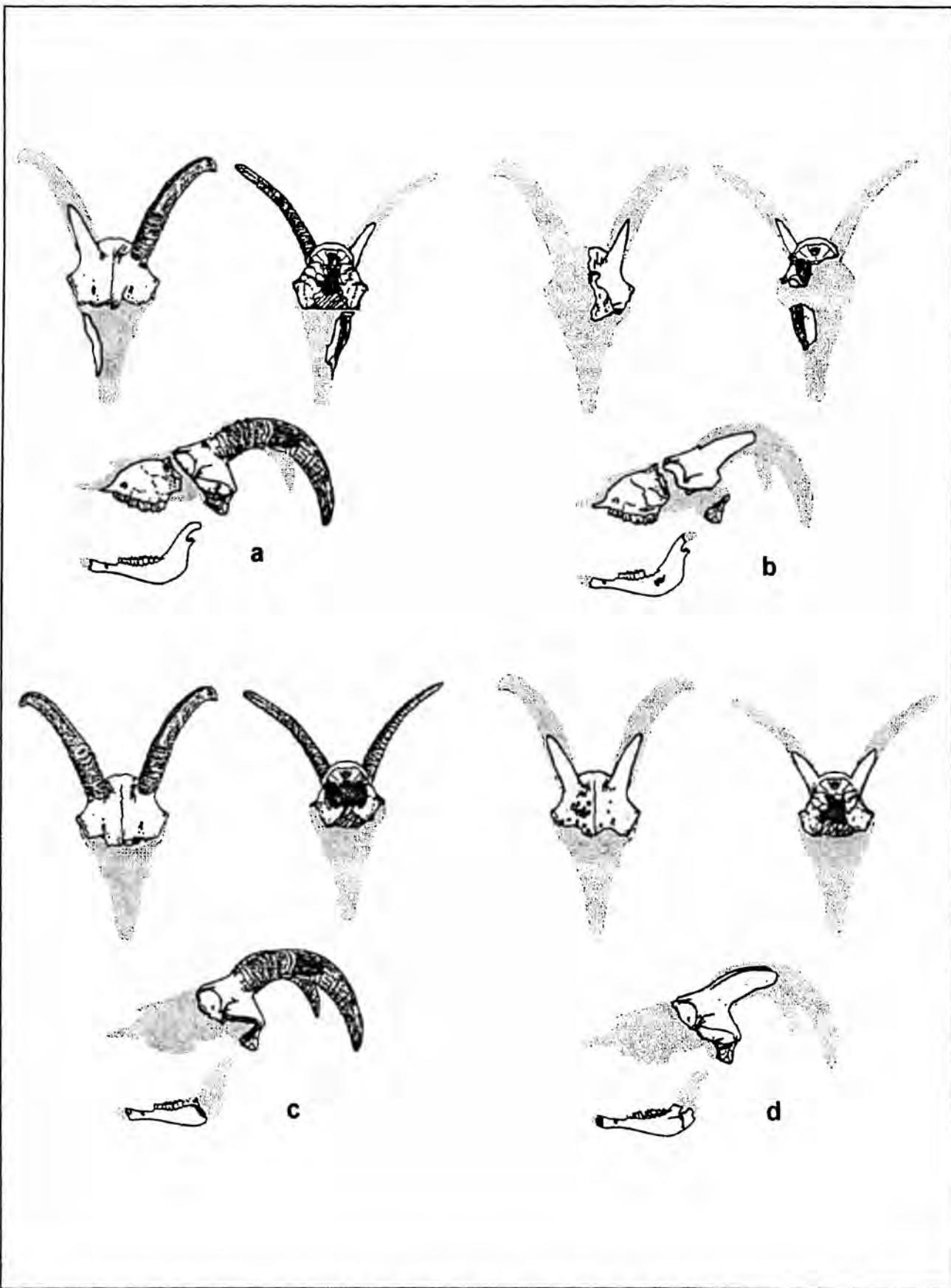


Figure 1. Bighorn sheep ewe crania. a. Individual 4, 4–5 year old ewe; b. Individual 1, 2–2 1/2 year old ewe; c. Individual 2, 3 year old ewe; d. Individual 3, 3 1/2–4 year old ewe.

incompatible with the depth of a cleaver blade, thus a sharp knife is indicated as a second tool type used in skinning.

The left horn sheath remains attached to the horn core, though bleaching, fraying and attrition of the horn sheath is fairly severe. The right horn core is dark yellow-brown, indicating that it only recently lost its sheath. None of the loose sheaths fit this core, providing some evidence of recent assemblage loss or incomplete recovery by the rancher.

The maxilla attributed to this skull shows incomplete cleaver scars angled sharply toward the posterior of the skull, as if the skull were held with the nose up, and cloven straight down along the face. The severed edges are somewhat irregular, and do not clearly indicate distinct cleaver scars. However, the plane of the edges is straight enough to indicate two distinct actions were involved in severing the front of the skull from the back. The incomplete scars are not consistent with the action required to sever the maxilla from the skull, and are not explained. They appear to represent impact scars, and not slicing scars related to skinning.

The mandible attributed to this individual is the most complete of the series, having an intact coronoid process and condyle. The first premolar is missing, postmortem, and the remaining teeth are in good condition. The incisors are missing, with damage to the anterior bone surrounding the tooth roots.

Taken together, these elements show that a zone of bone about 1 cm to 1.5 cm thick between the maxilla and lower orbit was removed and damaged in the processing of the skull, suggesting that a rough severing of the front and back portion was done first, then the horn bearing portion was carefully trimmed in a later step.

Individual 1. The other skull remnant with an attributed maxilla is the youngest ewe (Figure 1b). There are 11 skinning scars around the horn base, but cleaver scars are obscured by postdepositional damage. Skull was trimming was similar to the other skulls, but the brain case might have been split open top to bottom, rather than left intact as in the others. The maxilla shows several deep cleaver scars penetrating the facial area just anterior to the plane of severing the front half of the skull anterior to the orbit. The angle of force contrasts with the scars on the maxilla of Individual 4, described above, even though the location and orientation are the same. The maxilla cut marks in this skull are clearly related to cleaving the skull transversely. Two slicing marks at right angle to the surface show just above the tooth row, parallel with the tooth row. These may represent severing of the interior attachment of the lip from the skull during skinning, as they are somewhat anterior to the region where the masseter muscle is cut (leaving characteristic scars) when the mandible is removed (Binford 1981:102), but they also could represent mandible dismemberment.

The mandible of this young ewe retains two heavily worn deciduous teeth, and is missing the incisor bearing portion entirely. Four small cut marks occur on the posterior border of the ascending ramus.

Individuals 2, 3, and 5. The remaining ewe skulls are modified much like the first described skulls (Figure 1c and d, Figure 2), with variation primarily in the number of visible cut marks. No maxillas representing these skulls were recovered, yet all three have both mandibles, contrasted with the first two skulls which have a maxilla and mandible from one side only. All showed modification in the same locations, with evidence for both a cleaver and a knife. There is a tendency for the mandibles to exhibit patterned damage resulting from removal of the mandible from the skull. Most of the damage to the coronoid process or condyle is in the form of breakage, some of which appears to be impact on green bone, but

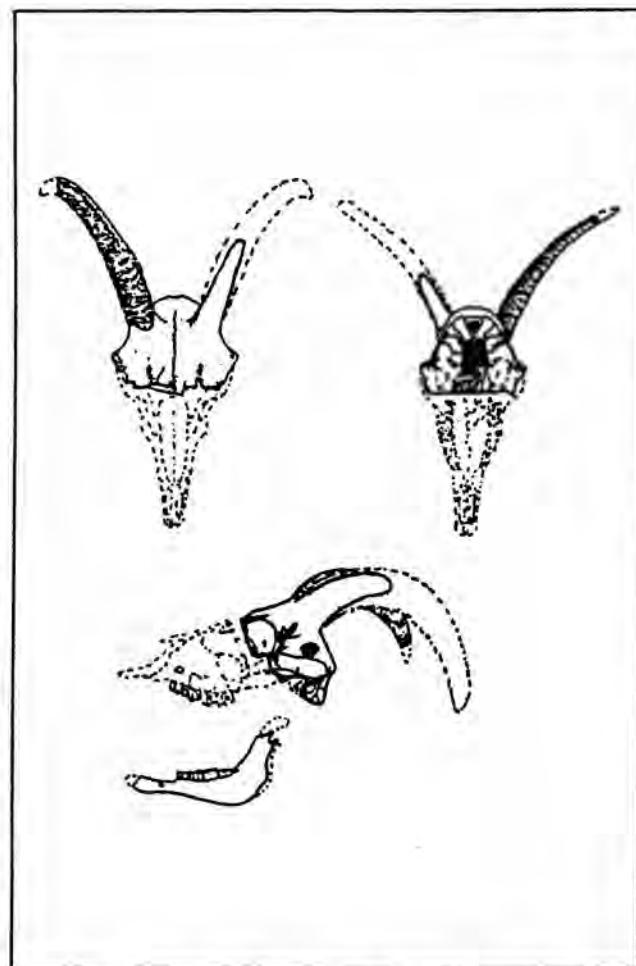


Figure 2. Individual 5, bighorn ewe, 6–9 years old. Possible bullet hole penetrates left parietal.

some may be post-depositional as the edges are rough and irregular. All mandibles are missing the incisors, whether the bone is damaged or not, suggesting perhaps that they were removed from a boiled jaw. The consistent absence of these teeth might have resulted from intentional extraction, and may yet another "use unit" represented in this assemblage.

The oldest individual (Individual 5, Figure 2) has a hole in the side of the brain case which might be a bullet hole. It shows just enough regularity and form to suggest such an interpretation, but weathering of the edges precludes confirmation. There is no internal damage in the braincase showing bullet damage.

Individual 6. The ram skull remnants consist of a systematically trimmed left horn core with sheath attached and an occipital, heavily modified by a sharp steel implement (Figure 3, 4 and 5), probably the same cleaver used on the ewes. There are many skinning scars around the base of the horn, and the horn appears to have been "shaved" along the interior surface, possibly a by-product of hacking the skull away from the base. As shown in Figures 3, very little of the skull remains, and all remaining portions show signs of cleaver scars. It was intentionally trimmed to the shape shown, and thus represents a different method of trimming than shown on the ewe (and deer) skulls.

The occipital remnant exhibits cleaver scars all around the existing edges (Figure 5), as if it were trimmed into a cup shape. There are no less than eight cleaver strokes on the right and nine

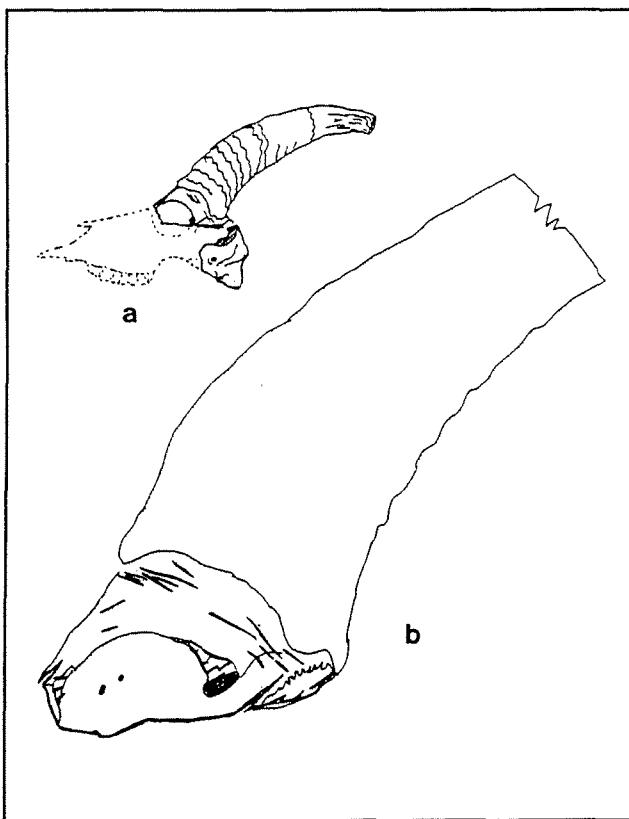


Figure 3. Individual 6. a. diagram of remaining horn core and occipital elements; b. Close-up of horn core base, sheath firmly attached, showing skinning scars and cleaver marks on lateral side (horn truncated for drawing).

on the left side, forming two intersecting planes meeting at midline. There is no sign of rounding or other attrition indicating it was ever used for anything. The flat, smooth and beveled edges of the occipital remnant contrast clearly with carnivore gnawed "cranial discs" described by Binford (1981). The ram occipital also exhibits classic skull dismemberment scars on the occipital condyles.

Individuals 7 and 8 (deer skulls). The two deer crania are trimmed in the same manner as the ewe skulls, cloven through the frontal toward the anterior basicranium (Figures 6 and 7). The brain cases were opened from the top, however, in distinct contrast to the ewe skulls, which were all opened from the base of the skull. The deer skull brain openings clearly exhibit the deep, sharp cutting action of the cleaver. Chopping force ending abruptly in the tight square could not be the result of any knife-like tool, or the tip would have hit the bone just beyond the hole.

More remarkable are the antler bases which exhibit the best evidence for the use of a steel cleaver in the entire assemblage. An average of 20 cleaver scars are found on each antler base. Two different techniques were used, one cutting the antler above the bone, and one cutting the bone below the antler. The bone-cut antler removal scars indicates that the tool was sharp when the operation was started, as the scars are in some cases over one centimeter deep in a single stroke. A deterioration of the tool edge may be indicated by shallower scars on one than on the other. Also notable are the numerous parallel striae perpendicular to the scar length, as described by Langewelter above. Some of the edges of previous scars were friction-polished by subsequent passage of the blade surface. These scars show the direction of the applied force and demonstrate conclusively that the antlers were not sawn off

with a stone or steel tool, but were chopped off with a very sharp edged and relatively heavy steel tool.

Chronological Indicators

Chinese cleavers were probably not available during the early exploration and fur-trapper era in northeastern Nevada. However, it is still possible that the cuts could have been inflicted by an unusually well-honed hatchet or axe. The deep cut marks of the cache represent a straight cutting edge with an abrupt end, like a cleaver or straight hatchet. Since most such tools in the early historic era were hand forged to various specifications, it would be impossible to argue conclusively for any particular tool type other than a range which includes a very thin, unbeveled, razor sharp blade heavy enough to sustain repeated sharp dynamic impacts on solid bone. Therefore we cannot use the tool type to determine the historic period or the ethnicity of the tool wielder. Is there another source of time sensitive information available for these skulls?

Bettenger's efforts to use lichens for dating archaeological features (Bettenger and Oglesby 1985) offers a possible approach. Perhaps we can add chronological information from the presence of lichens on one of the skulls (Individual 3). Bettenger and Oglesby explored growth rates of lichen in the White Mountains of eastern California.

Historical structures provide the data for the modern growth rate for these slow-growing plants. It takes about 50 years to begin visible colonization, less in moist areas. Preservation of the skulls indicates rather arid local microenvironment (with some exceptions), so the 50 year colonization figure will be examined. In the

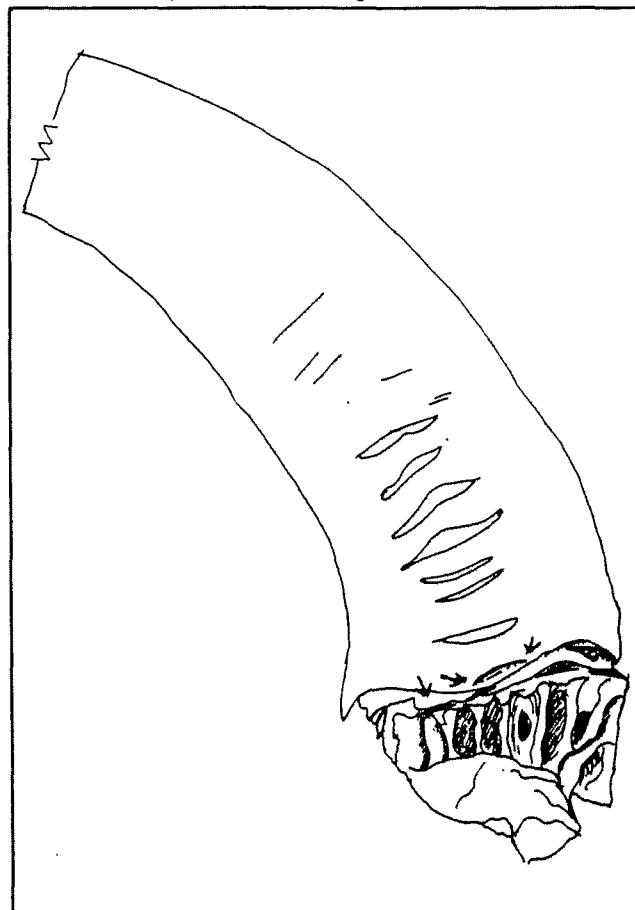


Figure 4. Individual 6. Close-up view of medial horn, showing skinning scars, cleaver scars and shaved off areas, some associated with straight terminations of very shallow cleaver strokes.

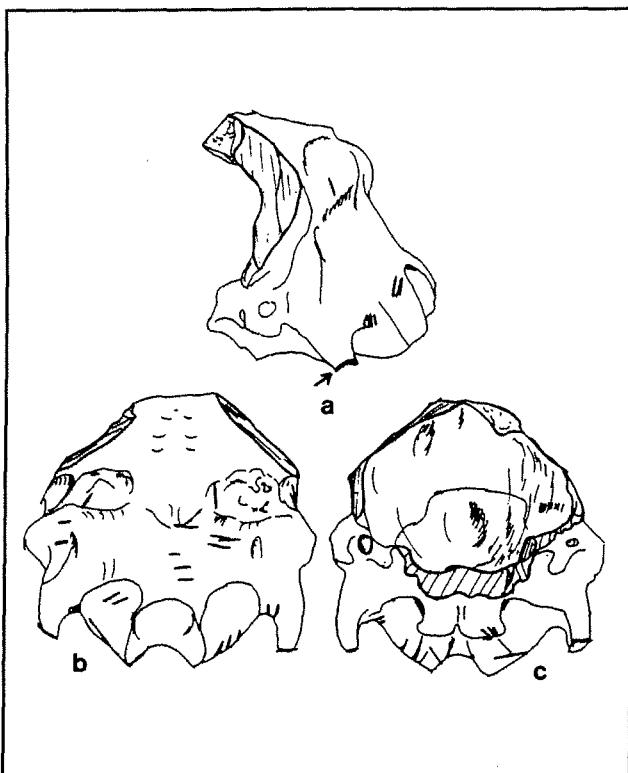


Figure 5. Individual 6, Three views of intensively modified occipital. The cross hatched areas are flat with multiple cut planes separated by minute steps, each indicating successive cleaver strokes wielded with unerring accuracy at the same location. Numerous decapitation scars show on each view, and both mastoid processes have been cloven at the tip. a. side view; b. exterior (posterior) view; c. interior (anterior) view.

first hundred years the growth rate is much higher than later in the life of the lichen. A rate of 0.14 mm per year is reported as the average rate used to estimate dates on features (Bettinger and Oglesby 1985). The largest thalli on the sheep skull is 5 mm in diameter. Adding 50 years before growth started, the approximate time the skulls were in the talus slope would be somewhere around 75 to 86 years. Bettinger shows a date of A.D 1910. for 5 mm diameter thalli ($1985 - 1910 = 75$ years, but $5 \text{ mm} \times 0.14 = 35.7 + 50 \text{ years} = 85.7$ years). Thus a date of about 1901 is indicated, just about the time sheep were first protected by law, and after they had declined seriously.

The excellent condition of most of the skulls, even including bone periosteum on some elements and pliable, greasy bone marrow in one mandible, suggests a recent date within the 1826 to 1920 date range indicated by other basic facts (metal tool, local extinctions). The admittedly crude lichen date of 1900 is surprisingly consistent with this assessment. This can help narrow down the cultural interpretation to a setting in which sheep were becoming steadily less common every day, and hunting them was illegal. It should be noted that deer were also probably becoming steadily more common during this same time period, as was documented for southern Nevada under the same conditions (McQuivey 1978).

Discussion

It is clear that some very specific and complex human behavior is represented in the skull cache. Within the general butchering unit of the skull there is precise evidence for the extraction of hides and

brains in all seven individuals. In the case of the deer, the antlers were also clearly extracted through purposeful human action, in a more precise manner than documented by Binford (1981), where the brain case was smashed to remove the antlers of caribou. The missing horn sheaths might also represent the extraction of sheep horn from some of the ewes, and the ram was trimmed so that only one horn sheath with a small amount of bone was left. Perhaps the other horn was utilized elsewhere. The trimming of the anterior portion of the face could perhaps represent removal of the nasal nares and exposure of the frontal sinuses for nutrient extraction, but the precision and location of the trimming is in contrast to documented cases of nose removal (Binford 1981). Finally, the incisor teeth may have been systematically removed for some purpose, perhaps, as in elk teeth used for necklaces farther north, they were extracted as ornaments.

Because butchering mark morphology supports the identification of a Chinese cleaver as the primary taphonomic implement, the possibility of a Chinese taphonomic agent was carefully considered. Because Chinese medicine involves the use of horn and antler, and there was a large Chinese community in Elko County in the late 19th century (Carter 1972), this is not a remote alternative. Specific data supporting this possibility were not encountered. Consultation with Chinese material culture specialists revealed no known documentation of Chinese modification of sheep and deer skulls, nor of the secretive caching of such items. Though not supported by specific ethnographic data, this possibility has not been ruled out categorically.

Jennings' original interpretation that the skulls represented a Native American ritual cache (Wright 1971) is an obvious possibility. Ritual modification of skulls is a well documented Indian behavior in the American West (Stewart 1941; Tuohy, nd). However, little specific data on the exact nature of the modification are mentioned when "ritual modification" is noted. When Tho-

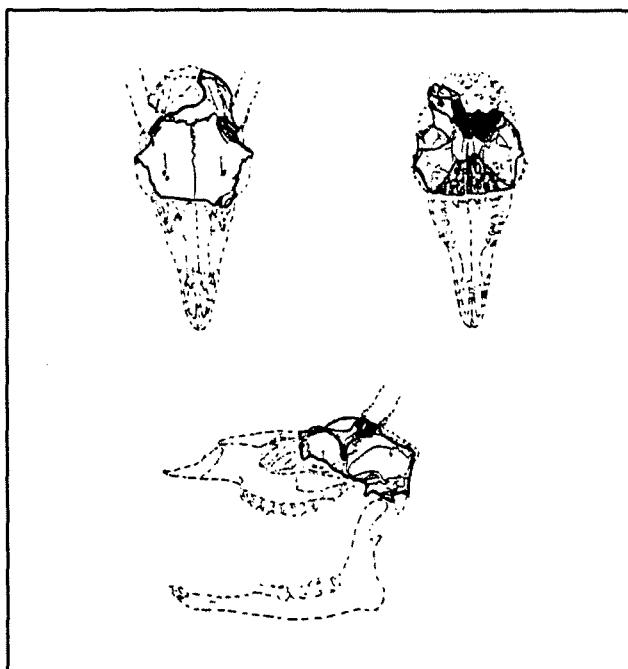


Figure 6. Individual 7, cranial elements attributed to adult mule deer buck. Antlers were chopped off below the antler bases, through solid bone, by a sharp cleaver penetrating over 1 cm on the first few strokes. The brain case was opened by cleaver from the top of the occipitals. Subsequent damage may be post-depositional.

mas and Mayer (1983:374) faced the opposite problem of interpreting the *rarity* of skulls in a well preserved bighorn sheep butchery assemblage in Gatedcliff Shelter, Thomas noted a find he and Tom Layton recorded in Northwestern Nevada. A male bighorn skull exhibiting "extensive butchering marks(made by a stone tool)" was found in a stone cairn. A ritual interpretation was offered.

The skulls in this skull cache were clearly heavily modified, but in many details the modifications were apparently aimed at extracting useful products. The two concepts are not mutually exclusive, and the skull cache could represent both systematic extraction of most (but not all) potential skull products, and the ritual treatment of the remaining portions out of respect for the animals within the spiritual framework of the hunter. The perceived decline of the local sheep populations, and perhaps even a recognition of pending extinction, may have enhanced the emotional significance of these particular skulls so fully and carefully

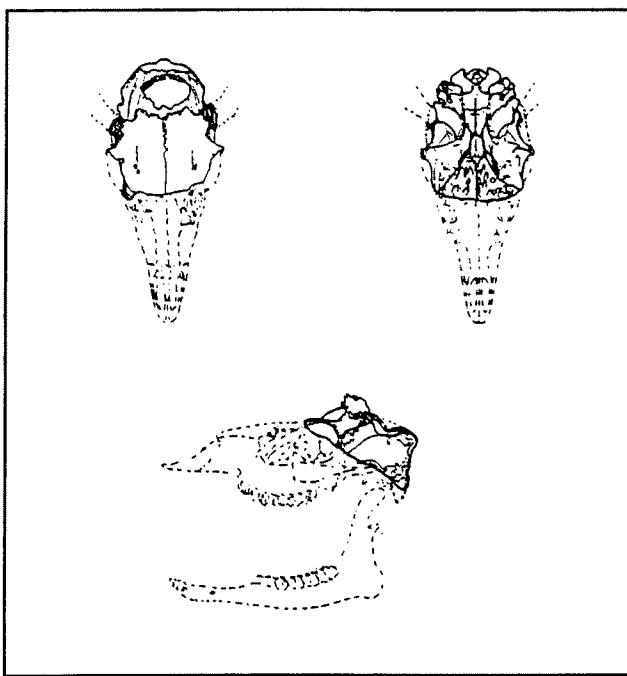


Figure 7. Individual 8, cranial elements attributed to adult mule deer buck. Antlers were chopped off above the antler bases, by a less than optimally sharp cleaver, penetrating less than 4 mm on any stroke. Brain case is opened by cleaver from the top of the parietals.

utilized.

The differential removal of the horn sheaths could represent cached products occasionally retrieved over time. Although there is considerable evidence for native use of sheep horn and deer antler (arrow shaft wrenches, sickles, spoons, bows, flakers, awls), Stewart (1941:234) notes that many of these "were quickly supplanted by objects procured from the white man." The ewe skulls could have been destined for headdress disguises (documented as rare by Stewart 1941), but the ram and deer would not have been useful for such a purpose in the form in which they were stored. Pendleton (1985) discusses other prehistoric uses of bighorn sheep horn, none of which are directly indicated in the Agee Skull Cache.

During the last 20 years that bighorn sheep existed in the region around the cache site, it was illegal to hunt them, and they had been getting scarce before that, prompting their legal (albeit futile) protection. The sheep represent distinct ages, which might

represent a single band under the oldest ewe (the male had not quite reached his first rut). They might have been one of the last bands in the area, killed at one time. An appreciation of the declining sheep population might well have stimulated intensive ritual treatment of the sheep skulls, after they were utilized for their most valuable products.

It is interesting to note the lack of intensive marrow extraction, a practice common in prehistoric butchering patterns. The maxillary and mandibular marrow cavities were systematically intact, *none* of them were used for marrow, providing a rare case of positive evidence for the *non-use* of a specific carcass product. Thus the skulls do not represent maximum food extraction. Some of the butchering mark sets document the use of tool and hide products. However, none of the functional interpretations of products extracted from a skull accounts for the systematic trimming of the sheep and deer skulls. The lack of use wear on the trimmed surfaces, some of it very delicate bone, shows that the skulls so carefully trimmed were not *used* in a ritual context. It may have been that the mere act of trimming and caching was the ritual act.

Though ritual interpretations are often viewed as an excuse for lack of thorough analysis, in this case, intensive analysis directly supports a ritual component in the human behavior creating it. It does not appear that this assemblage is entirely ritual in content, but may be primarily so in its final form. Since there is no sign of ritual *use* (handling attrition, ochre staining), these skulls are probably not ritual paraphernalia in themselves. The Shoshone ethnographic literature does not specify that ritual *use* would be expected, on the contrary, Stewart (1941:288) states that "ritual and magic played no important role in the culture" related to hunting. Furthermore, Indian shamanism "was the concern only of individuals" (Stewart 1941:216). However, a religious or spiritual content cannot be ruled out in this sense.

These skulls may have been carefully stored away in a remote talus slope for future use in ritual or individual spiritual activity settings, and never claimed due to the death of the person involved. However, in reference to the placement of the skulls in the talus slope, Stewart (1941:257) noted that "burial was preferably in rock slides or talus slopes,...as it required no digging," suggesting that the cache reflects economizing ritual burial behavior, and the setting is not as bizarre as it first appears. Whether "stored" or "buried", these skulls were placed in a talus slope as normal behavior within the local Shoshone culture.

Conclusion

The modification of these skulls is not recognized as typical of any specific cultural practice, but is consistent with the known ritual modification of prey animal skulls and disposal out of reach of scavengers (Don Tuohy personnel communication). The cache probably represents an idiosyncratic, perhaps shamanistic, behavior within a broader, and fading, cultural context of Native Americans in the late 19th or early 20th Century.

The survival of traditional values and behaviors in a post-contact setting represented by this assemblage makes it an important and unique document of Native American anthropology. The care and precision creating this archaeological find supports the current Native American assertion that a sacred attitude toward nature goes beyond simple categories of religious ceremonies into the entire realm of culture. Every one of the hundreds of cutting operations was done with precise aim and controlled force application.

This is the only archaeological find this author has seen that so precisely evokes, in precise physical evidence, the signature of

human spirituality. Like human art, care and methodical precision in handling anything in the material world leaves a material trace of the state of mind of the creator. This is more than mere "fossilized behavior", it is fossilized spirituality. The abundant physical facts of geography, ecology, chronology, history, behavioral taphonomy and anatomy in this remarkable find support this conclusion, in a field of study which is usually forced to rely on a paucity of facts. It is a rare find, one which Don Tuohy identified accurately at first sight, and found the references to support it, an even more remarkable accomplishment to me. I am proud to dedicate this report to him, my mentor and friend.

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Native Nevadans, the Federal Government, and Archaeologists

by Mary K. Rusco (Research Associate, Nevada State Museum)

When Don Tuohy joined the staff of the Nevada State Museum in 1964, he did not need federal legislation to tell him that he needed permission to survey and excavate archaeological sites on the Pyramid Lake Indian Reservation. His field work there was always and still is undertaken with the permission and sometimes at the invitation of the tribe. He has made a practice of employing tribal members on his crew and has enjoyed a long and fruitful relationship with the tribe.

Don has a long record of providing assistance to Nevada tribal groups concerned with protecting Indian grave sites from vandalism. The most dramatic effort took place in 1985 and 1986 when rising water in the desiccated Stillwater Marsh exposed burial sites. When notified by the Fallon Paiute Shoshone Tribe, Don made resources of the Nevada State Museum available to direct and coordinate a large volunteer effort to recover the exposed human remains and associated grave goods. After excavation and a brief period of study, these remains were reburied in a vault within the Stillwater area, as specified in a cooperative agreement between the Tribe and the U.S. Department of Fish and Wildlife.

Subsequently in 1989 *NRS 383* (the legislation establishing the State Historic Preservation Office) was amended by Nevada's State Legislature to afford protection to Indian grave sites. This legislation calls for notification and consultation with tribal councils in the event of the accidental discovery of an Indian burial site or prior to archaeological field work in a site where burials might be discovered. It also includes a provision for reburial.

Where ever you go in Nevada you are in the traditional territory of one of the four Great Basin tribal groups; in some parts of the state you may be on or near an Indian reservation. There is also an 86% chance that you are on federally managed public land. These conditions have consequences for Nevada archaeologists. Anthropologically trained archaeologists have long been aware that there is much they can learn from living people about the practices and material culture of their ancestors. Moreover, they have acknowledged that it is common courtesy to notify the local tribal council when wishing to engage in field work on or near a reservation, and of course it is necessary to seek the council's permission to work on the tribe's trust land. Today the notification of tribal councils and consultation with tribal elders are, in these as well as many other cases, legally required before beginning or during an archaeological project.

The United States Congress in 1978 passed a statute, known as the *American Indian Religion Freedom Act* (AIRFA), as an attempt to bring an end to a long history of federal restrictions on the free religious practices of Native Americans. Early federal policy had been directed toward the discouragement or even prohibition of traditional religions, but this has not been official policy since the passage of the *Indian Reorganization Act* in 1934. Since then, the many government actions that were objectionable to Native Americans were not the implementation of federal policy, but rather the result of administrators' insensitivity and ignorance about traditional religious practices.

AIRFA (*P.L. 95-341*) was a joint resolution of Congress that affirmed "the policy of the United States to protect and preserve for American Indians their inherent right of freedom to believe, express, and exercise" their traditional religions. Its preamble acknowledged the First Amendment guarantee of this inherent

right for all people and that the lack of a clear federal policy often resulted in violations. The resolution recognized that some laws designed to protect endangered species and some land management decisions had unintentionally prohibited the use and possession of sacred objects and free access to sacred sites.

The intent of Congress seems clear: Native American religions are entitled to full First Amendment protection, recognizing that they differ in significant ways from dominant religious traditions in the United States. All relevant federal agencies began to evaluate their policies and practices that might affect the religious freedom of Indian people, and AIRFA was well-received in Indian Country. Other Federal laws were amended and regulations were adopted to recognize this Congressionally-advocated policy. These included amendments to the *National Environmental Policy Act* (NEPA) of 1969 and the *National Historic Preservation Act* (NHPA) of 1966 and provisions of the *Archaeological Resources Protection Act* (ARPA) of 1979.

NEPA established the Council on Environmental Quality and the environmental impact assessment procedures. Regulations adopted in 1978 after the passage of AIRFA in that year clarified the role of Indian tribes and provided for their notification and for their participation in the planning and research early in the environmental review process (*Federal Register Vol. 43 No. 230:44987-56007*). Regulations also include assessment of social and economic as well as religious impacts.

NHPA established the President's Advisory Council on Historic Preservation and the National Register of Historic Places (*80 Stat. 915*). Amendments in 1980 (*P.L. 96-515*) expanded the register and the role of the Advisory Council in ways that insure that Indian tribes have full opportunity to participate under the Section 106 consultation provisions, which require reviewing federal undertakings on non-Indian as well as tribally owned lands.

ARPA expanded penalties provided in earlier legislation to protect archaeological sites on federally managed public lands. It calls for notification of Indian tribes before archaeological excavation permits may be granted on their traditional as well as tribally owned lands. The supplemental regulations passed later (*43 CFR Subtitle A, 1989 Edition*) specify consultation when an excavation or collection permit may have an effect on an important cultural or religious site.

In 1989 the Nevada Senate and Assembly passed an amendment to existing State Historic Preservation law to protect Indian burial sites from vandalism and destruction (*NRS 383.150*). It covers private as well as state land, but is superseded by federal legislation and regulations on federally-managed land.

During the past year the nation's museums have been inventing archaeological and ethnological collections (particularly those associated with human remains) from Native American tribally-held or other traditional lands as called for by the *Native American Graves Protection and Repatriation Act* (NAGPRA) of 1990 and its proposed Regulations (*43 CFR Part 10*). This act provides for the repatriation of certain classes of objects to Indian tribes determined to be "the direct descendants of or culturally affiliated with the historic or prehistoric persons or population whose remains, funerary objects, or sacred objects are being requested under these regulations" (*43 CFR 10.14b, c*).

Collectively all of this legislation and resulting agency actions has had a significant impact on archaeological practice in Nevada. Native American dissatisfaction with the effect of AIRFA has led to the preparation of new legislation.

During the decade following the passage of AIRFA, agency consultation with tribal governments yielded some results that

were satisfactory to both parties. Where this did not happen, litigation ensued, with over 20 significant appeals reaching the United States Supreme Court. A series of progressively more adverse decisions culminated in the April 1990 ruling in *Employment Division v. Smith*, in the words of one non-Indian religious leader, "eviscerated the First Amendment's Free Exercise of Religion clause" (Howe 1994:8).

Long before that time, the Native American Rights Fund attorneys and other organizations, individuals and tribal governments had begun to consider legislation. Sen. Daniel Inouye and the Senate Select Committee on Indian Affairs considered amendments to AIRFA. This resulted in the preparation of the *Native American Free Exercise of Religion Act* (NAFERA), S. 1021, introduced by Sen. Inouye in May 1993.

Like the other legislation discussed above, NAFERA can be expected to have its own impact on archaeology. Archaeology as we have known it is changing in many ways. I do not think we can predict the extent and nature of this change, but it is possible to make some preliminary estimates. First, archaeology may become more costly and may require more extensive preparation before field work. Final reporting will also become more time consuming and expensive as agency review of archaeological reports is extended to include tribal comments. Certain kinds of archaeological and ethnographic collections long maintained in museums for study and exhibit may be subject to repatriation after a relatively brief study time. For many archaeologists all of this expense and delay will seem burdensome and unnecessary. Others, however, may find that the ensuing dialogue between archaeologists and Native Americans may be beneficial. Archaeologists may benefit greatly from insights they receive from tribal elders who visit an archaeological excavation in process. Finally as Native Americans gain a greater understanding of the knowledge that can be derived from the study of archaeological and ethnographic collections they may become a new and powerful constituency for the protection and scientific study of archaeological sites.

Portions of this paper have appeared in different forms in environmental assessments for Tosawih Shoshone territory (Rusco and Raven 1992) and the Nevada Test Site (Pippin 1991).

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S. M. Wheeler: Nevada Test Site's First Archaeologist.

By Alvin R. McLane (Desert Research Institute, Reno, Nevada)

Sidney M. Wheeler (1902-1959), generally accompanied by his wife Georgia, conducted archaeological research over Nevada and southern California from 1934 to 1956. He came to Nevada in 1933 as a lieutenant in the newly formed Civilian Construction Corps (CCC). While stationed at Kaolin near *Pueblo Grande de Nevada* (Lost City) in southern Nevada, he met M. R. Harrington of the Southwest Museum. From this contact and through reading, Wheeler gained an appreciation for archaeology.

Wheeler was born at Digby, Nova Scotia, Canada, and his later high school years were spent in Springfield, Massachusetts. He also attended Suffield Military Academy in Massachusetts and then graduated from West Point U.S. Military Academy. Wheeler married Georgia Slone, of Lufkin, Texas in 1927. He was of slim stature and possessed a military bearing. His field attire consisted of khaki clothes, a pith helmet, and tennis shoes. To his friends, he was known as Wheeler or "SM." Also, he had an unusual quirk by signing his letters "sm Wheeler". During his archaeological years in Nevada and California he was known as a practical joker.

After transfer to Caliente, Nevada in 1934 from Kaolin, Wheeler's first excavations were in Etna Cave in Lincoln County. It was here that he developed the grid coordinate system of excavation and provenance as it is known today. Afterwards, during associations with the Southwest Museum, the Nevada State Park Commission, and the Nevada State Museum, Wheeler worked in Nevada and California at sites such as Lehman Caves, Smith Creek Cave, Borax Lake, Little Lake, Hidden Cave, and Tule Springs. He also excavated Pleistocene fossils, explored caves, and pioneered in the study of split-twig figurines.

Wheeler's first archaeological reconnaissance into the so-called Forty Mile Canyon country, which included the approximate area of the present Nevada Test Site (NTS), took place from February 27 - March 2, 1940. M. R. Harrington had been the "first" archaeologist to journey into Forty Mile Canyon and visit Big George Cave. The only record of this trip is in a letter written by Harrington to Governor James G. Scrugham in 1925. On Wheeler's trip, he was accompanied by a party of four, including a local guide, Roscoe J. Wright - better known as Death Valley Curly. Others included Frank Garaventa, Robert Threlk, and Jack Cooney. The group explored the environs from Cane Spring to Oak Spring Butte. A written record by Wheeler has not been located concerning his second trip into Nye County. However from the date of May 29, 1940 on the back of photographs credited to Wheeler, it is assumed that he was back in the Forty Mile Canyon Country at that time. However, a manuscript by Mary Ream (1940) shows that she accompanied the Wheelers during April 13-19 "and went on out to Tippah [sic.] Springs over the cut-off route." A photograph of the party in Tippah Spring cabin shows Mrs. Georgia Wheeler, and Robert Allen as added members to the second trip. Collections were made at a campsite near Big George Cave, Capt. Jack Cave, Indian Retreat, Sunken Park, Oak Spring Butte, Buckboard Mesa, Basket Cave, Ammonia Tanks, By Fogle area, White Rock Spring, and Tippah Spring.

Though Wheeler never published on the Forty Mile findings,

he did prepare a four-page manuscript on the material from Tippah Spring. Artifacts include Anasazi and brown ware ceramics, projectile points, bifaces, a clay pipe, pendants, arrow shaft smoothers, and glass trade beads. The Desert Research Institute (DRI) borrowed the Wheeler collection from the Nevada State Museum in 1988, and the DRI laboratory personnel analyzed the some 2000 artifacts from the Nevada Test Site area. These data will help in understanding past life ways of central Nevada inhabitants. Table 1. lists the sites that Wheeler visited and collected on the Nevada Test Site. These are sites with the earliest Smithsonian numbers for Nye County.

We may now ask, "What contributions did Wheeler make to NTS and southern Nevada archaeology?" If it were not for Wheeler, many of the artifacts now preserved at the Nevada State Museum would be lost forever. In 1940 the Forty Mile Canyon country was infrequently visited. Its isolation helped preserve the abundant archaeology. However, this isolation was disrupted in January 1951 with construction of NTS test facilities. Many of the thousands of employees took up artifact collecting. During the mid-1960s Frederick Worman tried single-handedly, unsuccessfully, to stop this wanton destruction. Consequently, Wheeler's collections have preserved several of NTS's surface artifacts that otherwise would have disappeared.

Some of Wheelers' artifacts, such as projectile points and ceramics, provide information on spatial patterning of certain Nevada Indian groups. His numerous collection of pendants will provide a future researcher with an interesting study. The examination of Nevada pendants is particularly weak. Also, not to be overlooked is Wheeler's contribution to biological resources. Either he or Nevada State Photographer, Robert Threlk, took



Figure 1: Sidney M. Wheeler (1902 — 1959).

Table 1: Sites within the Nevada Test Site recorded by S.M. Wheeler.

Smithsonian #	Wheeler #/Name	Site Description
26Ny1	12-8/Forty Mile Canyon	Many petroglyphs near Big George Cave. Polished, striated pendant, Pinto and other points, and ceramics were collected.
26Ny2/65	12-9/Forty Mile Canyon	Wheeler made no collection here, but noted an "open site" at crossing of Timber Mtn. Road.
26Ny3/66	12-10/Tippipah Spring	Area of lithics, ceramics, ground stone, pendants, and a clay pipe. Historic stone cabin that Wheeler camped in still standing.
26Ny4/75	12-11/Cane Spring	Historic cabins and a grave characterize this site. A stone from fireplace is engraved "and contains a skull and crossbones. Wheeler made no collection from here but noted "some surface indication of Indian occupation."
26Ny5	12-13/Ammonia Tanks	Many archaic points, ceramics and polished and engraved pendants were collected. A rock shelter has been walled up to make an historic cabin. Red pictographs near tinaja.
26Ny6	12-14/By Fogle Rock	Feature here is a rock carved with inscription "By. Fogle 1863." Also rock alignment (geoglyph). Many archaic points and bifaces and red, grey, and brown ware ceramics, and a promontory peg were collected.
26Ny7/2670	12-15/Capt. Jack Cave	Death Valley Curly told Wheeler about the red and black pictographs, a wickiup, metal primers, and a powder pouch found there. Collections include cordage frag., bifaces, and brown ware ceramics.
26Ny9	2-18/White Rock Spr.	This site covers several acres. Stone cabin used by Wheeler still stands. Several points, bifaces, painted gray ware, and brown ware ceramics were collected.
26Ny10	12-19/Indian Retreat	This is a spectacular secluded area, and includes petroglyphs and pictographs and shelters. Points collected range from Desert Side-notched to Great Basin Stemmed series. Grey ware and brown ware ceramics also collected.
26Ny11	12-20/Sunken Park	This is a pinyon/juniper clad area that is called Big Burn Valley today. Several Archaic points, four pendants (one incised and one with serrated edge) and Puebloan and brown ware ceramics collected.
26Ny12	12-21/Oak Spr. Butte	Wheeler photographed an Indian wickiup here and collected two pendants.
26Ny13	12-22[Buckboard Mesa]	Bifaces and two Pinto points collected.
26Ny213	12-7/Big George Cave	This is a 46-foot deep shelter that Wheeler noted had been "cleaned out by Big George."
(Pro-8)	12-12/Basket Cave	Cordage came from here. Area is designated "Mustard Cliffs", near Indian Retreat.

several landscape photographs of the NTS region in 1940. A repeat photographic study of the NTS environs would provide an understanding of a 50-year biotic change.

Acknowledgments

Thanks are extended to Sue Ann Monteleone for providing a synopsis of the artifacts in the Wheeler collection. Sue Ann and Ron Reno prepared the map showing the extent of Wheeler's collection in the Forty Mile Canyon Country. Don Touhy's generosity allowed the author to examine the Wheeler file at the Nevada State Museum over a number of years that made this presentation possible. The Nevada Historical Society permitted copying of the Wheeler photographs. Linda Adkins provided drafting skills and Carol Bailey made slides of Wheeler's collected artifacts.

Based on a paper presented at the 1988 Great Basin Conference.

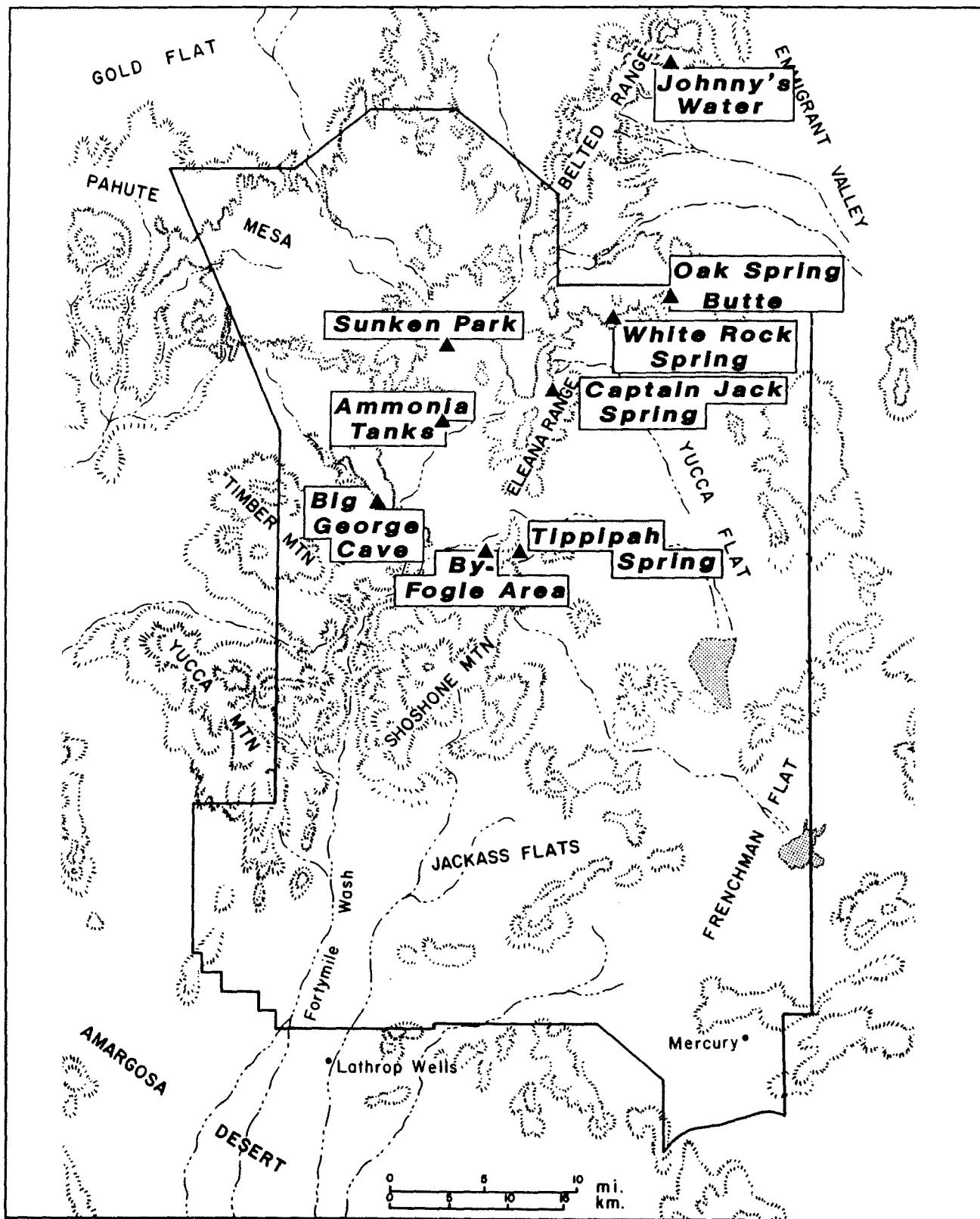


Figure 2: Wheeler's sites in the Nevada Test Site Region

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Production Stage Analysis of Bifaces from Lowe Shelter, Nye County, Nevada.

by William D. Self (*Self & Associates, Orinda, California*)

Lowe Shelter, approximately 32 km southeast of Tonopah, was excavated by the Nevada State Museum in 1970 (Fig. 1; Tuohy 1970; 1978). The site sits on the western edge of Mud Lake, a presently arid pluvial lake basin. The site offers a wide variety of artifacts including floral and faunal remains, olivella shell, coprolites, twined basketry, incised stone, ground stone, flaked stone, and petroglyphs. The temporal placement of the site is late Holocene, perhaps the last 3000 years, judging from the projectile point sequence present. A single radiocarbon date and the presence of a variety of historic artifacts suggest the site has been occupied, at least intermittently, into historic times (Tuohy 1978).

The largest category of artifacts present at the site is flaked stone. It is estimated that 27,000 pieces of debitage were recovered from the 1970 excavation which sampled approximately one-half of the site area (Self 1980; Tuohy 1978). Bifaces constitute the second largest category of artifacts recovered. Bifaces have traditionally been given second billing in most Great Basin archaeological site reports. Methods of description vary widely, and there is no standard format for analysis. Bifaces are often described as a simple "type" (e.g. III, 5a, etc.) based on a single gross morphological attribute such as plan outline (e.g. ovate, biconvex, or lanceolate). Analysis and description in this manner makes inter-site comparison difficult and often produces nearly as many "types" of bifaces as bifaces in a sample.

One of the reasons for the lowly position of bifaces in Great Basin literature may be due to the lack of temporal associations based on observable morphological traits in the manner that projectile points or shell beads often provide. In addition, most typological classification systems for bifaces tend to view the artifacts as finished implements, failing to recognize the production technology responsible for the "crude" or "refined" nature of the specimens.

Although bifaces provide little information on temporal placement, they offer insight into what must have been a signifi-

cant activity for prehistoric populations—stone tool manufacture. Flaked stone, whether in the form of projectile points, bifaces, or debitage, is usually the single largest category of artifacts recovered from Great Basin sites, and the manufacture and utilization of stone tools was undoubtedly crucial for survival.

One alternative to analyzing and describing bifaces in terms of some amorphous "type" is to apply the concept of a trajectory from raw material to finished product. A trajectory begins with a suitable piece of raw material that varies in thickness, width, length, or material type depending on the preconceived end product. The artifact passes through a series of stages during its manufacture, each of which are identifiable by certain criteria, although the function of each stage remains the same—to reduce the artifact to a given, pre-visualized morphological state present in the mind of the manufacturer.

A stage is a point in the manufacturing process (trajectory) of the artifact where one or more major operations occur, culminating in the reduction of the artifact toward its desired goal. Some of the operations that define a stage in the biface reduction sequence from the Lowe Shelter collection are: the removal of cortex; the establishment of a flaking platform; maintenance of the flaking platform; edge regularization; general thinning of the artifact; and production of a hafting element.

In reality, the removal of every flake throughout the trajectory constitutes its own stage. The trajectory itself should be viewed as a continuum with stages placed at arbitrary points where, it is believed, certain manufacturing processes predominantly occur. Stage forms, the artifacts within a trajectory, exhibit considerable variation within each stage as they tend to blend into the continuum from earlier to later forms.

Bifaces need not pass through every stage in the trajectory. A piece may enter the sequence mid-way should it possess the necessary criteria of thickness, width, and length. Some of the cryptocrystalline silicates in the Lowe Shelter area were formed in veins, naturally thin, and when used as core material may begin the trajectory in a secondary stage. Artifacts may be removed from the sequence to perform a selected task should their morphological state, at that point in time, be deemed appropriate. They may be reduced to a given point and removed from the sequence to be worked at a later time. It is also possible that relatively "crude" bifaces may be completed forms, in which case an examination for the presence of use-wear would be warranted.

Should an artifact be broken during manufacture, it may be removed from the trajectory and put to another use or simply discarded. Many of the "tools" from Lowe Shelter are broken stage forms that exhibit use-wear indicative of cutting or scraping tasks.

It should be noted here that the concept of biface reduction sequences and production technology is not new. William Henry Holmes, as early as 1890, applied a stage-concept to the manufacture of bifaces from a quarry site in the eastern United States (Holmes 1890). Researchers including Guy Muto (1970, 1971b, 1976), Bruce Bradley (1975), and Bruce Womack (1977) have made use of these ideas over the past decade.

Stages of biface manufacture evident in the Lowe Shelter collection, and the criteria of production technology that define them are:

Stage I (Fig. 2).

- Removal of cortex, if any;
- Establishment of flaking platforms (through establishment of acute edge angle allowing for more precisely controlled flaking subsequently);
- Discrimination between tip/base is minimal;



Figure 1: Location of Lowe Shelter within the Great Basin

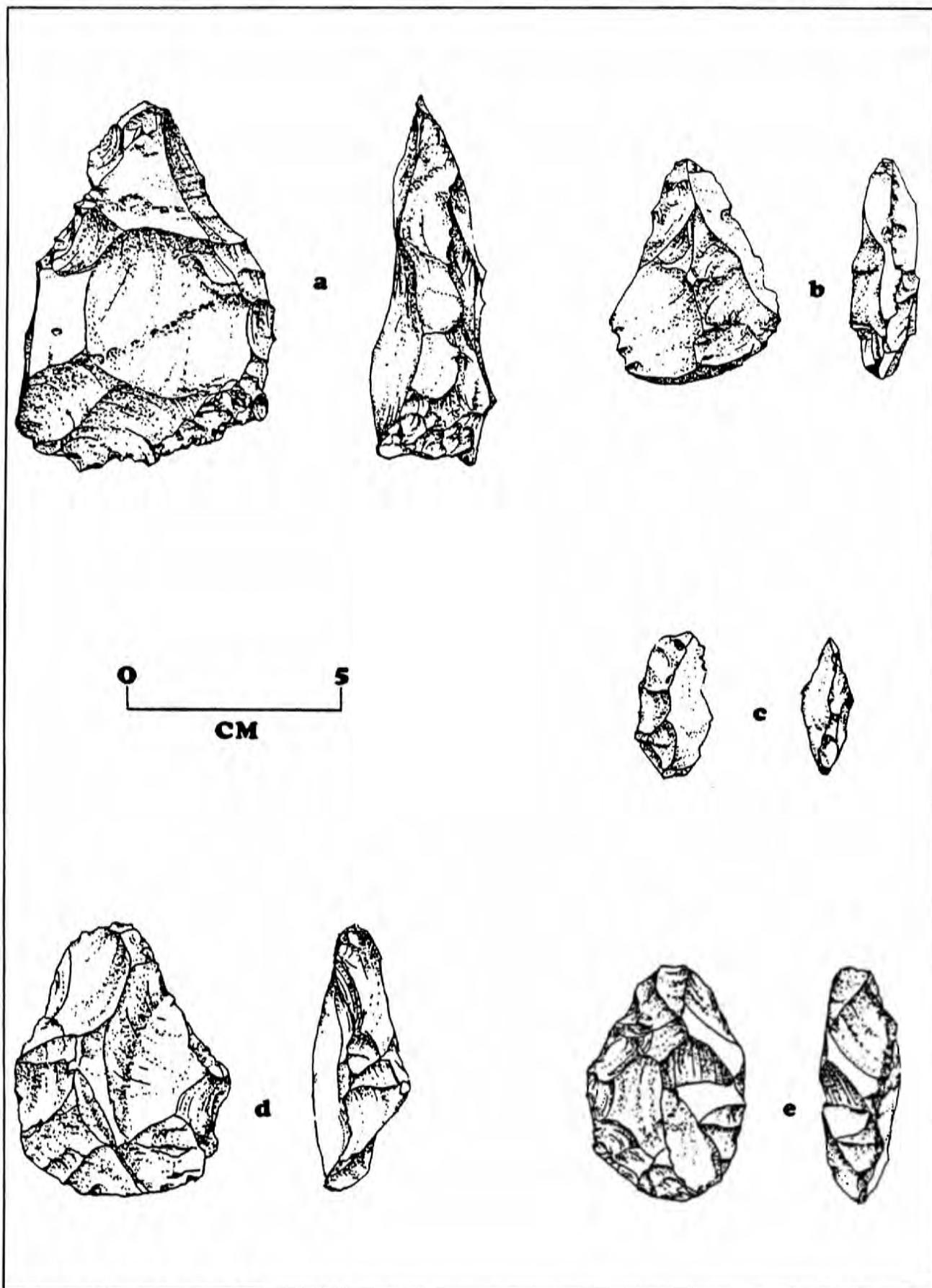


Figure 2: Bifaces. a - e, Stage I.

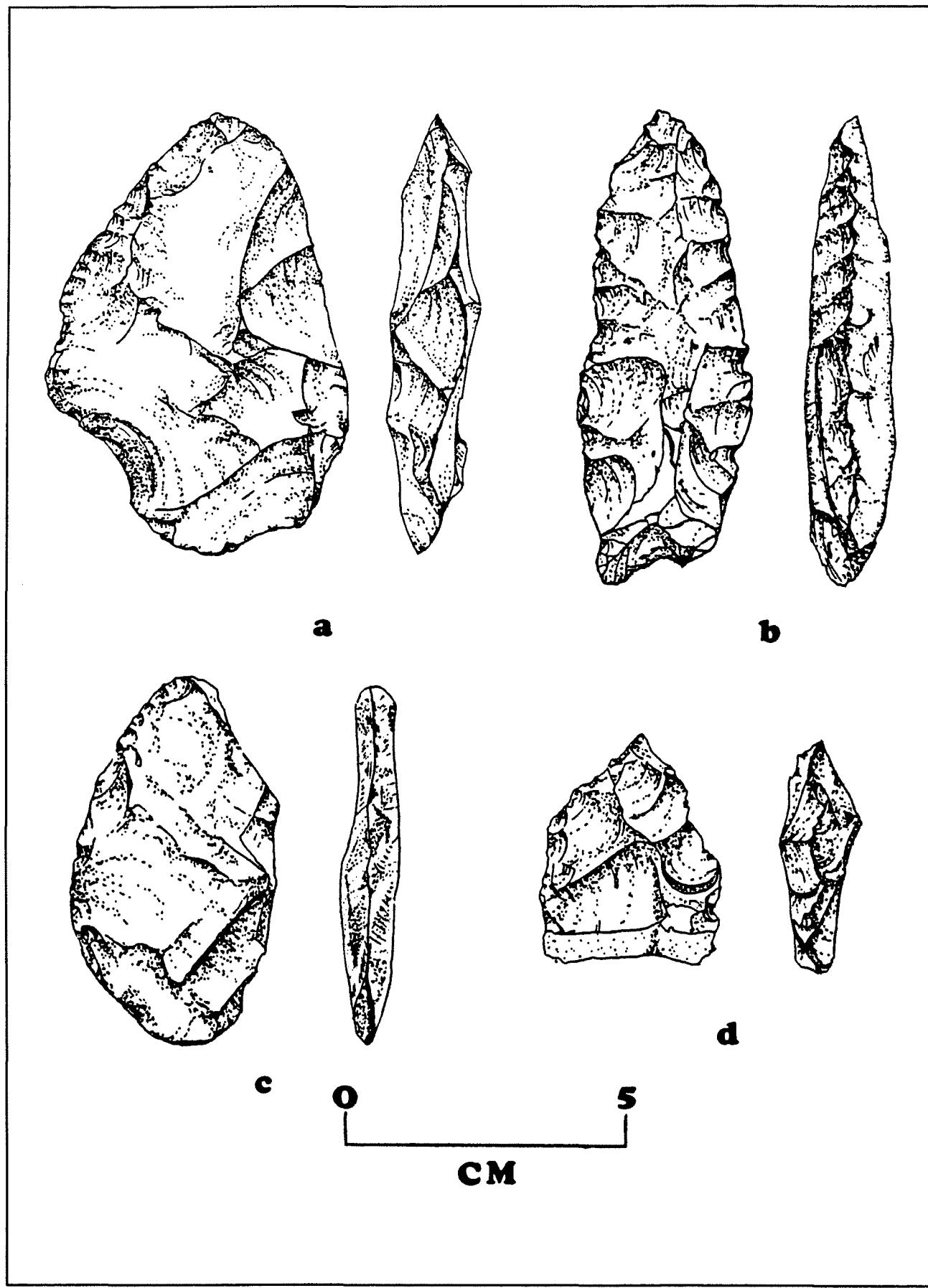


Figure 3: Bifaces. a - d, Stage II.

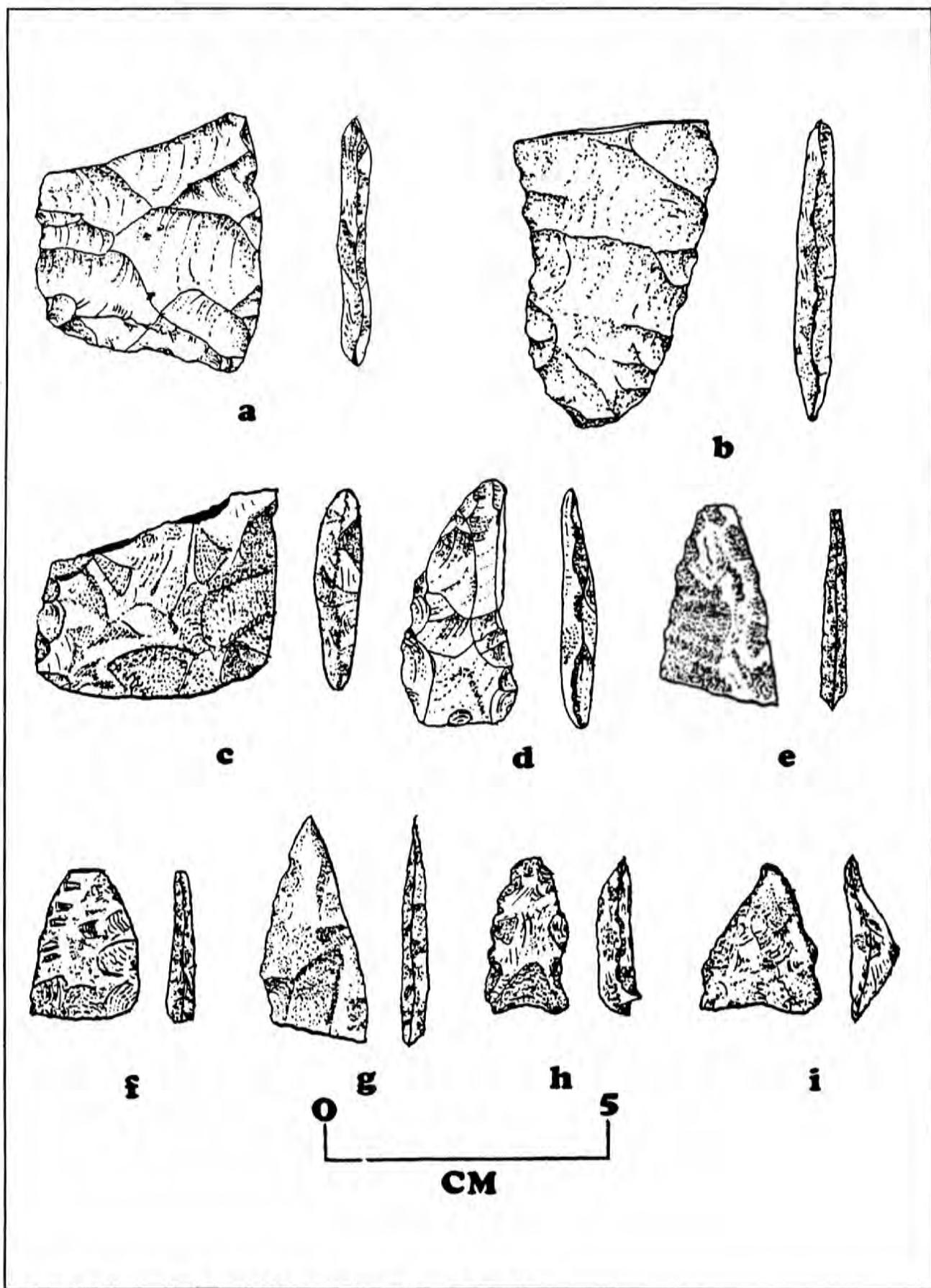


Figure 4: Bifaces. a - d, Stage III; e - g, Stage IV; h - i, Stage V.

- Diamond-shaped cross-section;
- thickness/width ratio of .41 (mean).

Thinning of the artifact is of secondary importance to the establishment of flaking platforms. Cortex is often visible at this stage (57% of the collection exhibit cortex).

Stage II (Fig. 3)

- Maintenance of the flaking platform;
- Further removal of cortex;
- Greater regularization of edges from both side/plan view;
- Definition of tip and base;
- General thinning of the artifact;
- Biconvex cross-section;
- Thickness/width ratio of .32 (mean).

Stage III (Fig. 4. a-d)

- Thinning of the artifact becomes primary task;
- Further platform maintenance (through edge grinding);
- Distinction between percussion/pressure flaking;
- Edges more centered when viewed from the side;
- Generally lenticular cross-section;
- Thickness/width ratio of .21 (mean).

Stage IV (Fig. 4.e-g)

- Production of regular, centered edge from side view;
- Production of regular? symmetrical edge from plan view;
- Establishment of flaking pattern, if desired;
- Predominate use of pressure flaking;
- Thickness/width ratio of .19 (mean).

If no hafting element is desired, the form is complete at this stage. Artifacts at this stage are often referred to as "preforms".

Stage V (Fig. 4.h-i)

- Production of hafting element only (side/basal notching).
- The reduction sequence presented applies to the bifaces from Lowe Shelter, and the criteria used to define each stage apply primarily to this assemblage. Other manufacturing processes from other localities may or may not follow a trajectory similar to this. Differences in the preconceived final form, as well as the properties of available raw material (e.g., cryptocrystalline silicates vs. basalts) may vary considerably, thus altering sequences between areas.

The purposes of this paper are threefold: initially, to discuss a biface manufacturing sequence for the materials from Lowe Shelter. Secondly, to demonstrate that bifaces can be analyzed in terms of empirically-derived data based upon the production technology employed by the manufacturer. This may be a viable alternative to the traditional approach of simple description of the artifacts, and it hopefully comes closer to the cultural manifestations behind the artifact.

Finally, it is suggested that the terms "preform", "blank", and "knife" be limited in their usage or, at least, reevaluated. Although the usage of "knife" may be warranted when substantiated through lithic use-wear analysis, all three terms are become "catch-all" in archaeology whose definitions grow increasingly ambiguous with each new site report. The reasons for this may be due, in part, to the ready availability and application of the term, while the alternative is the formulation of a reduction sequence for an assemblage. It would, perhaps, be more valid to use the phrase "Early Stage Form" or "Late Stage Form" in reference to bifaces, for it takes a minimal amount of analysis to distinguish early from late production stage bifaces and the results are based in empirically-derived research, not mere guesswork.

It is hoped that a reevaluation of biface analysis and description may provide a better understanding of this important aspect

of Great Basin material culture. And, although the concepts presented may not be a panacea to the biface problem, perhaps they will generate much needed re-thinking on the status of bifaces in Great Basin archaeology.

Acknowledgment

I would like to thank Dr. Eugene Hattori for his assistance with the flaked stone analysis on the Lowe Shelter materials, as well as his guidance in the formulation of this production stage analysis.

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Tuohy and the Baja

by Richard H. Brooks and Sheilagh Brooks, (Department of Anthropology [emeritus], University of Nevada, Las Vegas) with Contributions by Lynda Brennan (Harry Reid Center, University of Nevada, Las Vegas) and Hal Turner (Nevada Department of Transportation)

Donald Tuohy and Richard Brooks have known each other since 1950 when they were both undergraduates at San Francisco State University, taking field classes in archaeology from Adan Treganza. Some wild tales could be told about that time period (ask the sheriff of Calaveras County), but we felt the numerous Baja trips would be a better vehicle for allowing insights into the mature Tuohy. In addition to some of the humorous incidents that are cited here, he has published many serious articles about the Baja area and is known and respected for his contributions to Baja archaeology.

For eleven years during the UNLV "mini-terms", between New Years Day and spring semester, Don Tuohy with Richard and Sheilagh Brooks "guided" January field classes into Baja California, as well as a five week summer class. The intention of the classes was to learn about and record the archaeology of Baja California,

and also to experience the culture of the people of Baja. These purposes applied to all members, including professors, since only Tuohy had any prior experience Baja archaeology and lifeways. Tuohy was not exactly our "fearless leader (perhaps fearful is more appropriate)", he shared that dubious honor with the Brooks. Many of the trips were named, usually with reference to some incident or behavior of the participants which had relevance to the members of that field class.

When one thinks of Don Tuohy and Baja California in the same sentence hair cuts and shoe shines are what first come to mind. After crossing the border, Tuohy always went immediately to the barber shop for a cut and a shine, everyone else went to the Panadaria (or for a *cerveza*). He always looked so charming when he was finally collected, suavely patting his paunch and grinning. It appeared that experience was what put him right with the coming Baja event, as he always arrived from Carson City "high on panic and low on gas!" He often drove into Las Vegas about an hour before departure, with just enough time to pack his gear in the field vehicles.

The 1975 field trip (our first) was memorable because we had five vehicles (3 of them decrepit UNLV 4-wheel drive trucks), the Brooks' Jeep Wagoner and Dr. Charlie Evans' Blazer. If it wasn't Tuohy's truck wandering off, it was our "guide" (a graduate



student from Colorado) becoming lost. It really was Tuohy who knew about Baja, but he had a propensity for getting lost (with or without some of the vehicles following him). In all probability he turned off his hearing aid and did just what he had already intended to do. This trait became more and more apparent as Baja trips continued, although on the 1975 Baja Tuohy outdid himself. First he took the wrong road in the border town just as we entered Mexico. We would have lost him, if one of the "Little Green Men" (men in small green vehicles from the Mexican Tourist Department who helped tourists on the Mexican highways) had not seen where he went and told us.

The most spectacular "lostness" on the 1975 trip was when Tuohy and the other large vehicle did not notice the three smaller vehicles turning off the main road (which went to Loreto). We were (as previously planned by the group) going up to San Javier to visit the oldest church in Baja. No one noticed Tuohy was missing, along with the other vehicle, until we were too far along. So, we went on and saw the church about dusk. Then took off to drive to Loreto. San Javier is in the mountains and although an easy grade up from the south, the dirt road down a canyon to the east coast at Loreto is another matter. Narrow, winding and precipitous, it only allowed the smallest vehicles to make it around the curves with all four wheels on the road (or at least the edge of the road). Fortunately we were in the three smallest vehicles and it was night, so the horrors of the road were not as visible. When we reached the bottom of the canyon we could see lights and vehicles in a wide area beside the turn-off to Loreto. There was our lost Baja crew, under the leadership of Tuohy! According to two of the female students (one with Tuohy and one with the San Javier trip) they were psychic and "knew" where the road would come out of the mountains. More reasonable, but not as intriguing, was the fact that Tuohy knew the Baja and where the San Javier road met the Loreto highway. What no one knew was that the sign at the Loreto end warned the dirt road to San Javier was impassable due to rain damage. Those of us who traversed it, believed that firmly!

Summer 1975 we took a five week trip to survey the area along the coast north and south of San Felipe, and also drive into the mountains and visit the Comandu area. The east side of Baja in the summer is not just hot, but humid, with mosquitos and gnats. Tuohy couldn't take off the five weeks from the Nevada State Museum, so he came down by Baja bus to join us and go to Comandu. When we picked him up in Mulege, he was almost unrecognizable after being three days on a Baja bus - but a swim in the gulf and a beer (or three) rapidly restored him. That was the first, last and only summer trip we took.

Tuohy was the heart of these field trips. The few occasions when he could not stay the whole time, everyone missed him and was continually aware of his absence. The one thing we did for him during that summer Baja trip (when he was only with us about two weeks) was to drive up to Comandu - actually two villages, San Miguel de Comandu and San Jose de Comandu. Tuohy worked with William Massey excavating a cave site in this area and needed charcoal for a radiocarbon date for his M.A. thesis. Not only was the Comandu area as hot and humid as the Gulf coast, but there were lots of mosquitos and gnats (which one student insisted crawled out of the cracks in the soil). We camped the night and the next morning, Richard, Tuohy and the crew took off for the site to collect radiocarbon samples from the cave.

On our walk to the cave site a memorable experience was passing the carcass of a dead cow apparently in the extreme stages of putrefaction. Tuohy sort of laughed and said "This is Baja, just hold your breath and get by as fast as possible!" Unbelievably when

we returned in the late afternoon, a farmer had hauled the carcass off, and again Don informed us that some of the parts might still be useful.

We walked over a pass and around a lake to reach the valley in which the cave was located, since the usual road was closed because of the rains. Once we were on the other side of the lake, we discovered that Tuohy had forgotten where the cave was, except on that side of the valley. We split up into two groups and some of us went to the south while the other group, with Tuohy, went to the north. We found ourselves amidst hundreds of goats in a canyon; where even a few goats sounded like a herd. We decided the cave wasn't in that neck of the woods, so covered with dust and goat poop, we backtracked to the north and found Tuohy's group at the cave. There was a problem; Tuohy had forgotten that the "cave" was a real cave and no one had a flashlight. After some debate as to a reasonable course of action, some of us agreed to jump into a trench at the back of the cave, hoping we wouldn't get eaten by anything too poisonous. Arnie Turner, meanwhile, stood at the edge of the trench and did her best to reflect light from the mouth of the cave off an aluminum canteen into the trench.

As a result of the summer Comandu trip, Tuohy got his radiocarbon date, completed his M.A., and wrote the longest (2 volumes) thesis submitted to the UNLV Anthropology Department. The day it arrived, before it was delivered to the Graduate Office, there was a fire alarm in our building. The only thing the secretaries grabbed from the office (except for their purses) in their rush to leave, was Tuohy's thesis!

The 1976 "Fluke You" trip was named after the appalling puns that developed when the crew went whale watching to Scammons Lagoon. That name was appropriate also with regard to the events that occurred that year. The UNLV Motor Pool assigned Vehicle #1669, a Dodge power wagon, to us and Tuohy drove it. We crossed the border at Tecate, and from there went to Ensenada through the various coast ranges. Joe King (a botany graduate student and driver of a vehicle) was ahead of Tuohy, but Richard drove behind him, worried about the sound of 1669 as it groaned its way up and down the hills. Finally, about a half mile from the Tijuana/Ensenada highway 1669 simply collapsed and fell apart. Some vital bolt had sheared, the axle broke and everything ground to a screeching halt. We all helped collect the bits of hot metal that were scattered along the highway.

The Brookses and Joe went into Ensenada to telephone UNLV collect (no phone charge cards then) at the Central Telephone Office. Motor Pool had just received a new nine passenger vehicle, but without license plates as yet. Reuben (head of Motor Pool) would see what he could do, and we were to phone him at his home that evening. On our way back to 1669 we stopped at two motels and chose the cheaper one, San Jose (\$20 for two rooms and a kitchen). Richard managed to get a tow line on 1669 and bring it to the San Jose motel. Here in the kitchen we settled down and began our preparations for dinner. Joe went back to town and phoned Reuben, they would bring down the new vehicle leaving early and expected to reach us for dinner (and other refreshments) the next day.

When Reuben, his assistant and the new truck arrived, their only complaint about 1669 was that they were sad that it had not lasted until we were further south. Reuben told us he knew 1669 would break down and they were planning on driving into Baja to drag it back, after they had done a little fishing and relaxing. That explained why they always gave us the old clunkers for our Baja trips. They enjoyed our stew and spent time after dinner playing cards, until some of our students found out the motel owner rented

rooms by the hour. A man and woman would drive in ring a bell, the owner let down a basket from his second story window, the man would put money in the basket in exchange for a key and the couple would go off to a room. The students timed the visits - more or less an hour but they never found out the cost per hour. It was a hilarious evening.

The next day 1669 was ignominiously hauled off, and we headed south with Tuohy driving the new vehicle, using 1669's plates and hoping the police or border patrol would not notice the discrepancy - they didn't! That was the only time we ever had a new vehicle for the Baja.

In commemoration of the event Lynda Blair put a huge patch over the hole in the middle of the front of the old sweater Sheilagh wore to keep warm. Within the patch Lynda embroidered "Recuerdo 1669", and Sheilagh wore that sweater during the 1976 and all subsequent Baja trips.

Tuohy and Richard were standing beside their trucks with their crews on one field trip, deciding whether a particular locale would be a good camp area, when Joe King and his vehicle crew decided to introduce a new element into the evening camp circle. Their truck went sailing by us, but someone ran out in the road and came back to say that all was okay, Joe had stopped and was turning around. Shortly afterwards his vehicle came towards us (we were all standing by the roadside) and, except for the driver, the crew members were bent over with their jeans down mooning us! Needless to say there were a few more return moon-type episodes. The most famous was when three men (who shall remain nameless) stripped down and streaked the whole crew who were sitting around the fire.

One January we decided the class would survey the Vizcaino Peninsula for sites on the return trip from Bahia de la Concepcion (south of Mulege) where it had rained several days and some nights. We named this the "Quigley Hole Trip" as the crew had dug up lots of clams and some Canadians told us the Northwest Coast Indians called clam holes Quigley Holes. So after shopping at Santa Rosalia for supplies, we headed for the Vizcaino where there are a few small fishing or mining villages, but no towns. Tuohy cheerfully said according to something he had read: "It never rains on the Vizcaino Peninsula!"

Even when it clouded up one night as we were camped near an arroyo, we did not worry, because of Tuohy's pronouncement that rain does not occur here. That is until sometime in the middle of the night when thunder was heard and the rain began, despite Tuohy's quote. In pitch dark, with a flashlight, we set up our new domed tent and finally, rather wet, crawled inside. The rain continued the rest of the night. At daylight we emerged in the rain and strolled into camp, which on one edge of an arroyo. We had the sense not to put the stove, etc. in the arroyo. Someone had built a little fire and put the big black water pot on the fire. We were all standing there hovering over the fire warming our hands around our cups of coffee, tea or chocolate wondering how long the rain would last. Suddenly across the arroyo appeared a vision: Lynda Blair in a bright yellow raincoat, yellow boots, a yellow rain hat and yellow umbrella, with a bottle of brandy. What a sight for sore eyes and cold insides! We all toasted Tuohy's statement about no rain on the Vizcaino.

Tuohy was good with troublesome students, in addition to his other qualities. The field trip towards the last of the 12 times we ran the Baja, was probably one of the worst in terms of students. With reference to that we named it the "Watch for the Jacks Trip!" We had three young men along, two of whom had never done kitchen duty and didn't intend to begin. By the end of the trip they had learned to peel potatoes (none too well), wash dishes (only near a town where we could replenish the amount of water they wasted) and do other necessary camp chores (under duress). On this one occasion we had bought a kind of roast beef in Mulege at a butcher shop we trusted for their good meat. It was prepared with gravy and potatoes in the big pot. As supper began, the three senior members of the "expedition" were talking to one side with a young woman student about her photographic interests. By the time we came back to the fire, we opened the pot and there were the gravy and potatoes, but no meat. We shared what was left, cleaned up and went to bed. The next morning drinking coffee around the fire while the three were talking about the good dinner, Tuohy said quietly "No meat with the dinner, but the potatoes and gravy were good!" Without being reprimanded the young men did the dishes for the next few nights. Tuohy's favorite beach camp, Nopolo, was just south of Loreto. The beach is far enough down the Gulf that



there are nice waves. A cobblestone road used to go from the highway around and along the beach. There were umbrellas woven from reeds or grasses permanently in-place, and we all liked to stay there. Tuohy used to joke about himself flying on that strip of clean sand on the edge of the waves, "like a beached whale". During one extremely rainy trip Tuohy wanted to camp under the umbrellas and was about to stay there until he realized there was no room for all of us and our gear - especially as it began raining harder. So we copped out and stayed in a motel in Loreto.

Tuohy's comment during the "Harem Trip" when we had all women for crew - the only men were Tuohy and Richard - with regard to the Boojum and Boulders campsite was: "It is the only place I know where the whole country gives you the finger!" This was in 1981 and the oddly-shaped boojum trees were flourishing that year, and Tuohy was making reference to their tall, slender shapes which we could see in every direction for miles.

One night during the Harem trip we all got slightly, we mean a lot, silly. It was raining, and Don was given shelter in one of the larger tents. After only giggling all promptly fell asleep as soon as their heads hit the pillows. Those outside knew the exact order in which they slumbered, as they started to snore, one by one. The noise must have been incredible, someone finally got Tuohy up and moved down the gully, to spread out the decibels. So much for the "menage à many"; it turned out to be a sleeper.

Cabo San Lucas was a paradise except in that year of the Harem when everyone caught the flu. Bodies were lined on the sand to take on therapeutic sun rays and to get well enough to travel again. No one moved, just a few groans of anguish. Tuohy was well and reading a novel in a lounge chair. All of a sudden he sprang into action. Standing over the girls he began to read a gibberish eulogy from a make believe missal. Preaching and swaying over the almost dead, he finally ended his remarks by proclaiming they were anthropologists and thus probably atheists. With that he kicked dirt over all and left them to their misery.

Tortillas go bad after a few days in the tropics of southern Baja. One day when shopping Sheilagh bought fresh boleos (fresh "French" rolls) for the larder. The crew had been eating stale tortillas for several days, as they had been in an area with no stores. At the evening camp she FIRMLY announced that the tortillas must be finished before breaking out the fresh rolls. It was too much! The tortillas were turning green and people were sick of them anyway. Tuohy devised a plan, a wicked, wasteful plan. He would keep Sheilagh and Richard busy while everyone pretended to eat the last of the nasty things. With his eye on the students, he artfully maneuvered the Brooks around, keeping their attention on him and their backs to the group, while the tortillas sailed like frisbees into the fire - one by one! After he was sure they were 'all gone,' he innocently asked if there were any boleos. Everyone loved Tuohy, he was kind of like the Virgin Intercessor, bridging the gap between the teachers and students in troubled waters.

Those Baja trips would not have occurred were it not for Tuohy and we feel that every student who came with us during those twelve trips learned from him and grew to love him. It was his know-how that enabled us to survive the first trip and himself that filled the trucks with students for the rest of the trips. Many a student joined us more than once just for the pleasure being crew in Tuohy's vehicle - even old 1669, which had actually made one prior trip to the Baja before its famous death.

One of Tuohy's favorite sayings, for no reason known to those who heard him, was "grapenuts is a disease!" This phrase he, among others, chanted frequently around the evening fire, especially as the hour grew later and the liquid refreshments dropped

lower in the containers. Next time you are sitting around a campfire with Don Tuohy you might mention this phrase at an appropriate moment and let us know his reaction.



CRM, Nevada, Don Tuohy and Me: Musings and Mutterings.

By Robert York (U.S. Office of Navajo and Hopi
Relocation, Flagstaff, Arizona.)

It seems like a long time ago; it seems like yesterday. Late in 1970, I wandered into Las Vegas from Arizona to accept my first full-time archaeology position with Richard Brooks at the UNLV Museum on Maryland Parkway. I was employed to work primarily on a power line survey and a survey of the Spring Mountain Range for the U.S. Forest Service. The "Research Design" for those contracts was to locate archaeological and historic properties, record them, and recommend protection measures to the land managers. Judgments about significance of located properties were substantially ours — the field archaeologists. Decisions about acting on our recommendations were the land managers. Those decisions were often influenced by input from concerned citizens; sometimes archaeologists were counted among those citizens.

Secondarily (secretly, maybe primarily), we trusted that our work would be helpful to colleagues in archaeology, at least as a contribution to future data syntheses. Sounds simple does it not? It was. Sometimes our work did not live up to even these modest expectations, but often it did. Additionally, it was for the most part interesting, fun, and rewarding.

In the early 1970s, these projects passed for what is referred to now as cultural resources management or simply CRM. Now, of course, they would not. CRM in the 1990s has come a long way; it is a much larger, well-funded, and an enlightened, goal oriented, professionally managed program of archaeological and historic sites protection and research. Perhaps even more interesting, CRM is fun and rewarding for its practitioners. Well, maybe just rewarding.

It's sure different now. We have many more laws (and advocate even more), a lot more dollars (Depending on whose figures you use, the federal government alone spends over \$100 million a year on CRM.) and a lot more archaeologists (plus ethnographers, ethno-historians, historians, historical architects, etc.) employed by contract firms, federal agencies, SHPO offices, Indian tribes, you name it (even the Coast Guard). These individuals write reports (if punching up boiler plate on a word processor constitutes report writing) or, for many of us employed by SHPOs and federal agencies, review and criticize everything, and I mean everything. Archaeology or rather CRM (is "archaeology" practiced?) has now achieved parity with working at a car wash for job satisfaction, but it's a pay check, and that's something (certainly rewarding or maybe lucrative for some of us, but was that our goal?).

Well, if CRM is not a particularly satisfying career, at least archaeological site protection and management has significantly improved. Now we ensure that even the most trivial cow fence and timber sale are assessed for cultural resources, even if located in terrain demonstrably shown to have no potential for containing cultural properties (many have actually been assessed several times by archaeologists for other projects, but not this project, so it's done once again. More than that, we insist that Ph.D.s and M.A.s (I only add M.A.s because I'm one of them.) do this work (...And I mean do it, not just design and supervise.), though the work often demands little skill and generates great gobs of paper to document the efforts — even if nothing is discovered. Then the new breed, the Bureaucrat Archaeologist or BA (a.k.a. Cultural Resources Specialist; I'm one of them too.) swings into action and ensures the

report contains the requisite boiler plate regurgitating the usual litany of irrelevant theory and environmental data and/or weighing less than three pounds. The BA then requires at least two rewrites, even if nothing was found — of course, if they found nothing they're crooks or incompetent, right? What's possibly more interesting, some may say absurd, is that the amazingly critical BA often does not have the credentials or the expertise to conduct such reviews. I wager that any reader of this paper who has conducted CRM work directly or indirectly for a U.S. Government agency has encountered this BA. SOPA, stricter regulations (36CFR296, etc.), and college CRM curriculum were supposed to correct this problem. But from my vantage point, whether they have a Ph.D. or M.A., the problem of the incompetent BA is more pervasive than ever.

Well, if CRM is not much of a job and management of resources has not improved and arguably is worse than in the 1970s, at least CRM has benefited archaeology as a scientific discipline. We now know so much more about America's past than we did in the 1970s right? The U.S. citizen has really benefited. She/he sleeps a lot easier knowing that archaeologists are spending their tax dollars wisely (our tax dollars, my tax dollars!) making sure that not even one isolated stone flake or potsherd is harmed by a fence, a criminal logger or cowboy. (Really no need to specify occupations, everyone who is not an archaeologist is a looter to archaeologists.) Because there is only so much money to go around, major sites, rock shelters, ruins, rock art panels, etc., are sacrificed to vandalism, neglect without being studied, because they are not threatened with the dreaded CRM "undertaking" (36CFR800). Who says that archaeologists have no perspective or sense of priorities; perhaps Don Tuohy?

I met Don Tuohy at an NAA meeting in 1971. Don turned out to be a major influence in my archaeology career and my life. In 1972, I had the good luck to be hired by Don as staff archaeologist at the Nevada State Museum in Carson City. I still view the too brief period of time (1972-1974) that I worked for Don as the most enjoyable years that I have spent to date in archaeology. I am more fearful as the years roll by and the CRM bureaucracy becomes increasingly oppressive that the "to date" will become "ever".

Don early-on advocated and worked for changes in law and especially attitudes of federal land managers to provide more protection for Nevada's archaeological sites, especially from the depredations of looters and vandals. He knew that these activities needed to be aggressively checked if the archaeological data base was not to be lost to future archaeological inquiry. But he was also concerned, to say the least, that it would be exceedingly difficult to accomplish this without giving birth to an onerous bureaucracy that would work more to stifle legitimate archaeological research than accomplish the intended goal(s) of historic preservation laws.

Is this what has happened? Obviously, I am arguing that the answer is yes. How did this happen? Some would say it's the price we pay for more law; it was inevitable, i.e., that's what more law begets, a burdensome bureaucracy. My view is that this situation was not inevitable. It came about because yes, archaeologists all shared with Don the desire to strengthen and maybe, more importantly, enforce laws to provide for better protection of archaeological resources. But beyond achieving site protection, the profession was divided.

Many archaeologists had loftier goals; they envisioned a national program that would go far beyond protection to one of management of the resource base, i.e., "CRM". Others, amongst whom we can probably count Don, were not so sure the CRM

concept was one that would serve archaeology well. From their perspective such concepts sound good, but they tend to create a new class of individuals, that I have labeled BAs who, in fact, wind up controlling the program. When questioned, however, BAs will usually protest they do not. They are simply enforcing laws and regulations, or they are just carrying out orders of uniformed supervisors—but why are those supervisors uninformed? Who do they rely on for their information or misinformation?

This (i.e., program control by bureaucrats) in itself is not inherently evil (from my perspective, probably not Don's). The problem is, who controls or rather, as I do not want to imply BAs are puppets, who or what influences the BA (and remember I count myself as one)? Well, like anyone, many things, e.g., academic training, parents, biology, and friends are responsible. I would say though, that the current corps of BAs carries a load of baggage with them from the New Archaeology, again not inherently bad. Some of these BAs, however, foster New Archaeology polemics in a rather thinly disguised agenda to purge supposed non-processualists and other non-right thinkers from the ranks of archaeology. This is bad. It is not the BAs' job to ensure that only those archaeological studies which support a particular trend in the profession are allowed to be conducted. I maintain that such a position is, in fact, anti-science and anti-intellectual. Which is ironic when you consider what the New Archaeology was about.

I suggest the problem in CRM lies with the people who are charged with interpretation and enforcement of laws, particularly the BA, rather than with the laws themselves. At the federal level, I see little wrong with the laws and regulations we deal with, except there are now more than enough to protect virtually any interest in archaeology, from academic to Native American religious interests. However, we do need some consolidation and clarification, as in many cases laws have become redundant, confusing and contradictory.

Dealing with individual BAs is not easy. I do not believe anyone yet has come up with a system to guarantee pervasive competence in any profession, and I certainly will not be able to supply the answer here. But I do think we should have some understanding now of what doesn't work. We should understand, by now, that competence cannot be legislated or ordered, we've tried that, unfortunately many are still pursuing this route. I am convinced that too much law and regulation only handicaps the able person - limits their ability to think, problem solve - and does nothing to improve the abilities of the marginal to grossly incompetent person. In fact, it usually gives the latter individual more ways to accomplish what they are inclined to do; at best nothing—at worst, hinder the ability of the capable to succeed.

So what can be done? Possibly nothing, maybe it just all "works out" and we just have to adapt, resign ourselves to a more bureaucratic way of conducting archaeology or find another occupation. To some extent I accept the truth of this, but I also know there is something wrong with this attitude, at some point "adapt" becomes "sell out". Has this happened? I know that it has when I attend more and more meetings with archaeologists and compliance with Section 106 (36CFR800) is discussed by all as if it was the goal of CRM and archaeology.

If something can or should be done to get back on track toward a "goal oriented, professionally managed program of archaeological and historical sites protection and research—", it needs to happen in the area of training. Specifically, we need to better prepare motivated and qualified college and university students for CRM oriented careers. From my observations this is still not happening. If CRM courses are offered at all, they are

usually taught by unknowledgeable faculty. This needs to change. A generation of CRM practitioners is now available to colleges and universities; academia needs to actively seek these individuals to instruct CRM courses and be involved in designing CRM curriculum.

Training, most importantly both in an academic setting and on the job, must instill a new (but really old) ethic and attitude of public service in current and future BAs. Their role is not to dictate how "legitimate" (and legitimate simply means lawful and ethical not philosophical agreement with a particular use or research approach) archaeology is conducted. Rather they (we) are employed to protect and serve (apologies, kind of, to the LAPD).

When I started my career as a federal archaeologist with the Nevada BLM in 1974, a career BLM employee observed that there are two types of bureaucrats, bad ones and good ones (again, I have an idea Don and many others might argue there are only bad ones). The bad ones are those that when asked for help find fifty different reasons, all supported by laws and regulations, for saying "no way". The good ones, can find the one or two ways, equally supported by laws and regulations, for saying "I'll find a way". If archaeology is to ever again return to a worthwhile and rewarding profession, then we are going to need many more good ones. I think Don would agree.



A View from the Backdirt

by Eugene M. Hattori (*Nevada State Historic Preservation Office*) and Allen G. Pastron (*Archeo-Tec, Oakland, California*)

Introduction

Don Tuohy's opinions concerning the impact that state and federal agencies have had upon the field of archaeology are well known. Although from a generation after Don's, we too share much of his frustration upon examining the gray literature (to which we contributed) and dealing with the bureaucracy (which one of us [Hattori] is currently a player). Is there a basis for this dissatisfaction, and, if so, are there any solutions? Our paper is an admittedly biased review on the state of archaeology from our perspectives as graduates of western university programs in the 1960s and 1970s. The dissatisfaction with Section 106 (National Historic Preservation Act of 1966, as amended [NHPA]) compliance-driven archaeology is great with those who of us who witnessed the changes in contract or cultural resource management (CRM) archaeology beginning in the 1970s.

The Old Guard

Don Tuohy represents a generation of pre-Section 106 archaeologists who supported passage of the NHPA's provisions concerning protection of archaeological sites. Many of these same archaeologists were also instrumental in the establishment of State Historic Preservation Offices (SHPO) by acting as SHPO advisors or board members. Don and his cohorts viewed the "contract archaeology" projects spawned by the early days of this law as a means of protecting cultural resources and as an opportunity to conduct project sponsored research.

In the early-days of the NHPA, archaeological sites were identified through cultural resource surveys and a few of the "significant" sites were excavated. Site reports were oriented toward the principal research domains of the day... culture history and site and artifact descriptions. Deadlines were flexible, so many reports were never completed; unstandardized survey methods allowed sites to go undiscovered, and site documentation was often poor. But, a surprising number of the early survey and excavation projects were reported in professional journals and monographs. Although most of these reports are largely descriptive with conclusions related to questions of culture history, they were germane to the current research of the times. Universities and academic archaeologists were key players in conducting many of these early CRM projects.

Section 106 and Agency Archaeologists

Section 106 of the NHPA charges federal agencies with considering project effects on historic properties (buildings, structures, archaeological sites, etc. listed on or eligible for listing on the National Register of Historic Places). This single paragraph in the NHPA forms the basis for modern contract archaeology, with the more specific regulations contained in 36 CFR Part 800 entitled "Protection of Historic Properties." As few agencies had archaeologists on staff when this process began, agencies typically required that the project proponent hire an archaeological consultant (contract archaeologist) to conduct the work on behalf of the agency. The agency or contract archaeologist would then submit the work to the SHPO for review and comment. The SHPO's position was created by the NHPA (Section 101) to perform this review of agency documents as well as a number of other functions.

Many federal agencies and SHPO offices were initially slow to incorporate archaeologists into their staffs, so the consulting (contract) archaeologists were more-or-less free to conduct the

projects as they saw fit. Although post-dating the National Historic Preservation Act of 1966, the National Environmental Policy Act of 1969 (NEPA) environmental review process and the 1972 amendments to the NHPA were instrumental in adding archaeologists to agency staffs. The Bureau of Land Management (BLM) in Nevada, for example, did not hire an archaeologist until 1974. Through the 1970s and 1980s the various agencies hired increasing numbers of professional archaeologists to help ensure their agency's compliance with Section 106, and to review the work of consulting archaeologists.

The Nevada State Historic Preservation Office (SHPO) incorporated an archaeologist into their staff in 1977. This individual (presently Hattori) is responsible for review of agency compliance with Section 106, including reviewing contract archaeological reports submitted on behalf of the permitting federal agency and reviewing archaeological reports produced by the agency archaeologists. In other words, the SHPO archaeologist provides oversight review for the federal agency archaeologist's review of contract reports. The SHPO archaeologist also has increased responsibility when reviewing reports generated by federal agency archaeologists or for agencies, such as DOE, that have no state or regional archaeologists. The SHPO's office, in turn, is reviewed every two years by the National Park Service (NPS). The NPS ensures that SHPO reviews were completed within the 15 and 30 day review periods, site reports and site forms meet Secretary of the Interior standards, correspondence fulfills SHPO obligations to the Section 106 process, and project files are in order. SHPOs do not have independent authority to stop or prevent projects, although their comments may result in an agency delaying or modifying a project pending SHPO concurrence. Furthermore, the Advisory Council on Historic Preservation (ACHP) can review determinations of project effect in lieu of the SHPO and the Keeper of the (National) Register can make determinations of eligibility in lieu of SHPO concurrence with agency determinations.

Increasing Agency Involvement in Section 106

In the 1970s and early 1980s few site reports were rejected by the SHPO and federal agencies for any reason. Now, agencies are more exacting in their review of reports with the emphasis placed on fulfillment of their interpretations of the Section 106 regulations (36 CFR part 800). As a result, there is presently more consistency in report format; sites are recorded on the standardized IMACS site forms (Nevada), despite the collapse of IMACS, and agency survey standards are established for identification of historic properties within project areas. Yet, substantive comments are infrequently offered because of time constraints imposed by burdensome work loads and lack of background to critique the work.

Contract Archaeology in the 1980s and 1990s

Contract archaeology is now a formal business line with some firms exceeding the one million dollar "small business" threshold. With the oil and gold booms of the 1980s and 1990s, there has been a tremendous increase in contract archaeological projects in Nevada. In California, a state environmental law, population growth, and the economic boom of the 1980s also resulted in an increase in contract archaeology. At first glance, this would seem to hold promise for great advances in archaeology. The proliferation of the ensuing unpublished site reports ("gray literature") produced from these projects, however, has largely been at the expense of scholarship. There are too few substantive contributions to California and Great Basin prehistory and history, given

the large number of investigations that occurred. In fiscal year 1991-1992, for example, well over 500 archaeological reports were submitted for review to the Nevada SHPO, few of these reports, however, were disseminated to the profession or will ever make their way to publication. Likewise, few professionals would realize any benefit from reading the vast majority of these reports.

Part of the reason that the vast majority of CRM projects produce laundry lists of artifacts and basic site descriptions is that these archaeological reports are interpreted as complying with only the inventory/identification phase of the Section 106 process. Sites are located and then evaluated for their National Register eligibility. None of these newly discovered sites are subjected to treatment (i.e. excavation) during this phase, although limited testing ("probing") is permitted to judge National Register eligibility. The concept of probing to determine National Register eligibility is itself fraught with conceptual problems. Technically, little effort need be expended in explicating the sites, as this is the stuff of treatment, usually data recovery for archaeological sites. Many of the archaeological survey reports completely lack any discussions of the findings beyond bare site and artifact description.

Most archaeological investigations end with site inventory, either because no eligible sites are found ("No Properties") or because projects can avoid historic properties ("No Effect"). In both cases, the project can proceed, thus completing the Section 106 compliance process. When an eligible property will be affected by a project, then the NHPA regulations consider this to be an adverse "Effect" which requires consultation with the Advisory Council on Historic Preservation. In Nevada, the BLM has a programmatic agreement with the SHPO and Advisory Council that bypasses the Advisory Council's review in instances where the agency determines that the impact to sites will not adversely affect its National Register qualities, a situation described as "No Adverse Effect". Sites deemed not eligible for the National Register are not afforded any special consideration under the NHPA. The BLM and other agencies, however, will provide even these sites protection when feasible.

Contract archaeologists naturally want their report to pass agency review with as little agency involvement as possible. Not only do they feel an obligation to their client to facilitate their project, but they want to proceed on with the next project phase or the next project. Most agency comments are viewed as misfocused on minutia, with little attention paid to the contractors' contributions or to the shortcomings of their competitors' work. In fact, there is a widespread belief that agency approval of substandard reports brings down the overall quality of reports. Furthermore, if one is to compete effectively in the market, a wise contractor will provide only enough information to get by in order to prevent unnecessary rewrites at the behest of the agencies.

Many agency archaeologists are dissatisfied with the contract reports, as well as being burdened with their own Section 106 compliance projects, although that is the genesis of their positions. In fact, agency archaeologists are also guilty of adding their share to the gray literature.

New Reviewers

In an interesting twist, an increasing number of private project proponents are scrutinizing CRM reports and information generated through the Section 106 process. They are realizing that their expenditures result in products of questionable value. In some instances, mining companies have voluntarily contacted agencies about shortcomings in CRM reports. The demands of this group include competent identification and plotting of historic properties and understandable interpretation of the artifacts and features.

Likewise, Native American groups are becoming increasingly involved in the treatment of their ancestors' properties. Whether or not they can demonstrate their lineage to archaeologists' satisfaction, is a moot point. Their control over Native American resources is continually increasing. Opportunities to conduct studies on prehistoric and historic Native American archaeological sites will continue to decline into the foreseeable future.

The general public is increasingly being drawn into complying with Section 106 or local preservation ordinances as part of permitting or grant requirements for an ever increasing variety of projects. Coupled with other environmental and administrative compliance measures, an applicant's checklist can be several pages long. The potential backlash from such burdensome procedures will undoubtedly affect Section 106 compliance at the expense of cultural resources.

The Next Generation

We are now in an era where growing numbers of anthropology students are entering universities with participation in Section 106 compliance as their stated career goal. And indeed, the vast majority of students who graduate and pursue archaeology as a career will be involved in conducting, managing, or reviewing contract archaeological projects for Section 106 compliance. Their professors, more often than not, continue to view "contract archaeology" as a less than noble profession for which they choose not to address or only cursorily address in their curricula. Instead they view their jobs as providing students with a background in "anthropological archaeology" where archaeology is conducted to formulate and test hypotheses that are germane to current trends in research. It is their assumption that their graduates will learn Section 106 skills on the job and that students need their college years devoted toward loftier goals. What we find are many students entering the job market who are unable to integrate their undergraduate and graduate training effectively with their respective roles in the Section 106 process. Alone in an agency office, this individual is in danger of interpreting the law to favor agency goals rather than favoring the resource base. In a contract archaeology firm, they likewise learn little of the legal basis for the company's existence.

In recent years, in an attempt to further archaeological research on agency administered lands, university projects receive increasing levels of support through agencies. These projects are seldom related to Section 106 compliance, due mainly to constraints imposed by Section 106 driven deadlines and deliverables. Furthermore, academics typically lack the willingness to participate in the labyrinth of the bureaucratic process, and why should they? While these projects may help raise the image of the agency archaeology programs in the eyes of their former professors and agency supervisors, it also serves to widen the gap between contract archaeologists and the academics.

Can the overworked and litigious-shy public servants in agencies overhaul their system? Agency and SHPO archaeologists readily recognize the problem that they have key roles in fostering. Each has the power to reject reports and require rewrites. Any delays caused by their actions, however, are not viewed favorably by their managers, project proponents, and the offending contractor. In some instances the pressure upon agency archaeologists to "facilitate" a project is extreme and any action to the contrary is viewed as obstructionist. There are also instances where the agency archaeologist has approved projects, for whatever motivation, that they realized should not be approved. They, in essence, abdicated their responsibility to equally overworked SHPO archaeologists that may or may not notice an obvious shortcoming during their

hasty review.

Even within present agency frameworks there are ways for agency archaeologists to elicit a hint of critical data review. For example, site evaluation for National Register eligibility is oftentimes handled by paying lip service to the SHPO's State Plan (Nevada), preexisting research designs for an area, or a vague statement concerning the potential of the site to further our knowledge of human behavior. By citing these documents, the site evaluation is somehow elevated to a higher analytical plane. No matter that this section is oftentimes a "cut" and "pasted" word processing block (consultants have mistakenly pasted historic site eligibility justifications onto prehistoric site forms).

Some Suggestions for Change and Improvements

We believe that the motivation for improving the quality of reports lies squarely on the shoulders of the individual archaeologists, including those who produce the reports and those who review the reports.

Preservation of historic properties is but one means of fulfilling Section 106 compliance. Most National Register archaeological sites are eligible under the Secretary of the Interior's criterion d.; these sites derive their significance from their potential to yield information germane to local, regional, or discipline-wide research questions and goals. To this end it is important to justify this potential by providing evidence of a site's information potential beginning with its eligibility. This requires more than simply stating that it has the potential, but demonstrating the potential within the context of a valid research theme. In other words, the researcher must present some data and an interpretation of these data that are germane to other archaeologists.

Each property needs to be evaluated within a local, regional or national context. This is the basis for the requirement in the *National Historic Preservation Act* (Section 101) that each SHPO develop a historic preservation plan to provide contexts for evaluating eligibility. In Nevada, this consists of a series of historic and archaeological contexts. Although dated (Nevada archaeological state plan) and incomplete (Nevada historic state plan) these generalized documents attempt to provide guidance to those truly in need of such help. In fact, contexts need to be tailored to the resource at hand and its particular setting by the archaeologist or historian analyzing the project data.

Our responsibility as archaeologists is to contribute to our understanding of past cultures. Even within a Section 106 framework, the data and conclusions should extend beyond merely reporting on the presence or absence of sites and artifacts; even a short report should provide testable hypotheses. The range of inquiries can include predictive site location models to archaeological expressions of gender or ethnicity. The problems addressed, however, must not be dictated by a state plan or agency archaeologist. If a site has obvious data potential within one problem domain not identified in a state plan, then it should be exploited for its strengths and the state plan acknowledged but not explored. Further, the archaeologist must be individually responsible for dissemination of the data to peers and public, including clients, through publication. Agencies should provide publication assistance through whatever means possible. Although little time exists for publishing within the context of a small business, it is the responsibility of every archaeologist to do so, not for every project, but for data and ideas that are important contributions. Too many of us are working in isolation with little time devoted to journals or other venues of intellectual interchange. Only by publishing and thus actively participating in the archaeological community, can contract archaeologists hope to develop and improve the quality of

their work.

For example, to assist the Department of Energy's Nevada Field Office with their Section 106 compliance, they formed a peer review panel in 1992 to assess DOE archaeological programs and make recommendations to all parties concerned (contract archaeologist, DOE, Advisory Council on Historic Preservation, and SHPO). This panel consists of two academics and two agency archaeologists. The principal recommendations of the panel center on dissemination of knowledge through professional publications and production of a meaningful "gray" project synopsis. The time and costs associated with this peer review program are commensurate with the program expenses which are quite high. Results from the recommendations are due in 1995.

Rather than decry the loss of our control over Native American archaeological sites, we should garner as much information as possible from sites that we still have an opportunity to investigate whether in danger of destruction or in museum collections. We haven't done the resource justice to this point. We should also take active steps to recruit, train, and welcome Native Americans into the archaeological community.

Anthropology departments must realize that part of their responsibility is to educate and prepare students with sufficient information to function outside of a university setting. For most graduates specializing in archaeology, this should include meaningful training in Section 106 compliance. Most contract archaeologists are unfamiliar with the actual workings of Section 106 compliance beyond submitting the report to the agency and modifying sections as per agency instructions. Surprisingly, even a number of long-time agency archaeologists are also unknowledgeable in the details of Section 106. The Advisory Council on Historic Preservation and the University of Nevada Continuing Education Program offer short courses aimed at providing agency personnel brief but worthwhile instruction in various subjects. Further, university educators must also instill an obligation of professionalism in their students, that includes their responsibility to their profession to disseminate their findings through publication and to encourage others to do so.

Agency archaeologists usually avoid answering any questions concerning the suitability of one contract firm over another for fear of legal repercussions. Private project proponents, however, should be encouraged to continue their efforts in pressing for contractor information from previous clients, understandable deliverables from contractors, and competent treatment by agencies.

Conclusion

The NHPA is a means of protecting historic properties from unnecessary destruction by undertakings with federal involvement. Although we are somewhat critical of state and federal agencies for their role in promoting gray literature, we also find a lack of motivation in most archaeologists involved with Section 106 compliance to publish findings in journals or monographs. Non-participation in this facet of the profession fosters segregation between the universities and CRM. Most universities, likewise, fail to prepare their charges adequately for CRM, and their graduates apparently feel little obligation to publish their findings in the literature.

The realities of business leave little time for contract archaeologists to publish findings. The time to do so must come from overhead or personal time. Many of our peers from the 1960s and 1970s went into CRM feeling obligated to publish. Motivations to do so are not as great now. Clearly, we must all work to correct this problem and attempt to recapture the motivation that inspired Don Tuohy and his generation of archaeologists. Agencies should

encourage and promote publication as part of fulfilling the NHPA and the Secretary of the Interior's standards and guidelines for the conduct of archaeology. Each of us as contract archaeologist, agency archaeologist, university educator, Native American, and project proponent/sponsor must take responsibility for the present state of contract archaeology and provide our successors with a written legacy worth reading.

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