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NOTES

A REAPPRAISAL OF N. H. WINCHELL'S *PALEOLITHS OF KANSAS*

JOHN D. REYNOLDS

Deceased

In 1913 Newton H. Winchell, a well-known geologist from Minnesota, published a monograph entitled The Weathering of Stone Artifacts, No. 1: A Consideration of the Paleoliths of Kansas. In it Winchell presented his analysis of a large collection of stone tools collected from sites in the Kansas River valley. He concluded that the artifacts represented at least four distinct prehistoric cultures: Early Paleolithic, Paleolithic, Early Neolithic, and Neolithic and that the earliest of these cultures was assignable, minimally, to the Pleistocene or an even earlier time. Winchell, an outspoken proponent of great antiquity for the human settlement of the Americas, unwittingly utilized a collection of artifacts that contained a number of deliberate fakes. Winchell's thesis never gained acceptance by most American archeologists, and its impact on Kansas archeology fortunately has been minimal. Nonetheless, it is important to document the fraud.

In 1913 Newton H. Winchell, a geologist from Minnesota, published a monograph in the Collections of the Minnesota Historical Society entitled *The Weathering of Stone Artifacts, No. 1: A Consideration of the Paleoliths of Kansas*. Winchell presented the results of an analysis of a large collection of stone tools that had originally been gathered by Jacob V. Brower from sites in the Kansas River valley. Brower, for his part, used the artifacts to support his identification of the Native American provinces of Harahey and Quivira, both of which he placed squarely within the borders of Kansas (Brower 1898, 1899, 1903). Both provinces are mentioned in Spanish chronicles from the Coronado expedition of A.D. 1541.

Winchell had a different interpretation. He argued that these artifacts demonstrated the presence of a succession of cultures of "glacial" and "pre-glacial" man in Kansas. He thought that the artifacts represented at least four distinct prehistoric cultures: Early Paleolithic, Paleolithic, Early Neolithic, and Neolithic. The earliest of the earliest cultures—the Early Paleolithic and Paleolithic—Winchell thought antedated the Kansan glaciation, while the Early Neolithic occupation dated to the interval between the Kansan and Wisconsin glaciations. Only the Neolithic post-dated the

last glaciation. According to Winchell (1913:26), "These four stages of culture were separated by long intervals of time, and these intervals are to be stated by thousands and probably tens of thousands of years."

It is now known that Brower was mistaken in his placement of Harahey and Quivira, while Winchell was the unwitting and naïve victim of an archeological hoax. The artifacts that Winchell used in support of his hypothesis were, in fact, clumsy fakes. This paper exposes this hoax and, further, briefly considers its implications, which fortunately for Kansas archeology have been minimal. As an aside, it should be noted that the vast majority of the artifacts in the Brower collection are genuine artifacts of aboriginal manufacture. Typologically, they range in time from Paleoindian to Historic. Ironically, the collection contains at least a few of the distinctive Paleoindian projectile points that are indeed assignable to the terminal Pleistocene.

THE FORGER'S ART

Forgeries of antiquities have long been a concern of archeologists. In the Old World there is the notorious Piltdown fraud and the stultifying effect that this had on an understanding of hominid evolution

from the time of its discovery in 1912 until its eventual and final exposure in 1953 (Weiner 1955). Forgeries of lithic and other artifacts are similarly documented from well before the turn of the century from both the Old and New Worlds (Munro 1905; Wilson 1888).¹

As an archeologist who works for a public institution, and as a flintknapper, I have a somewhat heightened awareness of lithic forgeries and the dangers that they entail. Lithic fakes are of concern for the archeological profession both in the modern scene and in the collections that were amassed in past years. Sooner or later, these forgeries haunt us. They enter our domain in various guises. They may be included in artifact collections offered to public institutions as donations, with the donor often hoping for a substantial tax write-off. They may be presented by that peculiar breed of collector who delights in playing “stump the star” with every archeologist whom he encounters. This individual chortles with glee when he announces that he knows the knapper who made the “arrowhead” that the archeologist has just identified as being a dart point of Late Archaic type. As this paper demonstrates, the fakes may already be in the collections of many public institutions and for one reason or another be undetected. All of these fakes could potentially affect our understanding of culture history and culture process.

Lithic fakes are of many types and variable artistry. There are, unfortunately, no infallible guidelines or tests that can be applied to determine the genuineness of isolated artifacts. The appearance of newly chipped or reworked artifacts can be altered by various chemical methods, such as acid etching or additions of substances like egg white or albumin to create a glossy surface. Non-chemical means of aging may include the actual physical abrasion of the surfaces of the specimen. While it is possible in some instances to determine that this has been done, it is not always readily apparent. This is obviously one reason why archeologists rely so heavily on the in-situ context of artifacts for the testing of most archeological hypotheses, and unprovenanced collections are often used primarily for distributional studies or as teaching aids.

That said, artifacts may contain information revealing that they are fakes. Perhaps the most

obvious means of determining the presence of fakes is that reworking of specimens can result in the creation of new forms, which may be never-before-seen artifact types. At the extreme end of these new forms are what Witthoft (1960) has referred to as fantasies. In his terms forgeries are copies of actual artifact types, while fantasies are produced items that have no actual archeological referent. Instances of such artifacts include eccentrics from Oklahoma, which H. Holmes Ellis (1940) noted do not conform to any known patterns within their area of occurrence. Of course there are genuine eccentrics, that is, idiosyncratic artifacts that do not fit into known artifact patterns. Once again, archeological context is critical to distinguishing these from Witthoft’s fantasy types. Another fake of the fantastic type is the seemingly ubiquitous chipped stone fishhook, many of which were manufactured in the late nineteenth century by a Wisconsin flintknapper named Lewis Erickson (Jenks 1900). Erickson reworked aboriginally produced artifacts by pressure flaking with a pair of pliers. As Witthoft (1960:10–11) noted, chipped stone fishhooks have never been found in a good archeological context, though most large private collections and many museums have one or more examples.

QUIVIRA, HARAHEY, AND GLACIAL-AGE MAN IN NORTH AMERICA

With this introduction to lithic fakes and forgeries, we can return to the specific case of Winchell’s “paleoliths.” The story began in the late 1890s with archeologist Jacob V. Brower (Figure 1). Brower, a resident of St. Paul, Minnesota, combed the valleys of the Kansas River and its tributaries in search of evidence of the provinces of Quivira and Harahey in an attempt to prove, through archeology and history, that the Coronado expedition of A.D. 1541 visited east-central Kansas. With the assistance of a small group of dedicated but untrained local amateur archeologists, including L. R. Elliott, W. J. Griffing, John T. Keagy, Edward A. Killian, and Captain Robert Henderson, Brower quickly amassed a personal collection of an estimated 30,000 artifacts, primarily from sites situated between Topeka and Salina along the Kansas River drainage (Brower



Figure 1. Jacob Vradenburg Brower (1844–1905).

1903:xi). The largest number of artifacts was acquired from sites near Alma, Manhattan, and Junction City within an area that is known as the Flint Hills Uplands.

Brower, in attempting to identify the two Spanish-named Native American provinces, naïvely assumed that all these items were the products of those two groups during the recent past. Accordingly, he divided the collection into two general types. The more crudely made artifacts, especially chipped stone specimens, were presumed to have been produced by the Quivirans, a group that is now recognized archeologically as the Great Bend aspect and ethnohistorically as the Wichita. The more finely crafted chipped stone objects were assigned to Harahey, the protohistoric Pawnee. Brower then identified the center of Quivira as being in the Manhattan-Junction City area, and he located Harahey to the northeast, though still within the borders of Kansas. Brower published his findings in a series of lavishly produced volumes (e.g., Brower 1898, 1899, 1903). Modern researchers,

while not in agreement with Brower's specific location, and on the basis of much firmer artifactual evidence, have placed Quivira within the borders of Kansas, particularly in Rice and McPherson counties in central Kansas (Wedel 1959). Brower died in 1905, and his collection of Kansas materials was donated to the Minnesota Historical Society, where it remained until 1924, when it was acquired by the Kansas State Historical Society (KSHS). The collection has been in the possession of the KSHS since that time.

Newton H. Winchell, formerly the State Geologist for Minnesota, served as the head of Department of Archaeology at the Minnesota Historical Society from 1905 until his death in 1914, and it was during this period that he conducted his study of the Brower artifacts (Upham 1914:824–830, 1915). Winchell (Figure 2) was every bit as colorful a character as Brower, perhaps even more so. He was born in New York State in 1839 and graduated with a master's degree in geology from the University of Michigan in 1867. From 1872 until 1900, he was Minnesota's State Geologist and later was in charge of the Department of Archaeology at the Minnesota Historical Society.² While employed as State Geologist in 1874, Winchell was invited to accompany General George Armstrong Custer's expedition to the Black Hills on that fateful expedition when gold was discovered.³

Winchell was one of a number of American scientists in the late nineteenth and early years of the twentieth century who were convinced that the age of the human presence in North America had been greatly underestimated. Actually, a consensus that humans had settled the continent sometime during (or even before) the Wisconsin glaciation had emerged by the late 1880s, but soon thereafter was attacked by the anthropological elite of the era. By the early twentieth century then, the consensus had largely been reversed, though there were a number of individuals who rejected in their turn the new view that humans had been on the continent for only a few thousand years at most. Winchell became embroiled in the controversy regarding claims of glacial-epoch humans at site of the Lansing "Man" finds made in 1902 on the Concannon Farm outside Lansing, Kansas.⁴ This find actually consisted of the incomplete remains of an adult male and a

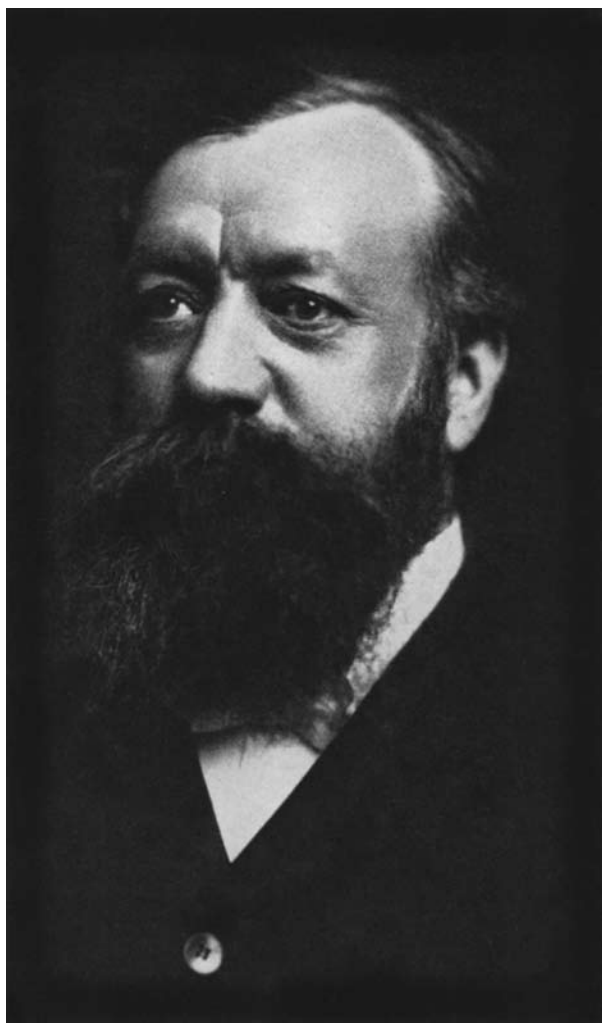


Figure 2. Newton Horace Winchell (1839–1914).
(Image courtesy of University of Minnesota Department of
Geology and Geophysics.)

child (Wedel 1959:91). The find spot, buried deep in the bluffs of the Missouri River, suggested to some scientists, including especially Winchell and his colleague Warren Upham, that they dated firmly to the last glacial epoch (Upham 1902; Winchell 1902a, 1903). Later studies have demonstrated that the skeletons are Archaic in age and were intrusive into the loess bluffs (Bass 1973), but the controversy over the age of the site brought Winchell into direct conflict with powerful members of the era's anthropological elite, namely William Henry Holmes (1902a) and, after the Lansing site discovery, with Holmes' protégé, the physical anthropologist Aleš Hrdlička (1903, 1907).⁵

In the preface to *The Weathering of Stone Artifacts, No. 1: A Consideration of the Paleoliths of Kansas*, Winchell (1913:vii-xii) quite eloquently set forth his thesis. He declared that American archeologists “ . . . have had little concern for the question of the relative ages of these specimens, and usually they have considered all their collections from American localities as the product of the historic Indian.” While this blanket condemnation of American archeologists was not altogether accurate, it did reflect, in part, the prevailing attitude of many prominent archeologists and physical anthropologists (Hinsley 1981:280–281). The question of the Pleistocene antiquity of humans in North America was not resolved, of course, until after the famous finds made at Folsom, New Mexico, in the mid-1920s.

A CONSIDERATION OF THE PALEOLITHS OF KANSAS

The full title of Winchell's monograph, *The Weathering of Stone Artifacts, No. 1: A Consideration of the Paleoliths of Kansas*, contains two important clues to what will be found within. First, Winchell attempted to use patination as an indication of the age of artifacts and, when he found differential patination on the same artifact, he interpreted this to be the result of successive reworkings of the piece by later humans. He recognized at least four, and sometimes six, different successive cultural groups that were responsible for this workmanship and he labeled them Early Paleolithic, Paleolithic, Early Neolithic, and Neolithic, respectively. The earliest cultures—Early Paleolithic and Paleolithic—Winchell thought antedated the Kansas (not Wisconsin) glaciation and that Early Neolithic cultures were present during the interval between the Kansan and Wisconsin glacial events. Neolithic post-dated the latter glaciation. Winchell (1913:26) suggested that the time between successive reworking by these cultures was on the scale of thousands or tens of thousands of years. He was quite specific in identifying the flake scars from these various cultures. Artifacts extant in the collection still bear Winchell's identifications in his own handwriting (see Figure 3). Purdy (1981:82) has documented the fact that later people did, in fact, sometimes find and rework chipped stone tools from earlier



Figure 3. An artifact from the Brower Collection with flake scars labeled by Winchell.

cultures; the means by which she determined this relied upon close study of the patination of the artifacts, along with archeological context. Winchell, however, very much overstated his case and, besides, the majority of the specimens were surface finds.

The second clue in the title of the monograph is the use of the word *paleolith*. Winchell deliberately employed archeological terminology developed for Europe and applied it to these Kansas artifacts. While some other American archeologists were also following this practice, especially prior to the mid-1890s, most were more cautious and refrained from using the European terms. Winchell was stretching analogy to the limit. His ongoing disagreements with William Henry Holmes apparently blinded him to the writings of Holmes wherein Holmes (1894) had so carefully outlined a reduction strategy for stone tool manufacture. To quote Holmes,

The facts to be especially brought out are these: The conditions of art in stone are such that the simpler forms of flaked implements employed in cutting, picking, scraping, and striking are necessarily shaped by like processes, pass through like changes of form, and reach closely identical results, whether made by people of low culture grade doing their best work, or by a people of high culture doing their rudest work. The early shapes will be repeated in the later shapes, and the refuse of rejection will, in the nature of things, up to the stage where specialization begins to take effect, be largely identical (Holmes 1894:137).

In other words, Winchell seemingly saw all chipped stone implements as finished forms. He did not realize that they could represent stages in a manufacturing sequence and that objects identified as crude could just be objects that were at an early

stage in this sequence. Hence, crude objects were to Winchell Paleolithic, while more finely flaked forms were Neolithic.

The Fatal Flaw

Now we must consider the truly fatal flaw in Winchell's thesis, the artifacts themselves. Winchell's approach to Brower's collection, which incidentally consisted of largely unprovenienced surface finds, was to select items for study that, in his opinion, showed dramatic evidence of differential patination. The most dramatic examples that he could find were a portion of the Brower collection that had been given to Brower by Judge John T. Keagy of Alma, Kansas. These specimens had reputedly been found on a series of prehistoric archeological sites located along Mill Creek in the vicinity of Alma (Brower 1903:65). There are indeed many sites in the Alma vicinity from where Keagy reportedly found these artifacts and some of these still contain abundant chipped stone refuse, of the blue-gray Permian-age chert that is local to this area, and it is evident that many of them are quarry and workshop sites. However, casual survey of these sites fails to disclose the kinds of crude, rude, and fanciful forms of artifacts described and illustrated by Winchell.

While not all of the artifacts studied by Winchell and depicted in his publication are fakes, the overwhelming majority are. A number of the original specimens were located in the Brower collection at the KSHS and these were subjected by the author to macroscopic and microscopic examination. The evidence of their fraudulent nature is as follows:

1) At least two fakes in the Brower collection are so labeled. These were evidently spotted by Brower and are clearly marked. The first of these, shown in Figure 4, is labeled "Bogus. Made in Kansas by Unknown person—1901." The second specimen is labeled with a K. and a B. catalog number 5770, and "Rechipped. 1903." The entry in Brower's catalog for this specimen states that it was returned to Keagy by Brower because it was rechipped. It evidently never made it back to Keagy, the donor. Winchell either missed or ignored the significance of these two items.

2) Many of the artifacts in the Keagy collection are of fanciful type and do not fit our knowledge

about artifact patterns for Kansas (Figure 5). This is obviously not a definitive reason for rejecting the artifacts as genuine. If it were so, then we would have a great difficulty recognizing the genuine exotic types that do occasionally turn up in a solidly documented archeological context. Nevertheless, the lack of fit with known patterns, as exemplified by these three specimens, all of which were used as evidence by Winchell, lends support to the hypothesis that they are fakes. Wedel (1959:95) hinted at the peculiar nature of some of these items when he stated that "... the chief merit of Winchell's book is perhaps its halftone illustrations of roughly fashioned cores, quarry blanks, blades, and other chert artifacts that once occurred in such extraordinary abundance along the belt where the Kansas River traverses the Flint Hills upland." In discussing the "Archaic" type points, that is, large stemmed and corner-notched points, which Winchell depicted in his plates 14 and 15, Wedel comments that they "... are not at all like the Plains Archaic specimens from northeastern Kansas. They look eastern rather than western, and seem strangely out of place when considered in relation to the usual run of specimens from the Woodland and later pottery cultures of the Central Plains" (Wedel 1959:538).

3) The flake scar patterns on these artifacts, plus the overall lack of symmetry of the specimens, is fairly conclusive evidence of their fraudulent nature. In general, new flake scars are restricted to the margins of the artifacts and do not invade the tool faces to any appreciable degree. This is so in spite of the fact that these trimming or shaping flakes were evidently removed with a percussion technique. The negative flake scars on the artifact margins indicate that striking platforms were massive and that the resulting flakes had large platforms, were abnormally thick, and were quite short. This is just the opposite of what one would expect from controlled or refined percussion or pressure biface thinning, such as was commonly employed by aboriginal artisans for final thinning and shaping of bifaces. The resulting angles at which the edges meet, whether on tools that Winchell identifies as celts, axes, knives, or projectile points, are incredibly and unbelievably broad. That is, they tend toward being obtuse instead of acute. Whereas a "typical" projectile point or knife has edge angles of less than 45 degrees, these specimens have edge

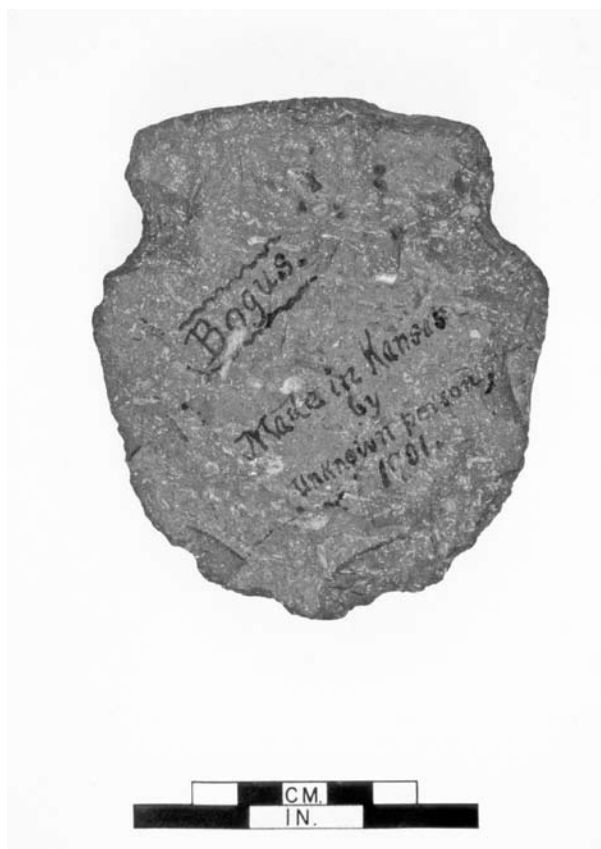


Figure 4. Two reworked artifacts in the Brower Collection. Brower himself found these and labeled one as “Bogus” and the other as “Rechipped.”

angles that approach or exceed 45 degrees. This results in bifaces that are very thick, relative to their width. The width-to-thickness ratio of bifaces, that is, their maximum width divided by their maximum thickness, is a fairly reliable indicator both of the knapper’s skill and of the place a specimen occupies in an inferred manufacturing sequence. Thick pieces usually occur early in the reduction sequence, and the thinner pieces are usually later in specific sequences (Callahan 1979). Thus, implements that have a width-to-thickness ratio of less than 3.00 are probably at an early stage of a manufacturing trajectory, while those that exceed this ratio are probably later. Errett Callahan (1979) in his classic paper, “The Basics of Biface Knapping in the Eastern Fluted Point Tradition: A Manual for Flintknappers and Lithic Analysts,” has observed that most projectile points exceed a ratio of 3.00, as do many so-called knives.

To give a couple of examples, a fairly typical Hell Gap Paleoindian dart point may have a width-to-thickness ratio of around 4.3–4.4, while a particularly well-made Pomona biface (excavated in

1983 from a site in Osage County) has a width of 55.3 mm and thickness of 6 mm, yielding an amazing width-to-thickness ratio of 9.22. In contrast, two fairly typical examples of the points studied by Winchell have width-to-thickness ratios of 2.17 with an even more extreme 1.75 for the other. While not every aboriginal biface is as well-made as a Hell Gap point or this particular Pomona biface, the width-to-thickness ratios of most genuine points and bifaces will approach these ratios more closely than do most of the artifacts that Winchell used in his study.

4) Finally, Winchell noted curious iron streaks (which he referred to as “iron mould”) that appeared occasionally on negative flakes scars and on flake arrises on some specimens. In discussing this he noted that

There is also a curious sprinkling of iron rust. It is found but occasionally, and prevails along the crests of anticlinal ridges formed by the intersection of two fracture planes, but it also occurs on smooth surfaces Occasionally it forms an

Figure 5. Aboriginal artifacts in the Brower Collection modified into shapes inconsistent with artifact patterns in the area.



interrupted small streak running in an unexpected and unexplainable direction across a flat or slightly curving surface, as if *some iron tool had left a portion of itself on the spot on being dragged across it* (emphasis added) (Winchell 1913:72–73).

Winchell then launched into an incredibly convoluted argument to prove that these stains were explainable by geological processes⁶.

I was able to locate several of the artifacts to which Winchell referred that had these curious streaks on them, and these were examined under a binocular microscope at magnifications ranging from 10X to 40X. Also, I was able to create iron streaks on some of my own replica artifacts that appeared macroscopically identical, and these were also subjected to microscopic examination. The iron streaks on tools in the Brower collection and those that I knapped were indistinguishable from one another. Winchell had discovered the evidence that should have suggested that the tools had been reworked in recent times, by someone using an iron billet, but he failed to realize this.

Who Was the Counterfeiter?

One question remains unresolved: Who was the counterfeiter? It certainly was not Winchell. The Brower collection was originally catalogued by Brower himself, and his finely penned catalog numbers are present on the specimens. They cross both the new and the old flake scars, so it is evident that the fraudulent items were created before his death

in 1905. It seems highly unlikely that Brower was the guilty party. He had nothing to gain by creating these fakes, and he himself spotted two other fakes in the collection that was given to him by Judge Keagy, and he so identified these pieces. All of the fakes analyzed by Winchell, in fact, were part of the Brower collection donated by Keagy.

Who was John T. Keagy? Keagy was born in 1840 in Pennsylvania, was a private during the Civil War, was wounded and discharged from the Union Army in 1862. Later he read law and was admitted to the bar in Pennsylvania in 1865. In 1870 Keagy came to Alma, Kansas, and starting in 1880, he held offices as probate judge and justice of the peace until at least 1910 (Keagy 1910; Thompson 1901:233–234). Keagy was a long-time and well-known collector of archeological artifacts. Robert Blasing, an archeologist with the Bureau of Land Reclamation, grew up in the Alma vicinity and reports that one of his relatives was instructed in the art of flintknapping many years ago by none other than Judge Keagy (Robert Blasing, personal communication 1988). The fact that the forgeries are limited to that portion of the collection given to Brower by Judge Keagy and that Keagy is known to have been a flintknapper (though evidently not a highly proficient one), strongly supports the identification of Judge Keagy as the guilty party⁷.

CONCLUSION

This paper documents a hoax, perpetrated in the early twentieth century in which a collection of

artifacts was reworked and intentionally or otherwise passed off as genuine artifacts of aboriginal manufacture. Based on a careful study of the specimens in the Brower collection that were later examined by Winchell, my supposition is that almost without exception, these started as genuine artifacts, particularly broken or rejected biface blanks, axes, and celts, which were subsequently reworked by a novice knapper who used iron tools and who lacked the skills necessary to properly thin the bifaces.

Fortunately, the impact of the hoax on our understanding of culture history and culture process in Kansas has been minimal. No one ever took Winchell's hypothesis very seriously and, except as an historical curiosity, it has been largely ignored by modern researchers⁸. Wedel (1959:95) notes that one of Winchell's contemporaries at the U.S. National Museum—and Wedel did not supply a name—penned the following missive on the flyleaf of a copy of Winchell's book: "This is the best illustration of utter error and silly foolishness ever issued from the press." Despite this admonition, Wedel (1959:539–541) nonetheless thought that the book had at least some value for the diversity of chipped stone tools shown in the halftone illustrations. In fact, Wedel's illustrator at the Smithsonian Institution apparently rendered drawings of some of Winchell's "artifacts" for inclusion in *An Introduction to Kansas Archeology* (Wedel 1959:539–541). For example, Figure 99j is a drawing of an artifact from Plate 14 in Winchell's book. This appears to be one of the fantasy types that are without good archaeological referent, as even Wedel (1959:95) had to acknowledge its problematic nature. To be sure, there are instances of reworking of older stone tools by later cultures⁹. Purdy (1981:82) has suggested, for instance, reworking of stone tools from one culture by flintknappers of a later cultures in Florida and, moving further afield, Gould et al. (1971:160) have documented re-use and rechipping of ancient Australian tools by modern peoples. We may eventually successfully document similar occurrences in Kansas, but however that may be, we can safely dismiss the evidence presented in Winchell's book.

EDITOR'S NOTE

A different version of this paper was presented in 1988 at the 10th Annual Flint Hills Archaeological

Conference in Topeka, Kansas. As presented here, the manuscript has been edited from its original form by Marlin F. Hawley, who has updated the paper with endnotes supplying additional information. It is hoped that John would have approved these changes.

ENDNOTES

1. In a sweeping review of the state of American archeology as it was in the late 1920s, Judd (1929) worried about fakes and forgeries and their potential to cloud understanding of prehistory. More recently, Whittaker and Stafford (1999:203) have also expressed deep concern that replicas (copies of stone tools) and fakes "have a perturbing effect on the archaeological record." One example of fake artifacts and their effect on an archeological understanding is the Sandia point, a type originally found in Sandia Cave, New Mexico and supposed to pre-date Clovis. The type has long bedeviled Paleoindian specialists; however, critical review of site stratigraphy by Haynes and Agogino (1986) and allegations by Preston (1995) that Frank Hibben, the site's excavator, falsified site data, including possibly faking the points, have undermined acceptance of the site and the Sandia point type. Another more recent example is that of the case of respected Japanese archeologist Shinichi Fujimara, who from the late 1970s through the 1990s planted fake artifacts at more than 40 of his archeological sites, thereby pushing back the accepted date for the settlement of Japan to as much as 500,000 years. Exposure of his activities has forced wholesale revision of Japanese textbooks. An associate of Fujimara's accused of complicity in the fraud committed suicide (Miller 2002).

2. Winchell's contribution to the unraveling of Minnesota's complex geological history is almost without parallel (Schwartz 2009; Upham 1915), and not surprisingly the vast majority of his over 300 publications are concerned with geological topics. However, archeology always ranked high among his interests and among his numerous archeology publications is the massive *The Aborigines of Minnesota* (761 pages, more than 640 figures plus halftones and maps), which collated and described a vast amount of information generated by the ill-fated Northwestern Archeology Surveys of Alfred J. Hill and Theodore H. Lewis, along with materials gathered by the late Jacob V. Brower (Winchell 1911). Not surprisingly, the vast majority of his archeology papers post-date 1905, when he succeeded Brower as archeologist for the Minnesota Historical Society.

Unlike many contemporaries, Winchell had, as early as 1881, abandoned his early belief in the Mound

Builders, the mythic, pre-Indian race responsible for the construction of tens of thousands of mounds and other earthworks throughout eastern North America. He reached this conclusion following examination of aboriginal quarry pits in the Lake Superior region (Upham 1916:70–71). As for the Kansas paleoliths, Winchell first put forth his thesis in a 1912 paper published in a now long defunct archeology journal, *Records of the Past*, and reiterated his belief in the accuracy of his statements regarding these artifacts as late as the week before his death in April 1914 (Winchell 1912, 1914, 1917).

3. According to Morey (1999), Winchell seems to have accompanied the expedition more in the role of naturalist, and collected both rocks and fauna. However, he did produce the first geological map of the Black Hills (Schwartz 2009). After gold was reported by prospectors accompanying the expedition, he publicly denied seeing any of it. Custer's response was predictably blunt: Winchell did not see any gold because he had not bothered to look for it. To resolve the dispute, the U.S. government was compelled to dispatch a second expedition to the area; this time gold was definitively reported in economic quantities, thus spurring a gold rush (Morey 1999).

4. Winchell was drawn into conflict concerning the glacial-epoch settlement of North America with the Smithsonian Institution's William Henry Holmes prior to the Lansing, Kansas, discovery. In 1877 Winchell (1878:54) found apparent quartz artifacts at Little Falls, Minnesota, which were subsequently ascribed to glacial epoch human habitation of the area by Minnesota's first female archeologist, Frances E. Babbitt (1883), a view that was roundly criticized by Holmes (1893) after a visit to the site in Winchell's company. McGee (1893:444) alleges that Winchell agreed substantially with Holmes, but by the early twentieth century he appears to have reconsidered this position, renewing the argument for substantial antiquity of the specimens (Winchell 1902b). In a 1906 article, "The Pre-Indian Inhabitants of North America," Winchell estimated the age of the site at 6,000 to 7,000 years or about 2,000 years after the end of the last glaciation (Winchell 1906:172). In modern parlance, this would ascribe the site to the Archaic tradition, although questions remain as to whether the locale is or ever was an archeological site and, despite the objections of early investigators, as to whether the quartzite "artifacts" were nothing more than naturally fractured stone (Birk 1991; Birk and Breakey 1993).

5. Regarded as modest and retiring by friends and family, Winchell also was not one to shy away from a fight, and the intensity with which he threw himself into controversies, be it the "early man" controversy or various geological controversies of the day, stemmed

in part from "a populist, midwestern distrust of eastern institutions and power" (Eagan 1989:238). His aversion to the eastern scientific establishment was such that he was a major force in founding the journal *The American Geologist* in 1885. Journals are arguably important in disciplinary self-organization, and *The American Geologist* was no exception, especially at it was a forum for discussion of a host of broadly defined geological topics in the still fractious field of late nineteenth-century American geology. Nonetheless, it is equally significant that the journal was based in Minnesota—far outside the eastern centers of science and learning. Winchell and his son (assisted by Winchell's wife) edited it until 1905, when it merged with *Economic Geology* (Bain 1916).

6. Convolutioned, indeed; the passage reads:

How to account for the curious distribution of brown hematite . . . is a puzzle. Can it be as follows? It is essentially a very late, probably post-Glacial, effect, and as iron oxide is the chief coloring agent in turning the chert brown, can any cause be named that would make it accumulate in this manner? The most evident feature in this oxide of iron, in its manner of distribution, is its accumulation in *streaks*, such as may have been formed by a hard tool (iron apparently, but perhaps another chert) dragged forcibly over the surface of the chert where the oxide has formed by chemical secretion. In case the surface of the chert were slightly crushed or powdered along such an accidental streak, would the crushed condition of the surface cause a more rapid deposition or a detention of iron along such a streak (or scratch)? I notice also that this curious accumulation is along a narrow belt where the chert is not crushed, but is covered by a thin coating of something like evaporated albumen, having a glistening surface when not removed by friction nor covered by the oxide . . . When this brown hematite occurs along a ridge formed by the intersections of two flake surfaces, which is very common, it seems impossible to ascribe it to a scratch by some hard foreign tool or other substance. In such cases I see it is not only scattered along the whole crest of the ridge, coloring the surface in a narrow strip, but forms, at somewhat regular intervals, little bunches, or concretionary spheres which lie exactly on the crest of the ridge, these being evidently simply local enlargements due to greater deposition from ferriferous solution. These little concretions are so numerous sometimes as to form almost a continuous line . . . (emphasis in original) (Winchell 1913:73–74).

Winchell went on to note that,

... a Paleolith from Loire Bassin [France] similar irony spots, in form of isolated small scales, but of darker color, are sprinkled over one surface, apparently the surface which was downward during a long period, though they are absent from the other surface . . . On the specimen from Thetford drift of England, only two spots are found where such accumulation of iron is seen, and these are on ridges, in manner comparable with the accumulation on the Kansas artifacts (Winchell 1913:75).

This passage also demonstrates Winchell's commitment to his hypothesis: when he needed comparative material, he turned to the European Paleolithic. As his final publication, "The Antiquity of Man in America Compared with Europe," makes clear, Winchell (1917) remained firm in his belief that humans had inhabited North America on a scale comparable with the settlement of Europe.

7. Keagy married Mary Morrison in 1874, and the couple had three children, one of whom died in infancy. Keagy's public service actually dates to 1871, when he was appointed county attorney to fill a vacancy in the position. The following year, he was elected county attorney in his own right and then reelected to the post in 1874. In 1880 he was elected probate judge, in which capacity he served until 1884, when he was elected Justice of the Peace for Alma Township (Cutler 1883:995). Keagy was reelected to this office at least as late as 1914, but sometime prior to 1918, age and failing health compelled him to resign his office. He then took up residence in the National Soldier's Military Home for Civil War Veterans in Leavenworth, Kansas, where he died in September 1929 (Anonymous 1918, 1928, 1929).

Judge Keagy was an ardent collector, amassing a collection of over 10,000 artifacts from sites along Mill Creek, and with Edward Killian, Captain Robert Henderson, and others, organized the Quivira Historical Society in Alma in 1902. In 1904 Keagy was instrumental in founding the Wabaunsee County Historical Society, of which he was the first curator (Winkler 2004a, 2004b). Keagy was one of several noted amateur archeologists and collectors appointed in 1914 by the KSHS's Secretary William E. Connelley to a standing Committee on Archeology (Morehouse 1923:18). Keagy's appointment to this committee is not without irony, as in the late 1890s when Brower approached the KSHS regarding his efforts to locate Quivira and Harahey, he was rebuffed and, according to Morehouse (1916:35) was told by

the president of the Society . . . that when Kansas wanted to conduct archaeological investigation[s]

it would not call upon an outsider. This unfortunate episode resulted in estranging a number of capable Kansas historians from our Society, who thereupon formed another organization, which conducted extensive original explorations, discovered vast quantities of prehistoric relics, published elegantly bound books of their labors, and erected several costly monuments—all of which was splendid work, but one in which our Society had no part. But the most unfortunate circumstance was that most if not all these relics found—a vast amount of stone and flint implements . . . —were taken out of the state . . .

However that may be, perusal of the older *Transactions of the Kansas State Historical Society for 1897–1900* and again for 1901–1902 suggests considerable interest by the Society in Brower's work:

Mr. J. V. Brower, an eminent archeologist, explorer, and author, has published two volumes of surpassing interest, "Quivira," and "Harahey," in which he locates the villages of Quivira, visited by Coronado, along the south side of the Kansas and Smoky Hill rivers, from Mill creek, in Wabaunsee, to Lyon creek, in Dickinson, principally. He locates villages as follows: In Pottawatomie county, eleven; Wabaunsee, ten; Riley, eleven; Geary, twenty; Dickinson, four; Marion, one; McPherson, six; Rice, one; Barton, one. Mr. Brower has visited this region once and twice a year, beginning with 1896. He has gathered thousands of the stone implements on the sites represented by him, after the most patient investigation, and he has enlisted the greatest interest among the people in the localities named. He has been assisted by J. T. Keagy and E. A. Killian, of Alma, W. J. Griffing, of Manhattan, and Robert Henderson, of Junction City; and so confident are they of their work, that they propose to erect a monument in honor of Coronado's march, in Logan Grove, south of the Smoky Hill, opposite Junction City. Mr. Brower's researches and publications will have a far-reaching effect among explorers and students in establishing Quivira in the heart of Kansas. He is a Minnesota man, and hence is operating without bias, being influenced solely by his own investigation and judgment, and paying his own bills, including the handsomest of publications (Martin 1902:21).

The *Transactions for the Kansas State Historical Society for 1897–1900* indicates that the Society lacked the ability to finance Brower's work or his reports (Martin 1900:22). At the same time, the KSHS Board of

Directors resolved in 1898 (after Brower's first volume appeared) that a committee should continue the work begun by Brower and further, cordially, requested that he collaborate with this group. Brower was also asked to turn over maps and other information to the KSHS "... for publication of the results ... in a volume which shall be of a style creditable to this Society and the state of Kansas" (Martin 1900:16). Perhaps, this is not so far from Morehouse's later assessment after all. In any case, among the estranged referred to by Morehouse was of course Judge John T. Keagy, who along with several of his compatriots then founded the Quivira Historical Society. As Keagy was in 1914 74 years of age, it is doubtful that he conducted much additional field work in the field on behalf of the Society.

Ultimately, assuming him to have been the perpetrator of the hoax, Keagy's motive remains unknown. His obvious interest in antiquities from the Alma area and his involvement in the Quivira Historical Society would seem to argue against any deliberate attempt on his part to dupe Brower or anyone else, especially as Keagy helped to finance a granite obelisk erected in 1904 in Alma, which commemorated both the discovery of Harahey by Coronado in 1541 and Brower's rediscovery of it. Keagy may have used some artifacts in his possession to practice flintknapping and these were then unintentionally included with the material he gave to Brower. Less likely, perhaps, is that Keagy saw no harm in what he was doing, that is, reworking specimens, and that he felt no particular qualms in their donation to Brower. Ultimately it is curious that Keagy read a draft of the monograph (Winchell 1913:68), though it remains unknown if he tried to alert Winchell to the reworked specimens. As Brower found at least a couple of these reworked artifacts and intended on returning one (if not the other) to Keagy suggests that Brower at any rate was aware of and concerned about such fakes.

8. There was some positive comment on Winchell's monograph: the historian-explorer Edward Swift Balch (1917) lavished praise on the volume in an article, "Early Man in America," published in the prestigious *Proceedings of the American Philosophical Society*, as did Warren King Moorehead (1914) in a book review in the *American Historical Review*. Both men found Winchell's arguments compelling and wholly believable. Positive mention of the volume was also made in *The Wisconsin Archeologist*, which deemed it "worthy of the attention of students [of archeology]" (Anonymous 1913:69). Upham (1916:72), of course, remarked favorably on the book, and G. Frederick Wright (1912:524–526) enthusiastically devoted a couple of pages to a discussion, not of the book, but of Winchell's earlier article in *Records of the Past*. On the other hand, Johan A.

Udden, discoverer of chain mail at the Paint Creek site in McPherson County in the mid-1880s and, at the time that Winchell's monograph was published, a geologist with the Bureau of Economic Geology in Texas, greatly admired Winchell's *The Aborigines of Minnesota* but privately pronounced his latest monograph "a tragedy" (quoted in Hawley 2002:31).

Some years later Nels Nelson (1933:117–119) penned a thorough review of Winchell's book and even went so far as venture to St. Paul to look at the collections: "... I am far from convinced of anything like six discernible artificial surface conditions; but even if they exist, it does not follow that any of them are necessarily of Pleistocene date" (Nelson 1933:118).

The Old World archeologist George Grant MacCurdy (1915) merely summarized the thrust of Winchell's arguments in a few sentences and without additional comment, as did the reviewer for the British journal, *Geological Magazine* (Anonymous 1914:35). Rivet (1919) followed a similar tack in a summary of Winchell's 1914 paper, "L'homme primitif dans le Kansas," which he presented at the International Congress of Anthropology and Archaeology in Geneva. Likewise, Boule (1923:401) briefly discussed Winchell's argument but, while acknowledging the controversy surrounding early man in America, added, "It must be admitted that discoveries of implements in the depths of geological layers would be more conclusive." Sir Arthur Keith, though, in his volume *The Antiquity of Man* went further and commented that

It is clear from the researches published recently by the late Professor N. H. Winchell that the Middle States of America were inhabited at an earlier date than is [generally] indicated ... Beneath the deposits of the Kansan glaciation, which preceded the Illinoian, Professor Winchell has found stone implements of a rude Paleolithic type, fashioned by the men who lived before the great Kansan glaciation. The remains of the men of that period have not yet been discovered (Keith 1915:283).

Winchell (1917), it might be noted, was a proponent of Piltdown man—championed by Keith and questioned by the likes of Hrdlička (and some continental prehistorians). Winchell was held in high esteem by many European prehistorians, including Boule and Keith, for his work in glacial geology. For that matter, even Holmes (1902b) evidenced a high regard for his geological activities, and made a point of noting that Winchell was the president of the Geological Society of America.

The laudatory remarks of Balch, Moorehead, Wright, and Keith notwithstanding, the mainstream of the anthropological profession in 1913 and for some years

after was predisposed *not* to accept his thesis, which ran counter to prevailing views on the early settlement of North America. Mostly the volume was met by a deafening silence. In this regard, it is interesting that George P. Morehouse (1923:19) commented in a discussion of a collection from the Topeka area that “some stone implements in Kansas . . . belong to the Paleolithic or earliest stone age, the first of four great epochs into which most scientists divide prehistoric archeology. This would indicate that man inhabited ancient Kansas away back in company with the mammoth and other extinct beasts and reptiles.” Although Winchell, too, subdivided prehistory into four periods, there was no specific mention of him or his *Paleoliths of Kansas*.

9. Patina formation does imply time, as Winchell argued. However, decades after the publication of Winchell’s monograph, the use of patina formation as a relative dating technique remains problematic. As Purdy (1996:119–120) notes:

. . . weathering phenomenon may have potential use as a dating technique. This is called patina formation, in which, through a diffusion process, there is selective leaching of elements (primarily iron) from the chert surface (known as dynamic weathering) If the rate of patina formation could be calculated it should be possible to determine the age of stone implements. But even though weathering always takes time, it can occur rapidly or slowly depending upon environmental conditions and the composition of the chert material. In order for patina formation to be used as a dating technique, therefore, it would be necessary to establish a reaction formula for each site and it would require large numbers of specimens and precise control over excavation processes.

Writing more than 60 years earlier, Nelson (1933:117) reached essentially similar conclusions.

At the outset it must be premised that while patination . . . is undoubtedly a valuable criterion of age, it is at the same time a most difficult phenomenon with to deal effectively. Thus, to secure valid results by the ordinary comparative method is next to impossible, because the essential factors involved in the patinating process are rarely if ever constant; that is to say, identity of raw materials to be affected and identity likewise of the predisposing physical and chemical activities do not obtain over any considerable portion of the world. Each case of patination, therefore, has to be treated independently, and whatever comparisons are instituted the resulting conclusions can have only the merest general significance.

In consideration of Winchell’s study, he (Nelson 1933: 118-119) added:

We need here . . . to have the opinion of both the mineralogist and the chemist as to what has really happened, and whether or not time is a uniformly important factor in the process [of patina formation]. . . . When we examine our jasper, chert, agate, hornstone, and other flintlike specimens, we rarely find more than the faintest trace of weathering, nothing, at any rate, to compare with the shiny ochreous condition of many of the flint coup-de-poing specimens from the Lower Paleolithic, e.g., of western Europe [from which Winchell drew for his comparisons]. At the same time, it is necessary to bear in mind that the patina on European flint artifacts does not always vary according to age. Thus, while the worked flints removed from the dry rock-shelter type station at Le Moustier show no appreciable traces of patination, the specimens from the open, wet La Mocque and later stations are sometime chemically altered to a creamy white cheeselike substance, the like of which I have not observed in America. Clearly, however necessary time may be as a factor in patination, certain other conditions are far more important In short, while our American patination studies leave much to be desired . . . a lapse of time comparable to that demonstrable for the Old World is out of the question.

It would be unfair to criticize Winchell for a failure to appreciate the many and localized variables involved in the formation of patinas. Winchell’s method—if one wants to call it that—was in any final analysis “a best guess.” Indeed, not even that, as it is difficult to come to any other conclusion but that, brilliant though he may have been, Winchell’s assignment of these artifacts to different cultural stages spanning the Kansan through Wisconsin glaciations stemmed from and reified his belief that humans had long been established on the North American continent.

Such a conclusion is potentially of more than historical interest, especially in light of the ongoing debate on the authenticity of the Kensington Runestone, as the patina thereon was also studied by Winchell (i.e., Winchell 1910). For instance, Kehoe (2005:31–34) discusses his assessment of the patination on the stone (in the case of the runestone, greywacke), casting it in a positive light. While there is no doubt that Winchell was knowledgeable regarding Minnesota rock and was of unimpeachable character (Kehoe 2005:31), the evidence of his slightly later work on the Kansas paleoliths suggests that, where patination was involved, neither he nor any

of his contemporaries had any reliable, replicable means of evaluating the time elapsed in its formation. Certainly this appears to be the case for the patination found on the chert artifacts in the Brower Collection that were studied by Winchell. Consider: a) the original artifacts studied by Winchell were mostly manufactured between roughly 1,000 and 5,000 years ago and b) the variation in patination attributed by Winchell to reworking by cultures separated “by thousands and probably tens of thousands of years” (Winchell 1913:26) was, in fact, the result of rechipping within the span of a decade, if not much less. By the time Winchell saw these artifacts in 1912, this rechipping had been done probably around 10 or at most 20 years before. In all likelihood, given the size of Keagy’s collection, the majority of the artifacts were probably kept out of doors where they continued to be exposed to the elements. Patinas then formed on fresh flake scars. That Winchell could be so disastrously wrong in the case of the artifacts from Kansas would seem to raise serious questions about his work involving patination in general as a means of dating archeological materials.

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THE NORTHWESTERN ARCHAEOLOGICAL SURVEY IN KANSAS

MARLIN F. HAWLEY

Wisconsin Historical Society

In May 1893 Theodore H. Lewis of the Northwestern Archaeological Survey (NAS) visited the Fort Leavenworth, Kansas, area in search of French Fort de Cavagnial. The NAS was funded by Alfred J. Hill of St. Paul, Minnesota. Over the 15 years of its operation, Lewis recorded many thousands of Native American mound, earthwork, and habitation sites. The search for Fort de Cavagnial is part of a secondary interest of the two men. Established by the French in 1744 and abandoned in 1764, the ruins of the fort were noted by later explorers, such as Lewis and Clark and the expedition of Major Stephen Long. Lewis surveyed, studied plat maps, and interviewed old settlers in the area where the fort was believed to be, but ultimately he was unable to find any trace of the site.

The Northwestern Archaeological Survey (NAS) was organized and funded by the St. Paul, Minnesota, businessman Alfred J. Hill and implemented by the indefatigable Theodore H. Lewis. From 1881 until the untimely death of Hill brought the enterprise to an abrupt halt in 1895, the survey documented many thousands of mound, earthwork, and habitation sites. Initially an effort to map these localities in Minnesota, the survey shortly expanded to include investigations in 12 Midwestern and Plains states, including Kansas, and one Canadian province (Benchley et al. 1997:51; Dobbs 1991; Finney 2001; Haurly 1990, 1993; Keyes 1928; Lewis 1898a, 1898b; Winchell 1911). The investigations conducted in any given state varied considerably, with, naturally enough, Minnesota and states immediately adjacent to it receiving the majority of work.

This paper chronicles the one incursion by the NAS into Kansas, the purpose of which was to locate the archeological remains of eighteenth-century French Fort de Cavagnial. The paper is based on information gleaned from the NAS notebooks and correspondence, curated by the Minnesota Historical Society. The history of the NAS and biographical details about Alfred J. Hill and T. H. Lewis are reviewed. Additionally, to provide background to Lewis' search, the history of the elusive Fort de Cavagnial, generally thought to lie somewhere in

northeast Leavenworth County, Kansas, is also summarized. Although the survey failed in its attempt to find the site, the venture nonetheless is one more piece in the larger picture of NAS activities and adds to the literature on the history of archeology in Kansas in general and of research directed at locating Fort de Cavagnial.

HILL, LEWIS, AND THE NAS

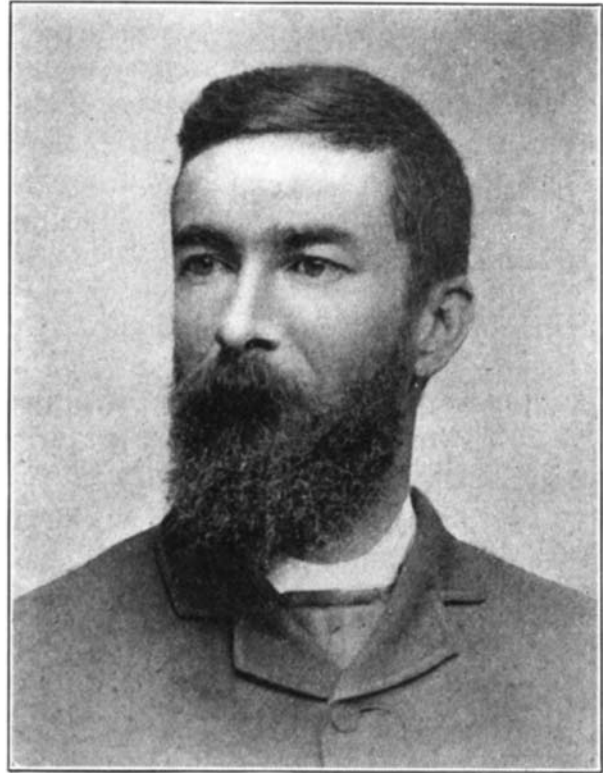
The NAS, along with its winter twin, the Southeastern Archaeological Survey (SAS), represents the most extensive effort ever conducted to record mound and earthwork localities in the United States (Finney 2005a, 2006). The NAS alone recorded the locations of more than 2,000 sites with an aggregate of 17,000 mounds and earthworks (Dobbs 1991). In conjunction with the SAS, Lewis mapped another 11,000 mounds, plus enclosures, rock art sites, and the like (Lewis 1898b:329), although the records from this latter survey have been lost (Finney 2004). By way of contrast, the Smithsonian Institution's much heralded mound surveys of the late 1880s and early 1890s recorded around 5,000 mounds (Thomas 1894) and, as Dobbs (1991:4) points out, the NAS often recorded its subjects in much greater detail. The final report on the Smithsonian Institution's work is often regarded as a benchmark in the history of Americanist archeology, finally

overturning the overwrought Mound Builder myth (Smith 1985; Willey and Sabloff 1993). Due to the tragic way that the NAS ended, only a portion of its data was published during its period of operation. Consequently, the full scale of its activities has only gradually come to be known.

Theodore Hayes Lewis, “arguably one of the most important late nineteenth-century archaeologists working in the Midwest” (Finney 2006:4) (Figure 1), was born in Richmond, Virginia, probably in 1856, although some uncertainty adheres to the actual year. The young Lewis grew up in Columbus, Ohio, and Little Rock, Arkansas. After completing his public school education, he relocated to Ohio, and in 1874 he enrolled in a course at the Miami Commercial College in Dayton (Finney 2005a). There are indications that he taught school in Ohio for a year or two, and at this time he learned the surveyor’s arts (Keyes 1928). In Ohio he developed an interest in archeology. Abandoning teaching from 1876 through 1878, he conducted excavations of sites in Arkansas, Mississippi, and Tennessee on behalf of a German archeologist (Finney 2004, 2005a, 2006).

In 1880 Lewis met Alfred J. Hill, and the next year the two began a joint survey of mound locations. In 1883 Hill and Lewis signed a formal contract, forming the Northwestern Archaeological Survey. Although originally planned to encompass only Minnesota, it almost immediately expanded into the Dakotas and Wisconsin (Finney 2006). The upper Midwest—the Dakotas, Minnesota, Iowa, Illinois (as far south as Cahokia mounds), and Wisconsin—constituted the core geographic area in which the NAS focused. However, from 1883 to 1895, the intrepid Lewis also ventured into Michigan, Missouri, Kansas, Nebraska, Indiana, Ohio, and the province of Manitoba. Lewis (1898a) later estimated that he had traveled 54,000 miles on behalf of the NAS, as many as 10,000 of those on foot. During the winter months Lewis headed south, conducting surveys from Florida to Texas on behalf of the Southeastern Archaeological Survey (Finney 2004).

Alfred J. Hill, the driving force behind the NAS, was born in England in 1833, and as a young man trained as a civil engineer (Figure 2). He immigrated to Red Wing, Minnesota, in 1854 and then to St. Paul a couple of years later. In St. Paul he



T. H. Lewis

Figure 1. Theodore Hayes Lewis (ca. 1856–1930) (from Winchell 1911).

worked as a draftsman and during the Civil War served in the Office of Topographic Engineers in Washington, D.C., and as a clerk with Company E of the Minnesota Volunteer Infantry (Finney 2006). As a member of the Minnesota Historical Society (MHS), Hill served on its Committee for Archaeology. The destruction of the state’s many Indian mounds was a source of concern to Hill, and as a member of the committee, he collected and was sent data on mounds from across the state and adjoining areas. After the committee was dissolved, Hill prepared a prospectus for a much more elaborate survey, laying out its rationale and goals. For a time he hoped that a source of public funding could be found that would permit a survey of mounds in the state, but when the funding failed to materialize, Hill opted to act on his own. For some years prior, he had invested heavily in real estate, generating a comfortable degree of wealth (Dobbs

Hill's untimely death, he reversed many of his opinions about Lewis (Finney 2006). While he has to be credited with rescuing the NAS field records from oblivion, he also had absolutely no intention of allowing Lewis access to them, apparently intending to incorporate the NAS data into his own projected summary of Minnesota archeology (Dobbs 1991). As it happened, Brower passed away in 1905, and it was not until several years later that Winchell accomplished the task, publishing a massive tome, *The Aborigines of Minnesota* (subtitled *A Report Based on the Collections of Jacob V. Brower, and on the Field Surveys of and Notes of Alfred J. Hill and Theodore H. Lewis*). The volume duly credited Hill and Lewis for collection of much of the site data, but Winchell, like his friend Brower, tended to be dismissive of Lewis and his obvious skills as an archeologist (Finney 2006; Winchell 1911:VII-X). Lewis and Winchell had clashed over several issues related to Minnesota prehistory in the past, and Lewis had been critical of him in letters to Hill, which Winchell had the opportunity to read in the preparation of his book. By the time the volume was published, Lewis had long since departed St. Paul, embittered by a turn of events that denied him access to the vast amount of data that he labored so long and hard to collect.

Although he never had the opportunity to publish the results of his surveys in the Midwest and South, Lewis was nonetheless a prolific writer, publishing over 50 articles stemming from the NAS and SAS in a wide variety of journals (see Finney 2005a for a bibliography of his writings). Subsequently, a number of these were gathered in two volumes, *Tracts for Archaeologists*, printed in St. Paul in 1893 (Lewis 1893a, 1893b). Dobbs (1991) and Finney (2006) observe that, despite the testimony of Winchell, Brower, and later scholars (Keyes 1928), neither Hill nor Lewis gave any credit to the Mound Builder myth (Silverberg 1968), which had become deeply rooted in the American psyche (Dunnell 2001; Hinsley 1996). Lewis, in fact, published a withering critique of William Pidgeon's 1858 *Traditions of De-Coo-Dah*, an important work in the canon of Mound Builder literature. He demonstrated through careful fieldwork the falsity of Pidgeon's statements regarding the High Island Mound Group in Minnesota (Lewis 1886). Lewis used the term mound builder, but in a way

that implied that the mounds were built by the ancestors of historic and contemporary Native American groups. In the post-NAS years Lewis turned his attention to ethnohistory, and with Frederick W. Hodge he edited a book, entitled *Spanish Explorers in the Southern United States, 1528–1543* (Hodge and Lewis 1907), which remains in print to this day. The volume arose in part from his longstanding interest in the De Soto entrada.

By the time *Spanish Explorers in the Southern United States, 1528–1543* was published, Lewis was about 50 years old. After its publication he gradually faded from sight, and by 1911 Winchell (1911:x) could only note that he was thought to be in Colorado. In fact, Lewis had apparently resided in Colorado for a time but ended up in St. Louis, where he sold newspapers. Lewis died in 1930 as an inmate in the St. Louis City Infirmary, an institution that served the poor and insane, and was buried locally in an unmarked grave (Finney 2005a, 2005b).

Despite the way in which the NAS ended, the data, collected in dozens of notebooks, has been preserved in the archives of the Minnesota Historical Society. This archive has been increasingly mined by archeologists (i.e., Arzigian and Stevenson 2003; Beaubien 1953; Benchley et al. 1997; Dobbs 1985; Finney 2000; Haury 1990, 1993; Peterson 1984; Wood 1978), not least because the data that Lewis assiduously recorded “can be used to reconstruct detailed drawings of sites that have [long] since been damaged or destroyed” (Dobbs 1991:5). Thanks in part to Lewis' (1898a) history of the NAS and Winchell's synthesis of the Minnesota data, Hill and Lewis to some extent escaped obscurity (Keyes 1928), even if knowledge of Lewis' fate and many details of the NAS (and especially the SAS) did not. More recently, though, archival and historical work by Dobbs (1991), Haury (1991, 1993) and Finney (2001, 2004, 2005a, 2005b, 2006) have revealed much about the scale and scope of NAS operations (and at least something of the SAS) and have further helped to secure the reputations of both Alfred J. Hill and Theodore H. Lewis.

THE NAS IN KANSAS

The NAS is not well known in Kansas for the simple reason that its activities barely grazed the state,

and as a result Lewis had no reason to publish on the work. According to his field notes, Lewis visited the Fort Leavenworth area in mid-May 1893 to search for the location of what he referred to as “Kickapoo Fort,” better known as Fort de Cavagnial, which was mentioned in the accounts of both the Lewis and Clark and later Stephen Long expeditions (Lewis 1893c). A few years previously, Lewis (1891) revisited the localities of three Native American sites in Nebraska and South Dakota that had been reported in the journals of the Corps of Discovery (Wood 1978). The search for Fort de Cavagnial was part of a secondary interest of the NAS, namely, early historic sites, principally forts and encampments associated with the expansion of Imperial France into the western Great Lakes and upper Mississippi River valley. Along the Mississippi River in present-day Trempealeau County, Wisconsin, Lewis excavated traces of what was long believed to be the post built by French diplomat Nicholas Perrot in the winter of 1685–1686 (Lewis 1889) but is now thought to be the 1731 encampment of Rene de Linctot. Moreover, from about 1876 to around 1900 in the context of the Southern Archaeological Survey, “Prof. Lewis ... made a special study of the Spanish and French explorations and settlements in the Mississippi Valley and the Gulf States” (Rowland 1902:449; compare Finney 2004). In other words, Lewis had considerable experience in locating and identifying such sites prior to his attempt to find Fort de Cavagnial.

In addition to his search for the fort, a letter to Hill from Fort Leavenworth also suggested that Lewis examined at least one possible aboriginal mound, “. . . a rock-heap which may be natural. There may be other mounds on Sheridan Drive [north of Fort Leavenworth], but if so I did not find them . . .” (T. H. Lewis to A. J. Hill, May 18, 1893). As early as 1830, the Reverend Isaac McCoy (1840) and several men from Fort Leavenworth explored a mound immediately north of the fort, and from the late 1870s through the 1880s there was a considerable amount of digging in others along both sides of the river from Kansas City northward (see Wedel 1943:12–14). As the Native American burial mounds in this stretch of the Missouri River were typically stone cysts covered with earth (Fowke 1922; Wedel 1943), it is at least possible that Lewis had come across one of these that

had been dismantled previously. However, most of his time seems to have been spent in search of Fort de Cavagnial and not Indian mounds. Had he truly wanted to find the latter, he certainly could have.

Fort de Cavagnial was established in 1744 “on rising ground overlooking the village of the Kansa Indians on Salt creek, opposite Kickapoo island in present Leavenworth county” (Hoffhaus 1964:425). As early as 1714, the French (including Etienne Veniard de Bourgmont) had conducted surveys along this stretch of the Missouri River as they maintained a compelling mercantile interest in the region. The stated purpose of the fort was, in fact, “to control the unruly Canadian voyageurs and couriers-de-bois in their fur trade with the Indians, to discover mines and minerals” in the region, and to establish a trail to Santa Fe. The fort was, in other words, “basically a trading fort, and the Kansa were customers, not enemies” (Hoffhaus 1964:426). In theory, as the principal trader was authorized to establish smaller trading posts and forts in the region, Fort de Cavagnial “was the commercial and military center of a vast territory in mid-America” (Hoffhaus 1984:60). The post remained active until its final abandonment in 1764, by which time much of it apparently had fallen into extreme disrepair through damages from a windstorm in 1752 and lack of funds to affect regular maintenance (Hoffhaus 1964:450). It did not help that in 1752 the Kansa relocated downstream near the confluence of the Kansas and Missouri Rivers. By mid-century the mounting influence of the British in the region, which culminated in the French and Indian War (1756–1763), was also becoming apparent. Tensions between the two empires probably resulted in disruptions in reprovisioning of the fort and the shipment of goods (Unrau 1971:67–69). Nonetheless, in 1757 Fort de Cavagnial traders shipped 100 bundles of furs—probably in the neighborhood of 9,000 pounds—downriver to New Orleans (Government of Ontario 1878:83).

According to available documentation (i.e., Nasatir 1990:36–37), Fort de Cavagnial’s infrastructure consisted of a square log stockade, some 80 feet across, with double-storied corner bastions. The interior ostensibly contained the commandant’s house, a guard house, a powder house, a trader’s residence, and a house for those employed by the fort’s licensed trader. All of the buildings were

constructed of logs, which were then plastered with mud. Doubtless there were also corrals and perhaps other ancillary structures, as well as gardens located in the vicinity (Hoffhaus 1964). Refuse middens also could be expected, though probably at a remove from the main compound.

Four decades after the abandonment of Fort de Cavagnial, William Clark wrote in his journal for Monday, July 2, 1804, that:

The french formerly had a Fort at this place, to protect the trade of this nation, the Situation appears to be a verry elligable one for a Town, the valley rich & extensive, with a Small Brook Meandering [meandering] through it and one part of the bank affording yet a good Landing for Boats [bracketed material in Moulton version] (Moulton 1986:341).

In 1810 Nicholas Biddle, who prepared the report on the expedition of the Corps of Discovery, in notes made following a visit with Clark, added for July 2, 1804:

As to the old village of the Kansas see *Du Pratt's Hist. of Natchez*. No remains of the village nor of the fort. The fort about a mile back of the town which it commands. The spring fine. Recognize the traces of the fort which was small. Some remains of chimnies. This party at the fort probably cut off as there seem no remains of them or history of their success (Jackson 1962:511).

In a letter to Biddle from that same year, 1810, Clark again referred to the site: "Part of stone chimneys where an old fort at a short distance from the upper village which was occupied by the French" (Jackson 1962:564).

Not to be confused with Fort de Cavagnial, on July 3, 1804, the Corps of Discovery did stop, according to the journal of John Ordway, at "an old Trading house built by a French merchant from St. Louis to Trade with the kansars Indians" (Moulton 1995:19–20). This post was located several miles farther upstream (Barry 1972:49) and had been deserted for sometime.

Fifteen years later the expedition of Major Stephen Long traversed the same stretch of the Missouri River. The expedition's chronicler, Edwin

James (1966:110) mentioned the site almost in passing:

The site of an old village of the Konzas, and the remains of a fortification erected by the French, were pointed out a few miles below Isle de Vache.

No other details were forthcoming. In 1833 Prince Maximilian of Wied (1906:256), in the course of his travels in the interior of the continent, also apparently alluded to the site (Hoffhaus 1984:87). However, knowledge of who had constructed the fort had become garbled, and the prince mistakenly attributed it to the Spanish.

Despite Captain Clark's supposition that little was known of the fort, Hoffhaus (1964, 1984) compiled a great deal of information about it, much of it gleaned from French archival sources (compare Nasatir 1990). Most historians and archeologists (e.g., Coues 1893; Hoffhaus 1964, 1984; Moulton 1986:343; Wedel 1959:33; Witty and Marshall 1968) have suggested a location for the fort somewhere a few miles north of Fort Leavenworth and southwest of Cow Island (Isle de Vache). The inferred location would put it somewhere near the confluence of Plum and Salt creeks, in northern Leavenworth County. Plamondon (2000:Map 45) and Harlan and Denny (2003:Plate 15), however, place the fort squarely in southern Atchison County. In particular, Plamondon situates it on Walnut Creek near present-day Port Williams¹.

Perhaps spurred on by the publication of the latest edition of the Lewis and Clark journals (i.e., Coues 1893), T. H. Lewis traveled to Kansas City sometime in mid-May. On May 16, he commenced his search, informing Hill that

Now that I am here will look the ground over well and see what I can find. The great trouble is to find "two high points of land" at the mouth of a valley for the land generally slopes down gradually to the creek [river?] valley or is merely plateau land. There are two high points in many places on the northside, but I will disregard the bluff description and see what I can find otherwise. I may conclude to take a skiff by train to Atchison and drop down the river (T. H. Lewis to A. J. Hill, May 16, 1893).

Interestingly, it appears that Lewis may have employed a pseudonym while working in the area, as an article in *The Atchison Champion* for May 20, 1893 proclaimed that,

H. Huhlenberg, a prominent archeologist of Wisconsin has been exploring Kickapoo and vicinity for several days in search of the remains of an old French fort which dates back to 1804 [sic]. Mr. Huhlenberg was informed that the site of the fort is at the junction of Salt and Sand creeks . . .” (quoted in Witty and Marshall 1968:13).

Lewis is known to have used a pseudonym on at least one occasion in conducting personal business on behalf of his employer (F. A. Finney, personal communication, September 2008). That he did so on this occasion is conjectural, but the dates fit and the likelihood that two archeologists from the upper Midwest were in the area at the same time searching for the same site seems unlikely. Lewis made no mention of crossing paths with any other archeologist in his notes or his May 18 letter to Hill. Probably Lewis made the decision on the spur of the moment because there is no mention of using an alias in the May 16 letter to Hill. As to why, Lewis evidently did not want it known that he was in the area. Fred A. Finney (personal communication, September 2008) suggested one possibility: Lewis was extremely upset by editorial liberties taken by the Reverend Stephen Peet in *The American Antiquarian*; Peet was the founder and editor of that influential journal. In the spring of 1893, Lewis indicated to Hill that he was no longer interested in publishing in the journal. Perhaps concerned that Peet might see any news article that appeared or eventually hear of it and either solicit an article from Lewis or, barring that, publish a short article noting that Lewis had failed to locate the fort, Lewis resorted to use of a pseudonym².

From May 16 through May 18, 1893, Lewis surveyed areas that appeared to match the landform described in the journals of the Corps of Discovery, apparently concentrating on an area above the Missouri River floodplain between Salt Creek and the town of Kickapoo. He also interviewed old settlers and studied plat and other maps at Fort Leavenworth. From these, Lewis (1893c:8) quickly realized that:

The river at the mouth of Plum [Salt] creek is only about 100 feet from the RR track, and less than that above the mouth. A new sand-bar—formed within two years—extends for ½ mile below Kickapoo . . . Formerly there were about 1000 or 1500 acres of high bottom land—(not over flowed) in front of Kickapoo Station which was washed away about 30 years ago, according to the statements of the old settlers and the County Atlas.

Even as early as 1804, Captain Clark had observed that the river was cutting into its bank in front of the purported location of the Kansa village (Moulton 1986:340). A comparison of Plamondon’s (2000:Map 44) reconstruction of the course of the channel of the Missouri River at the time of the expedition with its modern course bears out Lewis’s findings: the river made a substantial shift to the western side of the valley in front of Salt Creek.

Of the specific location of the fort, Lewis noted that

About 1¼ miles from the railroad, where it crosses Plum [Salt] creek valley, there is an old road crossing. On the south side of the creek there is a plateau and on the north side at the edge of the creek bottom—which is low—and near the old road, there is a spring but a very poor one. There is no place nearby on the north side for a fort, and it is doubtful about there being one on the south side, because the creek is hard to cross. The old crossing was made many years ago by settlers hauling rock to the place and dumping it there to make a solid bottom (Lewis 1893c:8–9).

As to the fort, if it was back in a valley at all, it was not intended to command the valley. If it was on the bluff, it would have been so described, which would make it either above or below the valley. In order to command the river cannon would have been necessary, and even then they would not have been able to stop boats at night or in high water.

Theory. As the Missouri river flows in all directions across the valley, and the rear of the camp may have been in any direction from straight back, to an angle to the right or left, it is probable, if there is any truth (beyond guesswork) in the statement that there was a fort in this vicinity, that it was located near the spring at Kickapoo station. If this

was the point, then it was located nearly in front of the depot on a small plateau (Lewis 1893c:10–11) (see Figure 3).

By the end of his survey, Lewis (1893c:10) expressed doubt that either the Lewis and Clark or Long expeditions had actually seen the fort, suggesting instead that “they probably wrote from ‘hearsay’.” The James’ account would support this contention, while the case for Captains Lewis and Clark is more ambiguous. There was no specific mention of the fort in the journals, but Clark later related additional information to Biddle, which was then interpolated into many subsequent versions of the journals. Gary E. Moulton (personal communication, September 2008) agrees that if Clark or any of his immediate party had visited the ruins, it would have been more clearly stated in the journals. No doubt, as T. H. Lewis suggested, Clark was relying on secondhand information, the Corps of Discovery venturing no nearer the old fort than the river. Probably, the information was supplied by French members of the Corps, in particular George Drouillard (Moulton 1986:342). As for Clark’s later mention of stone chimneys, according to French sources, these were supposed to have been made of mud-plastered logs, just as were the buildings (Nasatir 1990:36–37). This detail suggests that, upon hearing of chimneys, he may have erroneously concluded that they were made of stone.

Lewis’s search for Fort de Cavagnial ultimately failed. Other than a few bullets, scraps of lead, pieces of rolled brass and copper plate found on the north side of Salt Creek and chipped stone debris in proximity to an “Indian” spring in the same area (see Figure 3), he (Lewis 1893c:10) found nothing to suggest the presence of a fort as substantial as Fort de Cavagnial was said to be or should have been based on 20 years of occupation by a small garrison and probably a fluctuating number of traders, trappers, and Native Americans (Hoffhaus 1964, 1984:63, 65; Nasatir 1990)³. To Hill he wrote, “I have neither found any remains of a french fort or a large spring in any of the valleys—back from the Missouri river” (T. H. Lewis to A. J. Hill, May 18, 1893). His field notes suggest that he leaned toward the belief that the site had long since washed away, though he also expressed skepticism that it

was ever actually located in the survey area, as he had been led to believe.

Its negative results aside, Lewis’ 1893 survey indicates the lengths to which Hill and Lewis were willing to go in search of the physical traces of the early European colonization of the greater Mississippi valley. As for the location of Fort de Cavagnial, notably subsequent archeological survey of the general area around Salt Creek in the early twentieth century (Remsburg 1911) also came up dry, as did more intensive survey and photogrammetric efforts in the early and mid-1960s (Hoffhaus 1964:454, 1984; Witty and Marshall 1968). Witty and Marshall (1968:60) finally concluded “that the Missouri river in shifting its channel has washed away the remains of both sites [i.e., Fort de Cavagnial and the Kansa village].” By the time of Lewis’ survey, the river had undergone major changes in its course, and the town of Kickapoo had sprung up along the banks of the suspected location of the fort. Together with the difficulty in matching the vague descriptions in the journals of the Corps of Discovery with modern landforms, these have only complicated the search.

ENDNOTES

1. Plamondon (2000:7) notes that, “On the *Lewis and Clark Trail Maps*, expedition campsites and Indian burial grounds and villages are shown at exact locations only if these places are properly protected and supervised. All other sites are only generally located.” The site of Fort de Cavagnial would certainly have to fall into the former category, but it is unlikely that he shifted it greatly from its inferred location; i.e., from Plum/Salt creek area in Leavenworth County to Walnut Creek in Atchison County, Kansas.

2. However, Lewis, after leaving the Leavenworth area, ventured immediately to Saline County, Missouri, to search for Bourgmont’s Fort Orleans (F. A. Finney, personal communication, September 2008) and while there made no effort to conceal his identity. A decade later his visit to the area was still warmly recalled (The Circuit Rider 1904a, 1904b). Newspaper articles in the local press in both areas stood an equal chance of being picked up by the Kansas City newspapers, which would have greatly increased the chance of the Reverend Peet hearing of his activities. This at least raises the possibility that Lewis became aware of one or more persons in the Fort Leavenworth neighborhood from whom, for

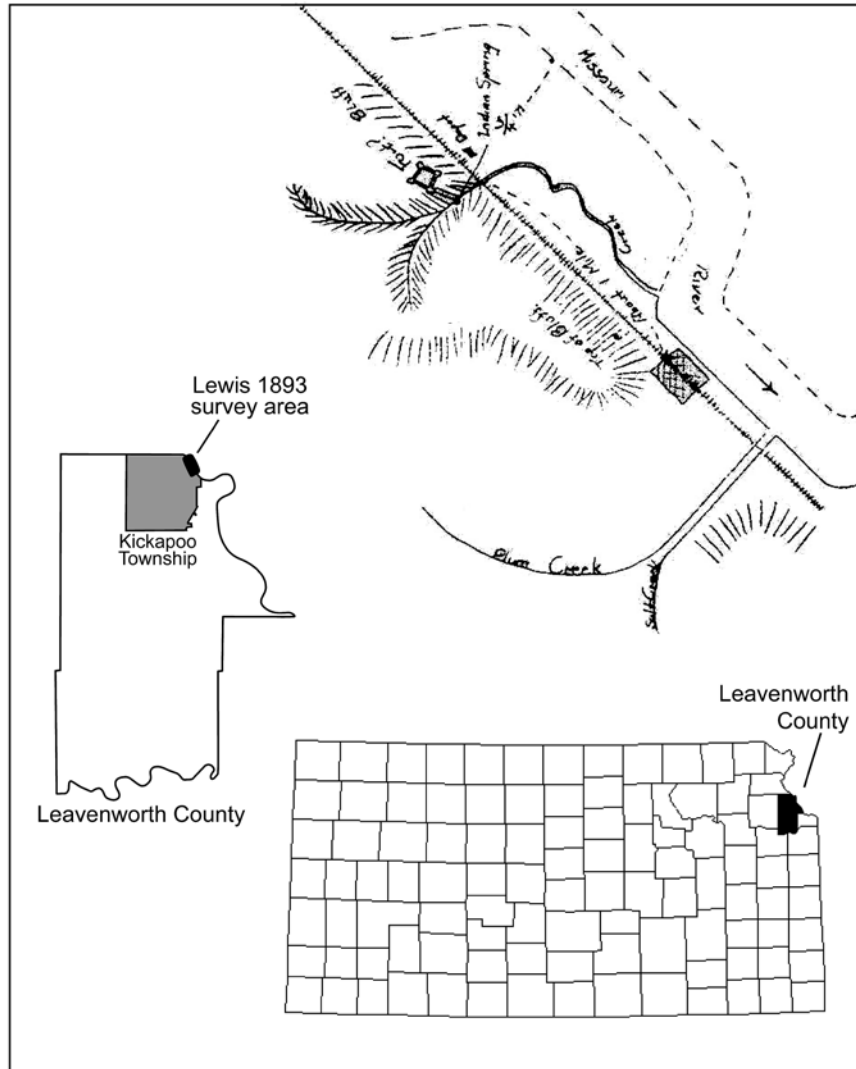


Figure 3. The NAS Leavenworth County Survey Area in 1893 (top), from Lewis' Unpublished Field Notes. Note that Lewis' schematic is not to scale and north is approximated. (T. H. Lewis map courtesy of Minnesota Historical Society, St. Paul.)

whatever reason, he wished to hide the nature of his activities.

3. Wherever the site is located (and if it has not been destroyed), it has to be considered that its actual layout and physical character may have diverged significantly from the 1743 official declaration of *how it was to be built*. As an instance of this, excavation at Fort Michlimackinac in Michigan in the late 1950s quickly revealed that the fort depicted on historic plans bore little semblance to what lay buried beneath the surface (Thurman 1998:49). Indeed, such differences could account for some measure in the discrepancy between the 1743 declaration and the only known description of the fort, made in 1758, as “a circle of piles’ enclosing ‘some bad

cabins and huts” (Hoffhaus 1964:450) (attributed to damage and insufficient funds for maintenance). Too, it could account for the discrepancy between log and plaster chimneys and the stone chimneys reported by Clark unless, as suggested in the text, this information was the result of a mistaken assumption. For that matter, it could also be a factor as to why the site has yet—as of this writing—to be conclusively identified.

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WITCHING FOR FAILURE: GRAVES AND GROUNDTRUTHING

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“Witching” for graves does not work. During my many years as an archeologist investigating burials, I helped to excavate 75 “really good” graves that were found by 15 or more witchers in 10 different Kansas locales, and I found nothing at any of them. Several other archeologists did the same thing. Basically, the claims of grave witchers are not substantiated by groundtruthing.

Witching for graves is a strange thing. Based on my experience, it is also totally ineffective. Nevertheless, there are a surprising number of people who believe in witching as a valid and dependable way to locate graves.

Witching is also known as divining or dowsing, although the latter term is more appropriately applied to a search for underground water. “Witching” definitely seems to be the right word to use when looking for graves because it brings to mind sorcery and magic often associated with dead people.

Witching is actually a very simple technique: the witcher generally holds long L-shaped metal rods in his hands (Figure 1) and carefully walks back and forth over what is believed to be a burial ground until the rods cross in front or swing to the side (there is considerable variation in how people do it). One way or the other, if the rods do the “right” thing, the witcher claims that he has found a grave. And more than that, many practitioners firmly believe that witching can tell them the depth at which the body was buried, whether the individual was a man or a woman, an adult or a child, and even how the body was arranged, such as with arms folded together. Strangely, witchers are not always totally sure of their finds. For example, I have heard them vacillate between saying that one spot is “really good” (meaning that they have no doubt as to its existence) and that another spot is more “iffy” (meaning they are not so sure). But basically, witchers think they can tell you everything.

Unfortunately, in my experience virtually none of their claims are ever groundtruthed (verified through excavation), so witchers go unchallenged. Because newspaper reporters love to tell their stories, several long-time witchers in Kansas have become highly regarded as “experts” and are asked to locate unmarked graves. In many instances the “lost” graves are in remote locations or neglected rural cemeteries where gravestones have been removed; however, city cemetery officials also use witchers to look for unmarked graves in actively used cemeteries. Naturally, the witcher’s pronouncements are regarded as the gospel truth—which can be a real problem if the cemetery sells you a plot and then discovers that the witcher “missed” a grave that is right in the spot you bought.

A few archeologists have been able to ground-truth witchers’ claims. Burial investigations—both historic and prehistoric—were part of my job ever since I started as an archeologist with the Kansas State Historical Society (KSHS) in 1979. In recent years I was even more active in this line of work due to my role as case investigator for the Kansas Unmarked Burial Sites Preservation Board, which is known more informally as the UBS Board. As an avowed scientist, I never believed in witching. Beginning in the late 1980s, I came into contact with witchers who were absolutely adamant that they could find unmarked graves. Seeing no reason why it would be a problem, I decided to use them—not only because they were available at no cost to me



Figure 1. Witching in progress in the uplands near the Hollenberg Pony Express Station in Washington County on June 25, 1998.

or to the state and were practically begging to help, but also and more importantly, because I realized I would be able to groundtruth at least some of their claims.

To make a long story short, my scientific beliefs have been totally vindicated: I have never found a burial in any of the places that witchers told me graves were present. More specifically, I have never found a burial in those witched places that I was able to investigate by groundtruthing, which was done through either excavation or soil core probing. I certainly have not checked every place that I was told about, but that is because I did not have the time, equipment, or permission to excavate in those locations. Of course, that is one of the main problems with groundtruthing: you are not able to check every witched grave. Luckily, some of the witchers were so eager to prove their witching abilities that they provided the equipment to check things out.

To sum up the situation, I am able to say that after nearly three decades as a KSHS archeologist, I

personally have groundtruthed 75 so-called graves that were found by 15 or more witchers in 10 different Kansas locales, and I found nothing at any of them. Of those 75 spots, 34 of them (in two localities) were checked with an Oakfield soil sampling tool, which is a hand-operated T-shaped metal probing device that is pushed into the ground and then pulled out to produce a soil core about an inch in diameter and a foot in length (Figure 2). The other 41 graves were excavated with a shovel, a backhoe, or (for two graves on one occasion) a tractor-powered auger. Rest assured, for all those 41 graves, I dug deep enough to be certain that nothing was there. The same is true for the 34 graves that were probed with an Oakfield, although the soil sampling at one of those two localities was not quite as good as I wanted (see the description of the so-called “Black cemetery” in Blue Rapids below).

Many of these investigations were somewhat sudden or unplanned. Because I was usually on my own and in a hurry to get things done (in addition

to being constantly badgered by local people), I was not always able to record everything in complete written detail, nor was I able to take many useful photographs. For some of those situations, my memory is just about the only record that exists of the investigation, so I am writing this report to record those details properly. Several of my investigations were described in progress reports that were written and placed in the KSHS archeological site files, while other such reports (sometimes just scribbled notes) were placed in the “UBS case” files that the KSHS Archeology Office maintains for the UBS Board. For some investigations, though, there was no site file and no UBS case file, so the all-purpose KSHS Archeology Office county file or individual cemetery file was the only place to stick the progress report, if indeed a progress report was produced, which was not always the case. One of my investigations, however, did result in a formal written report (Thies 2003), although it was an unpublished “gray literature” report produced for the Kansas Department of Transportation.

Not surprisingly, even though all of the “graves” that I checked produced nothing, the investigations were quite revealing in terms of the mindset of witchers. Witchers firmly believe in what they do, even when nothing is found. In fact, sometimes the witchers try to pass the buck back to the archeologist, claiming that the situation was not thoroughly checked out. That was not the case with me, however.

What follows, then, is a descriptive list—actually, more like a bunch of really strange stories—of the encounters that I have had with witchers whose claims I have been able to groundtruth.

1. My first experience with a witcher took place in 1989, when a Geary County resident, Chuck Olmstead, informed the KSHS that he had found “thousands” of graves by witching and that he could identify each individual’s gender, age, burial depth, body orientation, and body arrangement



Figure 2. Oakfield soil sampling tool, displaying subsurface soil strata.

(whether the hands were folded, etc). He further claimed that the graves were scattered all over the hillsides near the “many trails” that supposedly passed through the Flint Hills region. He earnestly explained that the graves were marked by stones that were set on end, sticking up out of the ground. Later, as we drove through the Flint Hills on our way to check things out, he pointed out the supposed gravestones. It was quite obvious to me that his gravestones were nothing more than natural slabs of rock that had fallen down from the edges of the limestone formations that form the many ridge tops that make up the Flint Hills.

We could not check all of the graves Olmstead had identified, but we needed to excavate at least one so that we would know whether his witching was valid. Because I did not want to expend a lot of effort in digging and he firmly believed that he could determine the depths of the burials, I had him pick out a “really good grave” that was as shallow as possible. Luckily, that turned out to be one that was only a foot below surface. Although that seemed much too shallow to me, it was Olmstead’s witching assessment, so I did not argue with him about it. We used shovels to dig down as deep as he claimed the burial to be, and then a little deeper, and found nothing other than undisturbed subsoil. Even this did not deter him, and he pointed out little limestone fragments that he thought might be decayed bone. It took a while to convince him that we had found nothing, but he eventually agreed (somewhat halfheartedly) that nothing “appeared” to be there, and therefore we need not continue the excavation.

After backfilling the excavation unit, I noticed that there was an old township cemetery nearby and realized that it offered an opportunity to see whether Olmstead could actually determine the genders of buried individuals. The grave marker inscriptions all faced away from the graves, so he could not see the names when he witched the graves, providing a “blind check” situation. He quite confidently stated the gender of each individual, and, as it turned out, he was about 50 percent correct, which was not surprising, given the fact that he had a 50–50 chance of identifying each individual as a man or a woman. In actuality, the witching associated with his gender identification attempt was a failed effort, just like the supposed grave that we investigated earlier.

2. My second witching adventure occurred in 1993, when I discovered some historical documentation that suggested that the oldest White cemetery in Shawnee County might be located northeast of the old Potawatomi Baptist Mission, which is part of the KSHS History Center complex in Topeka. There were no surface indications of a cemetery, but earthmoving was underway in this area, so I was concerned. A local Topeka witcher, Rick Knight, overheard me discussing this matter and eagerly offered to come out and find the graves, so I took him up on his offer. He promptly and quite confidently witched 20 putative graves that were oriented east-west and arranged in four north-south rows, as would be appropriate for any historic cemetery.

Tom Witty, State Archeologist from 1960 to 1994, and I then used Oakfield soil sampling tools to probe each of these graves to a depth of 60 cm below surface. As a double check, we also probed to a similar depth in the surrounding areas that were believed to be undisturbed. There was no evidence of disturbed soil in the putative grave locations; the soil was identical to that in the surrounding areas, which graded quickly from a 10-cm-thick dark brown topsoil into a light grayish tan clayey subsoil. Wanting to go one step farther, we had the witcher select what he felt to be “the best” grave and dug a north/south-oriented trench through the center of it. The trench (see Figure 3) was 45 cm wide, 160 cm long, and extended to a depth of 40 cm below the surface, which was well into the subsoil. There was no evidence of a grave shaft or any soil disturbance. We then used the Oakfield tool to probe even deeper in the trench, to a depth of 100 cm below surface, and still there was no evidence of any soil disturbance.

Later we discovered the reason for the lack of graves. The historical documentation was misleading, and the cemetery was apparently located somewhere else. Lesson learned: If you want witchers to find a grave, just pick a spot and tell them, “There’s supposed to be a cemetery here.” They will find graves even though nothing is there.

3. In 1998 I was contacted by members of a local organization of the Oregon/California Trail Association (OCTA), based in north-central Kansas. The OCTA “rut nuts” had witched what they believed to be an unmarked trail cemetery on a prominent alluvial terrace in the Cottonwood Creek valley near

the Hollenberg Pony Express Station in Washington County. They wanted to excavate some of the graves to verify their findings. Because they had no physical or historical evidence of a cemetery and witching was the sole basis for believing that they had found graves, I had no reason to keep them from excavating—especially as I anticipated that their witching would produce nothing but “dry” holes. I did warn them, however, that if they found any identifiable human burials, they had to stop immediately and notify me, as well as the local law enforcement officials.

The OCTA members realized that the best solution would be to have a professional archeologist on hand to identify the burials, so they asked that I be there, serving in my role as case investigator for the UBS Board, to monitor their efforts. I agreed to do so. When I arrived, three or more witchers (probably several, actually, because almost everybody in the group of about a dozen people seemed to think that they were good at witching) had already witched and marked the location of 55 putative

graves. In addition, somewhat strangely, they had witched a portion of the adjacent uplands alongside the Oregon/California Trail and found yet another grave. They were eager to dig, so I showed them what to do and then monitored as they used shovels to dig trenches into the one upland grave and then the putative cemetery’s four “best” graves (see Figure 4). The upland excavation was taken to a depth of about 2 feet, which was well into obviously undisturbed subsoil. The four other excavations were taken to depths of 3 to 4 feet into the alluvial soil, revealing identical sets of undisturbed soil strata, including a distinctive gravel layer. All of these efforts provided negative results, forcing the OCTA members to admit—reluctantly—that this was a fruitless effort. In other words, their witching was a failure.

4. In 1999 I volunteered to assist a Marshall County Historical Society member in investigating the purported location of an abandoned Black cemetery in Blue Rapids, after witching had been carried out by Mike (“Doc”) Lind, who was one of the



Figure 3. Trench excavated at 14SH325 on August 12, 1993.

witchers involved in the investigation of the supposed Hollenberg Station cemetery. Lind showed up at the site while we were there, and I was rather gratified to hear that, because of the Hollenberg failure, he now had doubts about the validity of witching.

The cemetery that we were there to investigate was said to be located in a vacant lot that reportedly had been used at some time in the past as a hog lot or pig farm. Supposedly, 30 or more graves were present in this area. We had Lind witch the area again and pick out “the best” graves. Some of these were in rather strange locations, oriented north-south (rather than the normal east-west direction) and located surprisingly close to the edge of the street that bordered the north edge of the vacant lot. I then used an Oakfield soil-sampling tool to probe 15 of those locations to depths of 60 cm below surface. The probing produced no evidence of disturbed soil that would indicate the presence of graves. The soil was not particularly conducive to the investigation as it maintained a single dark color throughout; however, it did have an obvious textural change from a friable topsoil to a clayey subsoil. While it was not the best situation for soil core probing, I certainly found no evidence of graves. Subsequent historical research produced no definitive evidence that a cemetery had been located there. It is also important to note that a gas line had been excavated along the north edge of the vacant lot, just south of the street, and no evidence of graves was encountered at that time.

According to an elderly gentleman who lived in a house just across the street, a Black child was buried at the northwest corner of the vacant lot at some time in the 1920s, and then a cedar tree was planted next to the grave. The tree is still there, but the resident was unsure whether the grave was north, west, or south of the tree. A somewhat scattered but slightly rectangular cluster of stones to the north of the tree suggested the presence of a grave in that particular location, but soil core probing produced no evidence of a grave there or anywhere else around the tree. Assuming that a child was indeed buried here, it may be that the oral history associated with this one grave is the reason why some of the local people believe that this location is the site of a Black cemetery.

5. In July 2001 I entered into a formal contract

to monitor a backhoe investigation at the Pilsen Catholic Cemetery in Marion County. A burial plot had been sold recently to someone who needed to determine whether anyone was already buried there. A question had arisen with regard to the possibility that burials had occurred there in the past. The location of the plot was in the “Devil’s Corner” section of the cemetery, where stillborns and suicides supposedly were buried. The question about the presence of previous burials derived in part from the efforts of a local witcher named Bill Benda, who walked over this area, witched the location of seven graves, and stated that they contained three adults and four children or infants. Despite the confidence he had in his results, nothing was found in any of his witched graves when they were checked by a backhoe that dug down to a depth of 6 feet or more below the surface.

6. Later the same year, in December 2001, I again was contracted to monitor a backhoe excavation in Jewell County near Mankato by Ray Vincent, a Topeka area man who had obtained permission from local authorities to move his great-great-grandfather’s remains to a nearby church cemetery. His ancestor’s grave marker stood by itself in the corner of a cultivated field, and Vincent naturally assumed that the grave was located right next to it on the east. He witched this location, and although he did not have a really “strong” hit with his rods, he did think the rods indicated that he was on the right spot. A backhoe was used to excavate the area to a depth of more than 6 feet below the surface, but nothing was found.

In retrospect, we now suspect that the landowner had earlier moved the grave marker from its original location somewhere farther out in the field, so that he would be better able to farm that area without having to work around the stone. This inference is based on an earlier discussion that Vincent had with the landowner, who seemed a little distant and uneasy when talking about the topic.

7. My favorite example of a witching failure took place on December 19, 2002, when I monitored the excavation of 18 so-called pioneer graves at Shibley Knoll. Historical documents indicated that several early day emigrants had been buried in the vicinity of this major stopping place along the historic Oregon-California Trail in the 1850s. Shibley Knoll is a steeply sloped alluvial terrace ridge



Figure 4. Excavations in progress on the alluvial terrace near the Hollenberg Pony Express Station in Washington County on June 25, 1998.

that projects prominently above the floodplain on the east bank of the Big Blue River valley at the west edge of present-day Marysville. Because this location was part of a hot-topic highway project, the Kansas Department of Transportation (KDOT) provided a backhoe to aid in the investigation and resolve the issue of whether anyone was buried there in the pathway of the project. I was the person in charge of the archeological portion of the project and therefore was required to produce a written report describing the investigation (Thies 2003). As far as I know, it is the only such report that has been produced pertaining to witching for graves in Kansas.

Historical documentation was lacking with regard to the precise location of the graves. The only reason the investigation took place was that someone in Marysville had asked two “expert” witchers from Blue Springs, Nebraska, to witch the location of the graves. The witchers (Bob Allen and Marlin Weiner, according to the Marysville newspaper)

walked over the area and marked the locations of 3 graves in the south part and 12 farther to the north (see Figure 5). Their discovery was bolstered by a man (unknown name) who overheard them talking about their witching abilities in a local Marysville café. He thought that he “could do that too,” so he did, finding 3 more potential graves in addition to the 15 that Allen and Weiner had already located. All three of the graves found by the Marysville man were located in a row on the floodplain at the west end of the north set of graves that had been located by the Nebraska witchers (see Figure 5).

When I arrived at the site, all of the witched graves had been marked with spray paint or pin flags, and the Nebraska witchers had gone one step further by marking their graves with small wooden stakes that had tags of paper on them, noting “possible Pioneer grave.” From the start, it was apparent to me that the arrangement of graves as marked was utterly ridiculous; all except one were placed tightly together in long east-west lines, closely

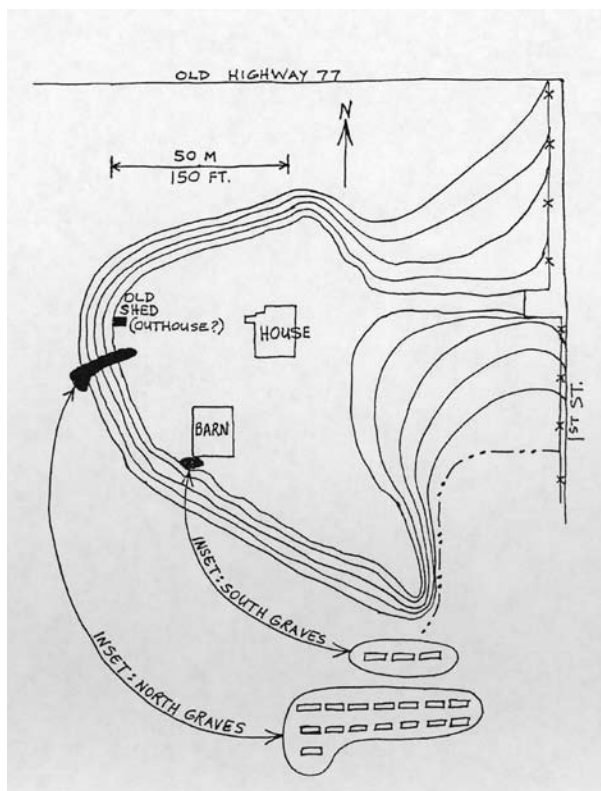


Figure 5. Map with insets showing the location and arrangement of the “graves” investigated at Shibley Knoll on December 19, 2002, along with the former location of various structures. Topographic lines are approximate and indicate 2.5-foot intervals.

spaced and literally head to toe, rather than the side-by-side north/south-oriented widely spaced row arrangement that is common in most cemeteries. In addition, the landscape setting of the north set of graves was even more unbelievable, because the two lines of graves ran right up (or down) the side of the alluvial terrace, from the flood-prone bottomland to the top of the terrace (or vice versa), which is definitely not the sort of arrangement you would expect to find in an old cemetery.

A host of city and county officials, media reporters, and KDOT workers were on hand to witness the event. Despite the obvious incongruities, we fired up the backhoe and proceeded to carefully excavate the marked spots, each to a depth of 6 feet or more below surface. All of the holes were carefully examined as the soil was removed, and in some cases the walls of the excavations were inspected with the use of a trowel. The north set of graves located along the terrace edge produced nothing but

the expected: undisturbed alluvial soil. However, to my complete surprise, the excavation on the south exposed a trash dump—an area of deep modern-day fill composed of dirt and debris placed there in the 1950s or 1960s, apparently to expand or raise the top of the terrace.

Once again, I felt vindicated by the fact that nothing was found. Nevertheless, several months later I was contacted by someone at the Beatrice, Nebraska, newspaper office and was astounded to learn that the two Nebraska witchers were discounting my results. They claimed that no one really watched very carefully during the excavation, thus suggesting that through incompetence and lack of attention the burials were missed. Obviously, they just could not accept the fact that their witching was worthless. In retrospect, this situation seems to epitomize all the sadly amusing aspects of witching: anyone can do it, even a first-timer from a local café; graves can be “found” in silly and illogical locations; and when nothing is discovered, even through carefully monitored deep testing, witchers can come up with an excuse as to why it all went wrong.

8. On October 20, 2004, I assisted in an investigation of the purported gravesites of Frank Alderdice and John Daily, two boys who were killed by Cheyenne Indians in 1869 and reportedly buried by their father on the south side of the Saline River near Beverly in Lincoln County. Their graves, which were supposedly lying side by side (and were oriented north-south rather than the normal east-west), had been located by witching, carried out by a well-known local witcher, Lee Modrow, and one of his local witcher friends, Lowell Larson. With several people working together, we used shovels to excavate a trench through the purported graves to a depth of 4 feet below surface (see Figure 6), which was about the normal depth of most historic burials. Nothing was found, and the soil stratigraphy clearly indicated that the area had never been disturbed. All parties agreed that no graves were present, no graves had ever been there, and any further work would be a waste of time, so the excavation was abandoned and the trench was backfilled.

Despite the fact that nothing was found, a few days later those involved in the witching attempt proceeded to carry out a funeral and supposed reburial for the two boys. The event was orchestrated

by various members of the Lincoln County Historical Society, assisted by Jim Gableman, the Hall Funeral Home director in Lincoln, and it drew in a crowd of nearly 60 people along with a television crew and several newspaper reporters. Although the event was a pretense, the actual facts of the matter were apparently not made known to everyone. This resulted in a very confusing situation whereby many local people and apparently even some newspaper reporters believed that the remains of the boys had actually been found and were being reburied.

For example, an article in the Sunday (October 24, 2004) issue of the Salina Journal led off by stating that the remains of the boys were relocated to a local graveyard, but then contradicted that information by quoting Gableman, who said that the event was more of a memorial than an actual burial. The article also commented, "Earlier in the week, the experts [i.e., the witchers] found what they presumed to be the boys' graves, but an archeologist was unable to positively identify them." Since I

was that archeologist, I can confirm that this account was true but totally misleading, suggesting that the graves were found but that I was not able to identify them in a positive way. Actually, I had positively identified them, so to speak, but in a clearly defined and definitely negative way. More specifically, I clearly determined that nothing was there and no excavations had ever taken place there. The article also stated, "It's not believed that the boys were put in boxes or caskets at the time, so it's not surprising little, if anything, remained of the graves after 135 years." As an archeologist, I know this to be an incorrect understanding of the situation. Skeletal remains of people *not* buried in boxes usually survive quite well and last for many years, hundreds or even thousands of years, actually. On the other hand, boxes and caskets typically cause bones to deteriorate much faster than a non-box burial. In other words, we probably would have found totally intact skeletal remains if the boys had been buried without caskets.



Figure 6. Excavation in progress in the search for the graves of the Alderdice and Daily boys in Lincoln County on October 20, 2004.

Interestingly enough, this was the second time that Modrow and Larson are known to have failed in their witching. In 2001 they used their witching talents in an attempt to find Frank Alderdice's mother Susanna, believed to have been buried near Summit Springs in Colorado. The witchers "both located what they positively felt was a grave," which was then shovel excavated twice—the first time to a depth of 8 feet below surface and the second time (apparently the first time was not enough) to a depth of 12 feet. They found nothing and eventually "gave up" their attempt (Broome 2003). Long story short, despite Modrow's supposed success in locating numerous unmarked graves, his claims have only been groundtruthed twice, and on both those occasions nothing was found, indicating that his witching was a failure.

9. On May 25, 2006, I assisted in investigating the purported Hinshaw Cemetery, which supposedly was located north of Sylvia in Reno County. The cemetery was said to have been used in the late 1800s by members of the Hinshaw family and was thought to contain 10 to 18 graves. A Hinshaw descendant, Helen Bridgeman, witched the location of 12 graves in the cemetery area and, strangely, one other in a group of trees just outside the cemetery area. (Bridgeman identified the latter as "an Indian" because "well, they were here, too, weren't they?") In the purported cemetery, six graves were investigated by subsurface excavation, four with the use of a backhoe (Figure 7) and two with a tractor-powered auger. Nothing was found, and the excavations clearly indicated that the area had never been disturbed. Naturally, I wanted to have more groundtruthing carried out on the remaining graves, but by then Bridgeman realized her witching was a failure and called the whole thing off.

10. My last investigation took place in Gray County in 2007 and involved a supposed child's grave at a Santa Fe Trail campsite known as "the Land of Many Ponds." The landowner, Dale Eichenauer, had witched five supposed graves around his farmhouse. He then picked one that he thought contained a child, dug a small child-coffin-sized hole to a depth of 3.5 feet, and then stopped because he thought he had found a bone, which he later realized was just a tree root. Further excavation, undertaken by the two of us, first with an electric auger and then with a shovel, permitted an

examination to the depth of 6 feet deep. Nothing was found, and the excavation and soil stratigraphy clearly indicated that the area had never been disturbed. Once again, the witching was a failure.

Those are my experiences, but there is even more to be told with regard to witching failures encountered by KSHS archeologists. In 1989, for example, KSHS archeologists Tom Witty and Martin Stein investigated four witched graves at the Ehmke site (14LA311), which is a multicomponent prehistoric site in western Kansas that was then undergoing deep testing in a search for a Paleoindian component. Earlier on the day that Witty and Stein arrived at the site, four supposed graves had been witched by Albert Wegele, "the Dean of western Kansas witching" and a farmer from the Otis, Kansas, vicinity. According to Witty, in a discussion we had on September 7, 2007, Wegele's graves were oriented north-south and grouped closely together, with two graves on the north and the other two immediately to their south. Witty and Stein promptly investigated the witched grave sites with the use of Oakfield soil sampling tools, and then dug a trench through one of them. Nothing was found, and there was no evidence that the ground had been disturbed. If this example is added to my list, KSHS archeologists have groundtruthed 79 so-called graves that were found by 16 or more witchers in 11 different Kansas locales, with 42 of those graves being excavated with a shovel, backhoe, or auger and 37 tested with an Oakfield soil sampling tool. To reiterate, nothing was found. And yet, perhaps not surprisingly, there is more that can be added to this story. In addition to his witching for graves, Wegele had also witched five supposed tepee locations at the Ehmke site. Soil core probing by Witty and Stein revealed no evidence of anything archaeological in those spots. Wegele's witching, in other words, when groundtruthed, was found to have produced nothing.

This lack of success in witching seems to be the situation in many other places, based on information that I gained recently. In November 2005 the Kansas State Archeologist put a request on the ACRA-L listserv asking for information about the experiences of other archeologists who had



Figure 7. Backhoe being used to search for graves at the purported Hinshaw Cemetery in Reno County on May 25, 2006.

groundtruthed witched graves. This elicited several responses, and they were all the same: nothing was found. I also learned that many witchers were perplexed by the negative results—and sometimes they outright objected, saying that something must have been done wrong. In fact, in one case the witcher was convinced the archeologists had secretly stolen the bones!

Most of the reports were anecdotal emails, but also received were two written archeological reports, although they are only unpublished letter reports. One described a situation in Wisconsin where a research archeologist from the Mississippi Valley Archaeology Center investigated five witched graves with the use of a backhoe and found nothing (Holtz-Leith 2004). The other report pertains to a similar situation in Maryland where the president and principal archeologist of a private company (Applied Archaeology and History Associates, Inc.) searched for four witched graves using a Gradall that excavated over 240 square feet of

land but found nothing (Ward 2004). In both cases the excavations reportedly extended into the subsoil to a depth that was sufficient to enable a confident assessment of the presence or absence of burials.

Some of the emails we received were equally informative, but in a different way than the situations described above. In an email dated November 15, 2005, Jack Elliott, a historical archeologist with the Mississippi Department of Archives and History, described a very revealing incident involving a failed witching attempt at the Confederate Veterans Cemetery in Biloxi, Mississippi. Elliott had earlier excavated two trenches at the cemetery and found several unmarked graves, mapped them, and then covered them over. He then brought in an “expert” local witcher and asked him to use his witching talents to relocate the graves in the trenched areas. The witcher witched the two areas and reported his findings, but according to Elliott, “his proximity to the real graves was no better than someone who was shooting in the dark, so to speak.” Elliott didn’t

want to embarrass the witcher by telling him that he was wrong, so instead he simply showed him the map and indicated the discrepancies. The witcher then said, "Let me try that again." Not surprisingly, he did quite well on his second try but provided no explanation as to why the results differed from his first effort.

Yet another example was provided in an email of November 21, 2005, submitted by Tom Barrett, Cultural Resources Director with Greenhorne and O'Mara, Inc., located in Greenbelt, Maryland. He stated that in his company's search for unmarked burials in Maryland, they tried "just about everything" to identify them, but "nothing's seemed to work to our satisfaction—especially dowsing!" They began their search by using ground-penetrating radar (GPR) and digging shovel test pits without success. Then they called on the services of a self-proclaimed dowser, but even though they tested all his spots, they found nothing. Despite that fact, the dowser said that he was going to come back to their office with a lawyer, to (presumably) claim that the archeologists had "missed" the graves even though he "knows they're there." I have yet to hear more about the outcome of that incident.

By far the best report I received was a synopsis of investigations undertaken in Iowa by various archeologists and described by Bill Whittaker, who works for the Office of the State Archaeologist at the University of Iowa. His article, entitled "Grave Dowsing Reconsidered," is available on the OSA web site (www.uiowa.edu/~osa/burials). Whittaker discussed the problems and perils of grave dowsing and the reasons why grave dowsing does not work, based partly on experiments he performed in various attempts to test his own dowsing abilities. More importantly, from my perspective, he described several examples of archeological testing of grave dowsing in Iowa, some of which resulted in written reports. In terms of actual groundtruthing, he listed eight locations that were investigated through subsurface testing. No evidence of burials were found at five of those locations, but at one of them, according to the project archeologist (Sellers and Ambrosino 2002), the dowser was so upset at the lack of graves that he accused the archeologists of stealing bones, even though he himself had witnessed the investigation!

At two other sites the dowsers said that no

graves were present. Subsequent excavations revealed several burials that the dowsers had walked over and failed to identify. And at one other site, the Johnson County Poor Farm cemetery, after the dowser had identified 94 graves, one was excavated and a probable grave shaft was found (there was no mention of a skeleton being found, though, so apparently they did not excavate deep enough to determine if a burial was present). This location, however, had an obvious grave-like surface depression, thus bringing into question the matter of whether the excavation of a grave-like depression within a known cemetery is a valid test of the efficacy of dowsing, a fact that was acknowledged by the dowser (Rogers 2004).

As noted in Whittaker's report, remote sensing was used to investigate witched graves at two other locations in Iowa, both of them considered to be Mormon cemeteries. The most recent of those investigations took place in 2002 at what was believed to be a Mormon Cemetery in Lee County. National Park Service archeologist Steve DeVore found some subsurface anomalies but determined that there was no apparent correlation between those anomalies and the locations that had been found by a local witcher. In addition, DeVore determined that the present geophysical data did not support the presence of graves in that location (DeVore 2003).

In 1999 the Garden Grove Mormon Cemetery (13DT133) in Decatur County was subjected to a remote sensing investigation carried out by Meg Watters, an archeogeophysicist from IMA Consulting of Minneapolis, Minnesota. The investigation involved conductivity, magnetometry, and ground penetrating radar, with the results being published later by DeVore (2005). Prior to that investigation, two local witchers (Paul and Karla Gunzenhauser) had located 39 graves within and four others outside the purported cemetery area (DeVore 1993:143–144). Several geophysical anomalies were found in the 1999 investigation, but unfortunately DeVore's report does not mention whether those anomalies matched the locations found earlier by the witchers. Watters, however, sent an email that greatly clarified the issue. While she was conducting her survey, some "Mormon historians" (the Gunzenhausers, according to DeVore in a personal communication of October 11, 2007) came to the site and used their

witching abilities to locate graves. After completing her investigation, Watters determined “with no doubt” that the location of the graves they identified (which included the sex, age, and orientation of the body, i.e., head to the north) “was not confirmed in any way by the geophysical techniques I was using.” Although it would have been even better if someone could have groundtruthed the graves, the results of the remote sensing strongly suggest that, once again, witching was a failure.

Despite all these negative results, it is important to note that I received two positive accounts pertaining to witching, although they did not involve graves. One of these stories was described in an email from an archeologist who said that a dowser identified an undisturbed refuse dump at a Chinese mining site, apparently somewhere in the Pacific Northwest. The other report was from a remote sensing expert who told of a witcher finding a buried feature somewhere in the northern Plains. Those are the only such reports—apparently verifiable—that I received of successful witching, but it is important to note that they pertain to archeological features, not graves, and it is uncertain whether the finds were a matter of luck rather than actual success.

It is also important to mention that on a few occasions I have talked to witchers who told me they actually found graves that were verified through subsequent excavation. Unfortunately, those excavations did not involve archeologists and were not documented, so I continue to have my doubts—especially since their finds were made in known cemetery locations, where anyone would have a high probability for finding graves. It should also be noted that the witchers made no mention of whether graves were found in *all* of the locations they marked. If they did make a few mistakes, that means their supposed success in witching was more a matter of happenstance than perfection. Naturally, I would really like to use more of those witchers in future field research, to test their reputed abilities. Unfortunately, though, I will not be able to do anything like that, because I am no longer involved in archeological endeavors. Even if I could do something, it wouldn't really be possible—usually because the gravesites are too far away, and it would cost too much to get the witchers out to the site.

With regard to my own research, I must admit that I wish it could have been more extensive; in

particular, I wish I could have groundtruthed many more of the witched graves. In most cases, however, my research was simply a hurried response to someone's request for assistance, and I did not have the time, money, or authority to check all of the claims. And often, because there was usually no requirement that I write a formal report, nothing was produced other than scribbled notes, a progress report, or the information that is in this article. I suspect that's true for many other such investigations, which may explain why there are so few formal reports that document the results of witching and groundtruthing.

In summary, I can say is that, in all my years of experience, I have never seen anyone find any verified graves through witching. In my opinion, if you're witching for graves, you're witching for failure!

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ANALYSIS OF A SURFACE COLLECTION FROM 14EK301, AN EARLY CERAMIC RESIDENTIAL SITE NEAR THE ELK RIVER IN SOUTHEAST KANSAS

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This article contains data obtained from an artifact collection housed at the Kansas State Historical Society. Analysis based on ceramic and lithic artifacts led to the conclusion that 14EK301 was a residential site from the Early Ceramic period.

The Early Ceramic village site 14EK301 is located [REDACTED] in southeast Kansas. The site is located on a terrace on the valley floor north of the Elk River, which joins the Verdigris River in the central part of Montgomery County. The river flows south and joins the Arkansas River in Oklahoma. This puts the site in the Arkansas River drainage into the Mississippi River.

Artifacts collected include potsherds, chipped stone tools, ground stone tools, bone, antler, and a historic cap lock hammer. The collection has remained unanalyzed since Kansas State Historical Society archeologist James O. Marshall performed the surface collection in 1967. At that time there was a rush to collect artifacts from sites that were in danger of damage from the nearby Elk River.

The author began this analysis toward the end of his time as an intern at the Kansas State Historical Society Archeology Lab. When looking at possible sites to analyze, 14EK301 was chosen because it had a variety of materials from different material classes and was a small enough assemblage to be analyzed within the scope of the internship.

ANALYSIS METHODS

Ceramic material was examined visually and under 10 × magnifications to determine temper, surface treatment, and decoration. Reaction to dilute

hydrochloric acid was used to test unidentifiable temper when it was thought that it could be shell or limestone.

Lithic artifacts were sorted by functional types. Projectile points were then grouped by morphological characteristics, measured, and weighed. Measurements on fragmentary specimens were recorded only when the measured attribute (e.g., length, width, thickness) was complete; therefore, the dimension ranges reflect only complete measurements for that attribute. When possible, projectile points were classified using names common to Plains archeology. When not assignable to a type, specimens were described.

Bifaces were classified into two groups, based on roughness. One group contains all bifaces that have lenticular cross sections, centered and aligned edges, and a width-to-thickness ratio greater than 4.00. The other group consists of all other bifaces. This classification follows that used by Reynolds (1987). Material and heat treatment for chipped stone tools were determined visually.

The paper catalog was referenced when identifying artifacts but was not taken as necessarily correct. Comparative materials from the Kansas State Historical Society Archeology Lab aided with identification of ceramic temper, lithic material, and faunal taxonomy. Weights were taken using a digital scale, and measurements were taken using digital calipers. Findings were recorded in Excel spreadsheets.

CERAMIC ARTIFACTS

A total of 260 potsherds was analyzed. The ceramic temper represented is extremely varied. The majority is tempered with crushed clay (N = 165; 1,543.3 g). Other tempers are grit (N = 29; 209.1 g), sand (N = 5; 48.9 g), shell (N = 41; 178.5 g), limestone (N = 4; 34.7 g), and bone (N = 14; 101.1 g). Two sherds, weighing a total of 9.8 g, have significant hematite inclusions. As hematite was not regularly found throughout the collection, it was presumed to be temper.

Some potsherds are cordmarked, but most have smooth surface treatment (N = 248; 1,985.9 g). Some sherds identified as plain may have been cordmarked but were smoothed extensively. The cordmarked sherds (N = 12; 139.5 g) are mostly clay tempered, with one each being tempered with grit and limestone.

Decoration is found only on sherds with plain surface treatment (N = 48), and many have more than one decorative element (Figure 1). Decorative elements are dentate stamps (lines of depressions made by pressing a tool with teeth into the surface of the vessel; N = 27, 56 percent), punctates (small depressions caused by pressing a simple tool into the surface of the vessel; N = 23, 48 percent), trailed lines (lines made with a blunt tool; N = 21, 44 percent), incised lines (lines made with a sharp tool;

N = 7, 15 percent), bosses (raised circles caused by pressing a simple tool from inside the vessel; N = 4, 8 percent), and cord-wrapped stick impressions (N = 4, 8 percent). Punctates are sometimes part of a zoned pattern (Figure 2), as are dentate stamps, where the zones often take the form of narrow bands (Figures 2 and 3). Rims sometimes are decorated with crosshatched incised lines. While rim sherds often bear decoration, many body sherds of uncertain location also are decorated. Lip decoration is limited to impressed tool marks on one sand-tempered sherd.

Note that this pottery is consistent with Cuesta phase material found in this region, as discussed by James O. Marshall (1972) and Thomas A. Witty, Jr. (1999). Located in neighboring Montgomery County is the type site for the Cuesta phase: the Infinity site (14MY305). Wares associated with Cuesta phase Component B of 14MY305 include Havana ware and Cuesta ware. Havana ware is described as sand tempered, smooth surfaced, and decorated with a variety of elements, including dentate stamps (sometimes zoned), trailed lines, bosses, and plain crescent stamps. Cuesta ware characterized as mostly clay tempered, smooth surfaced, with decorative elements, including dentate stamps (sometimes zoned), trailed lines, bosses, punctuates, cord-wrapped stick impressions, and smooth stick impressions (Marshall 1972:44–55).



Figure 1. Sherd exhibiting dentate stamped rim, punctates, and a boss.



Figure 2. Sherd exhibiting zoned dentate stamping and zoned punctuates.



Figure 3. Sherd exhibiting zoned dentate stamping.

Two radiocarbon samples, both 10-gram samples of charcoal collected from a midden area of Component B, yielded dates of AD 780 $\pm$ 80 and AD 970 $\pm$ 80 (Marshall 1972:92–93).

CHIPPED STONE ARTIFACTS

Many chipped stone artifacts are present, including projectile points, scrapers, drills, and bifaces. Projectile points were divided into morphological groups A–I. Their length, width, and thickness measurements are compiled in Table 1.

Group A consists of 43 projectile points (4 complete), similar to those that Noel D. Justice (1987:190–191) identifies as Dickson Contracting Stem. This style has a contracting stem with a straight base (Figure 4) and a blade that ranges from straight to slightly excurvate. The widest point occurs at the shoulders. Nineteen fragments classified as Dickson Contracting Stemmed were too incomplete for measurement. Justice (1987:191) stated that this Early Woodland point type probably dates from about 500 to 100 BC. While these contracting stemmed points are Early Woodland in the

Mississippi trench, in eastern Kansas they are more strongly affiliated with the Late Archaic (Robert J. Hoard, personal communication, 2009).

The 36 projectile points (7 complete) in Group B resemble those that Justice (1987:189–190) classifies as Gary Contracting Stemmed, except that they have rounded convex rather than pointed bases (Figure 5). Seven fragments were identifiable but too incomplete to measure. Justice (1987:189) placed Gary Contracting Stemmed points in the latter part of the Late Archaic period into the Middle Woodland period, about 1500 BC to AD 100. To clarify a messy taxonomic problem, the Middle Woodland period referred to by archeologists in the Midwest is roughly contemporaneous with the poorly defined Early Ceramic period used by many Central Plains archeologists. For further discussion of the temporal taxonomy, see the introduction to *Kansas Archaeology* (Hoard and Banks 2006: 7–8).

Five incomplete projectile points, comprising Group C, are ovate in shape with corner notches (Figure 6), similar to those that Justice (1987:201–204) identifies as Snyders. The points have convex

Table 1. 14EK301 Projectile Point Measurements.

Projectile Point Group	Maximum Length		Maximum Width		Maximum Thickness			
	Range		Range		Range			
	in mm		in mm		in mm			
	Total Number	Number Complete	Complete All Specimens	Complete Specimens	Complete All Specimens	Complete Specimens	All Specimens	Complete Specimens
A – Dickson Contracting Stemmed	43	4		39.27–53.16	18.24–41.78	22.75–27.5	5.04–9.05	6.36–9.04
B – Gary Contracting Stemmed	36	7		41.30–67.66	19.09–43.68	19.47–28.25	5.52–14.19	7.02–14.19
C – Snyders		5			42.52–42.80		8.17–10.28	
D	17	1		60.91	23.51–38.83	38.83	6.43–10.88	8.11
E		3			37.33–38.18		8.49–9.96	
F	7	1	44.19–45.41	44.19	21.39–28.91	28.91	5.30–8.17	5.92
G	4	2		48.74–51.04	24.06–27.76	24.50–27.76	7.65–11.83	7.65–7.75
H	2	1		64.00	33.43–34.22	34.22	9.65–11.01	11.01
I – arrowpoints	2				12.41–14.32		2.51–2.90	



Figure 4. Dickson Contracting Stemmed points.

bases and excurvate blade edges. The maximum width generally occurs above the shoulder. Justice states that Snyders points are diagnostic of the early Middle Woodland period, 200 BC–AD 400.

The remaining 119 projectile points were

grouped according to distinguishing traits but could not be correlated with established types. Group D has 17 projectile points (1 complete) with corner-notched hafting elements and convex bases. The blade edges range from straight to excurvate with



Figure 5. Gary Contracting Stemmed points.

the widest points at the shoulders. One fragment from this group was too incomplete to measure.

Group E has three incomplete projectile points with corner-notched hafting elements, convex bases, and excurvate blade edges. Distinguishing them from the previous group, the widest point occurs above the shoulder. Although they share many characteristics of Snyders points, they lack broad blades (Justice 1987:201). They may be reworked Snyders points.

Another group of seven projectile points (one complete) comprise Group F. These points have expanding stem hafting elements and straight bases. The blade edges are straight to slightly excurvate. Maximum width occurred at the shoulders. Other points with similar attributes were omitted from the group. One has tapered shoulders, while the Group F projectile points have sharp shoulders. A second was excluded because it is proportionately longer (length-width ratio = 2.44) than the complete point (length-width ratio = 1.53), which seems comparable to the other points in the group. A third was excluded because it is much larger, being 67.17 mm long and 42.34 mm wide.

Group G has four projectile points (two

complete). These points have expanding stem hafting elements and convex bases. The blade edges are excurvate, and the maximum width occurs at the shoulders. A projectile point was excluded from this group because it was proportionately wider (length-width ratio = 1.32) than the complete points (length-width ratio = 1.84–1.99).

The next group has two projectile points (one complete). These points have corner-notched hafting elements and straight bases. The blade edges are excurvate, and the maximum width occurs above the shoulders.

Two incomplete projectile points are much smaller and thinner than others found at the site; they appear to be arrow points. One has a corner-notched hafting element with a convex base and straight blade edges. It has a maximum width of 14.32 mm and a maximum thickness of 2.90 mm. The other has a side-notched hafting element with a convex base and straight blade edges. It has a maximum width of 12.41 mm and a maximum thickness of 2.51 mm.

Some projectile points do not fit in any of these classes. Many are fragments that lack important elements. Of these, 9 have contracting stems, 7 have



Figure 6. Snyders points.



Figure 7. Points reworked into drills.

expanding stems, and 69 are unidentifiable fragments. Others have attributes that are unique within the collection. Three have concave bases. Of these, one is corner notched and the other two have expanding stems. The proportions on the two expanding stem points are so different (length-width ratio = 1.21 and 2.97) that they were not put together to form a new group. Another has a concave blade edge unlike anything else in the collection. This point may have been reworked.

Drills from the site come in a variety of forms. While 13 are stem fragments, 23 have at least part of an expanding base intact. Of these, 13 have complete bases. Three are reworked projectile points: one from a Gary Contracting Stemmed point, a second from an expanding stem point with a convex base, and a third from a corner-notched point with a convex base (Figure 7). On three of the others, the basal form is concave, and seven have a convex basal form.

Three hundred-twenty other bifaces were recovered from the site. The bifaces vary in shape, size, and roughness. Approximately 12 percent of the bifaces have lenticular cross-sections, centered

and aligned edges, and a width-to-thickness ratios greater than 4.00. Among the bifaces is a siltstone chopper.

The assemblage contains an assortment of scrapers. Classic teardrop-shaped scrapers are fairly common, as are circular scrapers. Scrapers classified as circular range from 1.05 to 1.24 in ratio of maximum axis length to minimum axis length. Brad Logan (2006) notes that circular scrapers are more common in Middle Woodland sites. Many side scrapers from the collection are somewhat crescent-shaped. Unifacially worked flakes are present in all forms of scrapers.

The debitage consists of flakes (N = 19, 76 percent) and shatter (N = 6, 24 percent). The flakes are 32 percent (N = 6) secondary flakes and 68 percent (N = 13) tertiary. Fifty three percent are heat-treated. Two chert cores were recovered from the site, plus one piece of limonite that appears to be a tested core with only a few flakes removed.

Lithic raw materials identified in the chipped stone assemblage are Florence A and Foraker cherts, with a single projectile point made of basalt. Both Florence and Foraker outcrop in Elk County

(Kansas Geological Survey 2002). Florence is a Permian-age chert for which four varieties have been identified. "Florence A, also known as Kay County chert or Maple City chert, is a buff to yellow-gray chert found in the southern Flint Hills. It contains large fusulinid fossils, is fine-grained, and often has bands that resemble fingerprints" (Stein 2006:171). Foraker, also in a Permian formation, has an abundance of fusulinid fossils. Approximately 65 percent of the Florence A chipped stone tools are heat-treated. Foraker chert is roughly 35 percent heat-treated. Overall, approximately 30 percent of the chert is heat-treated.

GROUND STONE ARTIFACTS

A small number of ground stone tools were collected from the site. This includes four manos, two hammerstones, a sandstone abrader, and the working edge of a celt. The manos have battering on the edges, as well as wear on the flat sides. One hammerstone is smooth except for the wear at the ends, while the other has wear on most of its surface. The abrader has a lateral groove, which deepens toward the middle.

FAUNAL REMAINS

Animals identified from the site include deer (genus *Odocoileus*) and coyote (*Canis latrans*). Deer are represented by teeth and mandible sections, two calcanei, antler sections, and the distal end of a tibia. Because both calcanei are from the right side, at least two individuals were present. The tibia fragment has striations on its medial surface. A coyote is represented solely by a canine tooth. A mussel shell also was recovered from the site. Because this was a surface collection, it is unknown whether these remains are archeological in nature.

HISTORIC ARTIFACTS

Also recovered from the site was a percussion hammer from a firearm. This kind of percussion hammer was patented in 1807 by Rev. Alexander John Forsyth (not a United States patent, as these were not granted until 1836). It replaced the flintlock firing system in American military use in 1842, though it

was used outside the military before then (Shields 1954:59–62).

CONCLUSIONS

Even though no features were recorded during the survey, judging from the amount of ceramic material recovered and the variety of stone tools, this site was probably a village. The pottery decoration is akin to Havana ware, Cuesta ware, and other Early Ceramic styles. This and projectile point morphology suggest an Early Ceramic cultural affiliation. Surface collections can be difficult to analyze because there is no stratigraphic evidence to establish artifact associations. Although some of these artifacts may come from different time periods (for example, the historic percussion hammer), the corroborating temporal evidence from different artifact types suggests that most of the collection may be part of a single component and supports the use of a surface collection in this analysis.

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THINKING ABOUT HUMAN REMAINS AT THE ELLIOTT SITE

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O'Brien's commentary on Roper's earlier discussion of human remains from the Elliott site misunderstands how Roper constructed her argument. This response first discusses the circumstances under which the article was written and some principles involved in inferring behavior when only the outcomes of that behavior can be observed. It then considers the Elliott site case and, using examples, discusses how various disturbance processes affect archeological sites and may have affected Elliott. How the interpretation of the human remains and funerary objects in the Elliott site collections was constructed is then reiterated, with an emphasis on how and why the techniques used in constructing that interpretation were chosen.

Patricia O'Brien's recent article re-examining the Schultz phase includes an appendix that discusses the human remains from the Elliott site (O'Brien 2007). That appendix is a response to this author's earlier article (Roper 2000) that described and discussed those remains and their significance within the context of Early Ceramic period mortuary practices. The interpretation Roper offered differed from that placed on the remains when they were originally reported (O'Brien et al. 1973; Sorrell 1974), and O'Brien's response apparently was intended as a critique of Roper's article. O'Brien's critique, however, appears to misunderstand the context in which Roper's paper was written and definitely misunderstands and misrepresents how Roper's argument was constructed. If, as its concluding paragraph (O'Brien 2007:76) implies, the reader is simply supposed to choose between competing interpretations, then he or she should base the choice on not just what the chosen interpretation is, but on how it was constructed. A valid interpretation is not, or at least should not be, simply an opinion formed from an intuitive notion about what something should be, but rather a rigorous argument constructed using well-justified principles about how the archeological record is formed.

In the decades since the Elliott site excavations, archeology has been actively building theory concerning how human behavior articulates with material culture (Skibo and Schiffer 2008 is a recent

statement) and how post-depositional processes distort the archeological record, leaving the reading of that record not always a straightforward process. One of the results of these advances in theory building and the development of methods for connecting theory to phenomena (a.k.a., the archeological record) is that old interpretations must be revisited periodically and their continuing validity evaluated in light of greater understanding of these principles. This is what was happening here. The purpose in presenting this response to O'Brien's recent article, therefore, is less to reiterate Roper's interpretation of the Elliott site human remains than to discuss some of the principles used in re-reading the Elliott site case more than a quarter-century since the original reports were published and to comment on the methods used in constructing the argument.

It is highly relevant to first clarify the context in which Roper's paper was written. An endnote states that both the human remains and the funerary objects found at the Elliott site are listed on Kansas State University's (KSU) NAGPRA inventory. That statement is correct but probably does not make it sufficiently clear that the article was written in conjunction with preparing that inventory. NAGPRA—the Native American Graves Protection and Repatriation Act (PL101–601)—contains a number of provisions, one of which is the requirement that institutions inventory the American Indian human remains and funerary objects in their

possession or control and submit those inventories to the National NAGPRA Program at the National Park Service. Compliance is not voluntary or optional for institutions; institutions failing to comply can be assessed civil penalties, and those can be substantial. Nor do the institutions decide what of their holdings to include or how to present the inventory. Instead, the regulations implementing NAGPRA (43 CFR 10) are quite specific about what is to be included on the inventory, and the National NAGPRA Program requires a specific reporting format.

Preparation of KSU's NAGPRA inventory began with a search of all archeological catalogs and collections for human remains and funerary objects. This search readily indicated that human remains had been recovered from the Elliott site, an extensive and repeatedly used habitation site in the McDowell Creek valley in Geary County. Site function is irrelevant in NAGPRA context—human remains are human remains no matter what type of site they come from, and all of the Elliott site human remains had to be entered on the inventory. Also, a good faith effort had to be made to identify any possible funerary objects associated with those remains. Several items from the site's collections were identified as possible funerary objects and evaluated to determine their status. NAGPRA compliance requires the institution to produce an item-by-item inventory of human remains and funerary objects—a “simple itemized list” (25 U.S.C. 3003(e)) in the language of the law. A text that more fully describes how determinations were made may accompany the list. In fact, in *Na Iwi O Na Kupuna O Mokapu v. Dalton*, the court held that the simple itemized list defined in the law was intended as a minimal standard and that “Congress intended not only to permit but to favor inventories containing more than a simple list” (894 *Federal Supplement*, D. Hawai'i 1995:1414–1415; see also Hutt et al. 1999:337). KSU's inventory meets the higher standard, and it was in the course of preparing this more detailed account that Roper also prepared the article published in 2000. At the time it seemed, and it still seems, that the evidence from the Elliott site adds information regarding mortuary practices in the poorly known temporal interval that this site represents. Thus, the information was both incorporated into the university's NAGPRA inventory

and made more widely available by turning it into a short journal article.

The presence of human remains, including both teeth and skull fragments, in the Elliott site collections is non-controversial. Several individuals with expertise in human osteology examined the remains in the course of preparing the NAGPRA inventory, and the remains are listed as they are on the basis of those examinations. The issue is how these remains came to be a part of the archeological record at this site, i.e., whether they represent an intentional burial or other mode of burial treatment, which O'Brien maintains is a cremation.

Momentarily setting aside the specifics, determining how the remains got there goes to the heart of how archeologists make inferences about what happened to produce the archeological record. Archeology by definition is the study of past human behavior from the material record. Because it is the past that archeologists are investigating, they are looking only at the *outcome* of a behavior or an action and have no hope of directly observing the behavior itself. Thus, the behavior that produced the outcome usually is not self-evident, as most outcomes can be obtained in one or another of several ways. This is the problem of equifinality—the equivalence of outcomes. It is by no means limited to interpretation of the archeological record, for uncertainty about what actions led to a particular outcome is extremely common in all fields of inquiry, as well as in everyday life. It is the reason, or at least part of the reason, that the mystery works as a form of literature; it is the reason that multiple medical tests may be required to determine what is producing a patient's symptoms (if there were only one way a symptom could occur, there would be no need for any tests because the cause would be evident); it is the reason that the cause of airplane crashes may take so long to assess (and sometimes can not be fully resolved); and the list could go on and on.

A major problem in some of the archeological literature, as well as in everyday discourse, is how often people reason in the wrong direction. In the archeological case, they forget that although the main interest really is the past behavior, archeologists are observing only the outcome of that behavior. Thus, they know that a specific action *could* have produced a specific outcome and may even

have occurred that way in certain other instances they know about. So, without considering the possibility that other actions could produce the same outcome, they assume that the behavior or action previously observed to produce a particular outcome must also have produced the outcome in the instance they are seeking to understand. In terms of logic, this is the Fallacy of Affirming the Consequent: in essence the fallacy that because something *could* have occurred in a particular way, it *must* have occurred that way, when in fact there are other ways it could have occurred (see, for example, McInerney 2005:105–106 or any one of many other books about good thinking).

To take the case at hand, then, human remains in an archeological site can result from deliberate primary burial of the corpse of a recently deceased individual; from secondary interment of the remains of an individual whose corpse was scaffolded or defleshed after death; from cremation, which may have been either of a recently deceased corpse or of secondary remains; from exposure of a corpse that was not retrieved after a raid; from exposure because an individual died unattended and was not retrieved; or one of several other processes, some perhaps somewhat esoteric, some not (Sprague 2005, while nominally about burial terminology, also is a good exposition of possible burial modes). Archeologists look at the outcome of the mortuary act and seek to determine how the human remains got where they were found. Sometimes it is very clear, or at least the options are clearly limited. Sometimes, as in the Elliott site case, it is not clear. What must be done, though, is to make the determination empirically and not on the basis of what has been observed before for the particular culture represented. Although this must be considered as a possibility, other possibilities must not automatically be rejected just because they have not been observed before in the given context. In fact, an automatic rejection of other possibilities means that no discovery can be made, only affirmation of what is already known. And when dealing with past human behavior for a poorly known period, what is already known may be only a portion of the entire range of behavior associated with that period. Rather than use the limited knowledge of a phenomenon to provide narrow off-the-rack interpretations of newly observed instances, a better approach

is to build scientific arguments. The term argument in this sense refers not to a quarrel but rather, in the recent words of Lewis Binford (2007:236, *italics added*), to “a *justified* constructed statement as to how the world of interest works.” Much of the theory-building work of archeology in the last few decades has been directed toward providing principles that can be used to justify constructed statements about past behavior.

The set of Elliott site human remains in the KSU collection include some deciduous teeth. As O’Brien correctly noted, children routinely lose deciduous teeth. Thus, they *can* be incorporated into a site’s cultural deposits that way and not represent an inhumation in any form. However, it also is true that deciduous teeth *can* enter the archeological record when a child dies with those teeth still in its mouth. Thus, neither can be assumed at the outset, and it is necessary to look further to determine which is the case. (And either way, human remains in a university collection must be listed on a NAGPRA inventory.) In this case other cranial fragments from the facial region of a sub-adult are present. Still, the implication that the body of a deceased child was present does not specify the manner of corpse disposition, and a circumstantial case needs to be developed using additional lines of evidence. One potential line of evidence is the spatial coherence of the remains of both the sub-adult and the adult also represented here. Thus, Roper plotted the distribution of the Elliott site remains, including all teeth and skull fragments, within the grid used for the excavation. This plot (Roper 2000:50) showed that the human remains occurred in a reasonably restricted part of the excavated area. O’Brien, however, suggested that there was no grave pit here (or elsewhere in the excavated area) and, therefore, that the remains do not represent deliberate burials. Negative evidence, though, is subject to equifinality and is rarely definitive; to use the succinct phrase, absence of evidence is not necessarily evidence of absence. Instead, it is important to determine if no grave pit was observed because there was none or because post-depositional formation processes had obliterated a grave.

Here the archeologist turns to what has been learned about how natural processes distort the archeological record and how to work through that. This is done in several ways. One way, a weak one

but certainly not without merit, is to find analogs and precedents. In this case Elliott certainly would not be the only burial ever excavated where no grave outline was apparent. A case that comes to mind from personal experience is the Airport site (11Sg^v280) in Sangamon County, Illinois, excavated in 1974. The Airport site was a small Archaic period cemetery that contained at least 13 burials, for which it was reported that:

Fragments of human bone were profuse on the surface and/or in the plowzone . . . [upon excavation] grave or pit features were not observed . . . such features, if present, should have been discernable in the light yellow subsoil (Roper 1978:19).

The Airport site was in a cultivated field. Years of repeated plowing had obliterated any grave outlines and reduced the skeletons to fragments. Many bone fragments were recovered from the plowzone, and most skeletons were so severely reduced by plowing and discing that only portions of skulls and some teeth remained in place at the base of the plowzone. Were this site to be excavated now, 35 years later, one has to wonder if anything would remain in place. Although it has several analogs with the Elliott site, no one has ever disputed that the Airport site burials were primary burials.

Like the Airport site, the Elliott site has been repeatedly plowed and disced. When J. V. Brower reported it in 1896, he noted then that it had been plowed for about a half century. That was more than a century ago. Here increased understanding of the principles of landscape alteration by natural processes and how these processes affect the archeological record enters the process of constructing arguments about past behavior. Plowing does not simply turn the upper part of a soil and leave it unaffected otherwise. Instead, it exposes the surface and leaves it vulnerable to erosion. And erode it does. O'Brien seems skeptical when she comments that maybe a foot of sediment could be removed, but there are many situations where one might wish that *only* a foot of sediment had been removed.

A case study from a part of the Central Plains illustrates this. Writing specifically about the Medicine Creek valley of southwest Nebraska, J. C. Brice (1966) showed that a layer of wind-blown

sediment, averaging about a half-meter thick, once blanketed the area. As had been observed by several archeologists (Blackman 1930:357; Wedel 1933:149) before that time, Central Plains tradition sites are associated with the A horizon of a soil buried by this aeolian sediment unit. The upper post-Central Plains tradition sediment unit is widespread in Nebraska and Kansas. Yet Central Plains tradition remains often are exposed on a modern surface and are increasingly known to have been entirely plowed away (e.g., Latham 2006), implying that both the upper sediment unit and also the upper portions of the sediment unit it buried, were removed. This author knows of a location in the Medicine Creek valley (a portion of site 25FT39) where a mid-twentieth century house stood adjacent to the location of an Upper Republican phase lodge. The lodge location is exposed on the surface and has been plowed repeatedly; the modern house location is not plowed, and the post-Upper Republican sediment unit is intact. Excavations in the unplowed portion of the site recovered well-preserved Upper Republican features beneath about a meter of overburden. The modern surface dips gently from the modern house area to the lodge area. A continuous profile across this area showed a gradual thinning of the upper sediment unit, eventual surface exposure of the A horizon containing the Upper Republican lodge, a gradual thinning of the A horizon, and finally exposure of only the C horizon of this soil. At this point the modern surface is about a meter lower than the surface of the unplowed portion of the site, and the artifact scatter ends. The Upper Republican phase lodge now is represented only by the artifact scatter associated with the A horizon, and no floor or features remain.

An instructive example of how the 25FT39 house, and undoubtedly many other sites, was reduced came about in early June 1998. The investigation of the eroding lodge was preceded by a gridded intensive surface collection. A windstorm came up as the collection was being completed and blew hard for a few days. When the site was re-inspected about three days after the surface collection was completed, many hundreds if not thousands of additional objects were exposed as the wind winnowed sediments and left the objects, which were too heavy to be laterally displaced by the wind, lying on the slightly lowered surface. Although this

was a particularly intense episode of blowing, almost any archeologist with much field experience has seen soil picked up and moved by wind. While this may be slight in any given instance, the sediments are gradually deflated by repeated episodes of blowing, and the cumulative effect over the decades can be very large, as the 25FT39 instance attests. Accounts of soil movement during the period of the Dust Bowl are truly frightening, leaving one to wonder how an archeological record in good context remains at all in some places. Natural processes have destroyed many features, including lodges, pits, post molds, and graves.

O'Brien goes on to discuss the funerary objects. As she introduces that theme in her article, she says that Roper supported her argument for the burial interpretation of the human remains by arguing that the bone beads are funerary objects. That is an absolutely incorrect reading of Roper's analysis. An objective reading of Roper's article will show that the interpretation of the human remains was presented in its entirety *prior to* discussing the funerary objects and that it was made entirely on its own merits. Similarly, the funerary objects were interpreted as such on their own merits and initially without reference to the human remains. The spatial analysis was performed to assess their association with the human remains, for *which it had already been concluded that represented an inhumation*.

Funerary objects is the term used in NAGPRA and is not idiosyncratic, as is implied by the snide and annoying use of quotation marks around the term. In NAGPRA, it is defined as "objects, that as part of the death rite or ceremony of a culture, are reasonably believed to have been placed with human remains either at the time of death or later" (25 U.S.C. 3001 (3) (A)). In the law, funerary objects can be either associated or unassociated. Unassociated funerary objects are those that are not with human remains but are reasonably believed to have been placed with such remains; associated funerary objects are associated with human remains. In the course of inventorying the Elliott site collections, it was realized that the collection contains a number of the bone beads. Absent the human remains, it is unlikely that they would have been interpreted as part of the habitation site debris, as O'Brien suggests they would have been. As O'Brien's own reports, among others (especially Eyman 1966),

show, similar bone beads are common in Schultz phase mortuary context but do not appear in habitation context at either 14GE41 or 14RY38, the only excavated Schultz phase habitation sites other than the Elliott site component. Even absent the human remains, it is highly likely that the bone beads would have been treated as potentially unassociated funerary objects. However, it is something of a moot point as both beads and human remains *were* recovered at Elliott.

The negative evidence from the other excavated habitation sites is in itself not sufficient evidence to interpret the Elliott site beads as funerary objects, nor were these objects interpreted as funerary objects in that manner. Coupled, though, with their almost exclusive occurrence in mortuary context, a suspicion that needed further examination certainly was raised. To determine if there were, in the language of the law, a reasonable belief that these objects had been placed with the human remains, the beads were added to the distribution plot of the human remains within the excavation grid (Roper 2000:50). If the beads were placed with the human remains at the time of interment, then they should be spatially associated with those remains. The principle here was that, while the field has been repeatedly plowed, plowing overall does not displace objects too far from where they were originally placed. In fact, the distribution of funerary objects from the aforementioned Airport site provided one of the first published case studies of the effects of tillage on lateral displacement of objects (Roper 1976). That and subsequent empirical and experimental studies (e.g., M. J. O'Brien and Lewarch 1981) have abundantly shown that, while some individual objects may be widely displaced from where they were deposited, most are not, and the overall spatial pattern of the objects reflects the spatial pattern of their original placement. Wind deflation displaces objects vertically by dropping them on an increasingly lowering surface, as in the 25FT39 example, but does not laterally displace them. How closely the beads may have been associated with the human remains at Elliott was a pertinent question to ask, and spatial analysis was a reasonable way to evaluate the association with the human remains. The spatial plot showed that the beads were recovered from the same restricted part of the excavated area as were the human remains

and constituted good circumstantial evidence of the association between the two classes of objects.

The analysis went on to discuss the Scallorn points that also were recovered from the site. Scallorn points, of course, appear in habitation context in known sites too numerous to enumerate. In the absence of human remains, they usually would not be believed to be unassociated funerary objects or, in other words, they are not reasonably believed to have been placed with human remains. Scallorn points, however, also appear in some quantity in Schultz phase mortuary context (Eyman 1966). Thus, their presence in a component with both habitation debris and human remains could render their association subject to interpretation. While it intuitively did seem more likely that they were habitation debris at the Elliott site, a small amount of additional work provided some supporting evidence for that. The distribution of these objects also was plotted to examine their spatial coherence with the human remains. The plot showed that the Scallorn points recovered from the Elliott site were *not* particularly closely associated with the human remains, nor were they associated with the beads. By whatever process the materials at the Elliott site were deposited, it appears, as expected, that different processes were involved in the deposition of the Scallorn points and the beads, while similar actions were involved in the deposition of the human remains and the beads. There is good reason to suspect that they were deposited by a single episode of mortuary behavior.

These results of the spatial analysis were important, for cultural affiliation can be difficult to assess without culturally diagnostic objects. The Scallorn points, which, if not diagnostic of the Schultz phase, are at least attributable to it, are not demonstrably associated with human remains at the Elliott site. Thus, attributing the human remains to the Schultz phase on the basis of the habitation site debris is tenuous. Burials that are earlier than the living occupation represented at a site can occur—the Airport site was an example—while later burials intrusive through earlier deposits also are known. The Utlaut site in Missouri that O’Brien excavated in 1970 (O’Brien and Hart 1971) is an example of the latter, for here were several eighteenth-century Otoe-Missouria burials with closely associated funerary objects intruded through a scatter of

Woodland-period artifacts. Thus, only the demonstrable association with the bone beads allows the Elliott site human remains to be attributed to the Schultz phase and enables any discussion of the place of this component in Early Ceramic period mortuary practices in central and northeast Kansas.

Any interpretation of the Elliott site materials would be enhanced by a more detailed analysis of the nature of this component. It is a habitation component, but there are habitation sites and there are habitation sites. A fairly large and dense concentration of material, such as this component at the Elliott site, does not necessarily reflect a long and/or continuous occupation by a large number of people—it may represent the accumulated remains from repeated short occupations by small groups of the same cultural tradition (equifinality again). The separate occupations perhaps can be detected, but not by examination of the artifacts by themselves and certainly not by stratigraphy, which has been compromised in any event. Instead, spatial analysis is again a viable approach to building an argument. An example that comes to mind, again from experience, is site 13LC17 in the White Breast Creek valley in central Iowa, which was partially excavated in 1991 for a highway project. Here, about a half-dozen rock hearth features were recorded, and the artifact scatter was continuous throughout the excavation block, which comprised over 100 m². The question was whether the features and debris were left during a single large occupation by or repeated small occupations. Analysis of features suggested that one of the hearths was reasonably intact (this site was not and apparently never had been plowed; in fact, it was in the woods) but that the others probably had been dismantled at some time in the past, possibly to reuse the rock. A distribution analysis of the pottery and debitage, each divided into size classes, led to the conclusion that small groups periodically reused this site (Roper 1994). A similar analysis would be possible for the Elliott site collection (although the collection is large, and this would not be a minor undertaking). It may or may not be as conclusive as the 13LC17 analysis, but it might help address the question of the nature of the occupation and the behavioral context of those who left human remains at the site.

In sum, none of the objections raised to the identification of the Elliott site human remains and

funerary objects as the result of primary burial are necessarily valid. The human remains represented individuals who were deceased, the individuals shared a cultural affiliation with those who left the habitation site remains, the distribution of the human remains is spatially coherent, and objects that are found exclusively in mortuary context elsewhere are spatially associated with the human remains at the Elliott site. O'Brien invokes parsimony in arguing that the Elliott site human remains represent a cremation, but a parsimonious explanation is one that requires the fewest assumptions. It is difficult to understand how the complex set of actions necessary to skeletalize the dead or to conduct a cremation, which also consumes a large amount of fuel, is a more parsimonious explanation than the possibility that the deceased and a few funerary objects simply were placed in an expedient grave. That a grave could very well have been severely compromised by processes that have extensive precedent throughout the North American heartland has nothing to do with the behavior enacted more than a millennium ago but, of course, must be taken into account when the remains are interpreted.

Finally, the proposal that the Elliott site human remains represent an incidental burial does nothing to diminish the importance of mound interment of skeletalized and cremated remains to the people of the Early Ceramic period in the northern Flint Hills. That is abundantly attested and apparently was the predominant mode of burial at that time. The proposal of an incidental burial simply suggests that those people sometimes also made expedient decisions as circumstances warranted. Nor does the proposal that expedient burial was made during the Early Ceramic period imply that this is the only period during which such burials were made. Certainly it was not. Roper's article cites nineteenth-century members of a Pawnee hunting party effecting such a burial and undoubtedly many other groups also did this on occasion. Nor were American Indians the only people in North America to practice expedient burial. Accounts of overland pioneers and military expeditions make that very clear, and the periodic discovery of these isolated burials in unexpected places is further corroboration. Cultural ideals are just that: ideals. Actual practice sometimes takes unexpected forms.

That is one thing that defines humans and makes archeology so interesting.

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A RETURN TO GLEN ELDER: CULTURAL AFFILIATIONS OF ARCHEOLOGICAL MATERIAL FROM THE GLEN ELDER SITE (14ML1)

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Topeka, Kansas

The 1963 excavation of the Glen Elder site (14ML1) took place as part of the River Basin Survey project to salvage archeological sites that were slated for inundation by the Glen Elder Reservoir. The Glen Elder site was facing obliteration by the construction of the Glen Elder Dam. The results of the site excavation have never been published, an omission that this revised study finally corrects. Affiliations of the archeological material give evidence that Glen Elder is a Protohistoric-early Historic period occupation that, in part, is antecedent to the historic Kansa Indians.

The thesis of the report on the 1963 excavation of the Glen Elder site (14ML1) was to reconsider the existing classificatory designation of the Glen Elder material and related material using archeological evidence. The archeological evidence also was applied to describe the culture that is represented, to define the geographic distribution and temporal position of the archeological material, and to demonstrate affiliations of the material (Marshall 1969:3). The significant contribution of the study was demonstrating an affiliation of the Glen Elder archeological material with the Oneota archeological culture. The Glen Elder archeological material was presented as being a focus of a "western-like" Oneota manifestation (Marshall 1969:78-90). The description of the archeological material, its geographical distribution, and its temporal position were similar to the first formal definition of the culture that classified Glen Elder as a component of the Glen Elder focus, White Rock aspect (Rusco 1960:72-75).

An earlier designation stated that the Glen Elder site was a component of the White Rock Creek focus, Smoky Hill-Great Bend aspect (Wedel 1935:252). This classificatory assignment reflected a long-held view that resisted an Oneota affiliation. Waldo R. Wedel, years later, having had access to Rusco's manuscript, viewed the Glen Elder site as

a component of the Glen Elder focus, White Rock aspect, Upper Mississippi phase (Wedel 1959:534-536, 612-615). "There may be an Oneota relationship and such a connection is strongly indicated," Wedel concluded, "but present evidence did not warrant an inclusion in the Oneota aspect" (Wedel 1959:612-615). The evidence to which Wedel referred was that the pottery recovered from the Glen Elder site and related components, designated by Rusco (1960:30) as Walnut Decorated Lip, was formed with a paste tempered with grit particles. Simple stamping also was evident on the surfaces of a number of pottery sherds. Oneota pottery types were thought to be entirely shell tempered with no evidence of simple stamping. Several non-pottery traits were inadequately represented, too. The White Rock aspect material differed more from an Oneota manifestation than did the material that had been excavated from the Fanning site (14DP1) in Kansas and the Leary site (25RH1) in Nebraska (Wedel 1959:131-174). Wedel classified the Fanning site as a component of the Wolf Creek focus, Oneota aspect, Upper Mississippi phase (Wedel 1959:534-536, 612-615).

The W. C. McKern-devised Midwestern Taxonomic Method was applied to classifying the archeological culture into divisions determined by differing analyses. The particular version of the

method used was first published in 1934 (Lyman and O'Brien 2003:Part II, 214-226). The geographical limitation and the temporal position of the archeological culture were then discussed as evidence that was independent of the material culture from which the diagnostic traits were derived. The temporal position of the White Rock aspect and the reconsidered Glen Elder focus assignment both pointed to the conclusion that the inhabitants were present in the Central Plains between A.D. 1500 and 1600 and perhaps somewhat later (Marshall 1969:94; Rusco 1960:75). The collective evidence consisted of horse bones, European trade items, simple stamped pottery, a tentative dendrochronology-derived date, and ceramic cross-finds with an early Pawnee village and with probable components of the Dismal River aspect (Gunnerson 1960:207-208, 211, 227; Marshall 1969:73-75; Newman 1963:293; Rusco 1960:41-47, 70-71; Weakly 1962:42).

Geographical distribution of the sites is somewhat arbitrary in that the archeological effort was controlled by restricted area surveys within proposed reservoir projects. Three of the sites are on White Rock Creek within the Lovewell Reservoir area of Jewell County, Kansas: White Rock (14JW1), Warne (14JW2), and Intermill (14JW202) sites. The Glen Elder site is on the Solomon River within the Glen Elder Reservoir area in Mitchell County, Kansas. Two other sites were located on the Republican River in Harlan County, Nebraska, within the limits of the Harlan County Reservoir: Green Plum (25HN39) and Blue Stone (25HN45) sites.

The Glen Elder focus is presently viewed as one of 9 and possibly 11 components of the White Rock phase (Logan 1995:92-93; Ritterbush 2006:159). Willey and Phillips (1963:22) defined the classificatory unit, phase, as "a revision of a focus that addresses temporal implications." The temporal position and geographical limitations are traits that are indicative of the archeological culture as well as the material culture. A prehistoric temporal position for the White Rock phase has been concluded based upon radiocarbon-derived dates from two sites within the Lovewell Reservoir area: the White Rock site and 14JW24, which is associated with the Warne locality (Logan 1995:4-55). The radiocarbon dates are accepted without reservation, and all earlier suggested dates are stated to be wrong (Logan

1995:93). The geographical limitation for the phase is noted to be quite arbitrary. A Warne locality apparently is meant to be a temporary designation. A "core area" also has been defined for the White Rock phase that encircles the Blue, Republican, and Solomon river drainages in north-central Kansas, extending into south-central Nebraska. Future surveys within this area, it is projected, "should clarify a definite locality and sites that are peripheral to that locality" (Logan 1995:93-94).

This review of the 1963 investigation of the Glen Elder site offers evidence that a Protohistoric-early Historic temporal position is not entirely wrong. The Protohistoric, western-like Oneota manifestation may be antecedent to the historic Kansa Indians. The reappraisal also offers evidence that mound building may be considered a tentative cultural trait of Oneota culture carriers onto the Central Plains.

GLEN ELDER SITE SETTING AND PROJECT BACKGROUND

The Glen Elder archeological site was located

The east-flowing Solomon River cut into the underlying Greenhorn limestone strata, creating a north bank about 90 feet high. A draw cut into the bank, forming two ridges. Cultural material was concentrated on the surfaces of these two ridges, covering an area on each about 300 feet north-south and 150 feet east-west. Material also rimmed the north end of the draw.

The U.S. Bureau of Reclamation was charged with constructing a dam at Glen Elder to block the Solomon River and create a reservoir. The plan was to anchor the north end of the earth-filled structure to the western ridge. The dam was planned to rise 10 feet higher than the crest of the ridge. This locale was designated as Area C (Figure 2). The completed dam extended 2½ miles south, holding water in a 976,000 acre-foot lake (Figure 3). The lake was named Waconda Lake after the famous Waconda Spring or Great Spirit Spring that was located about 3 miles west of Glen Elder. The lake inundated that spring.

The National Park Service sponsored the arche-

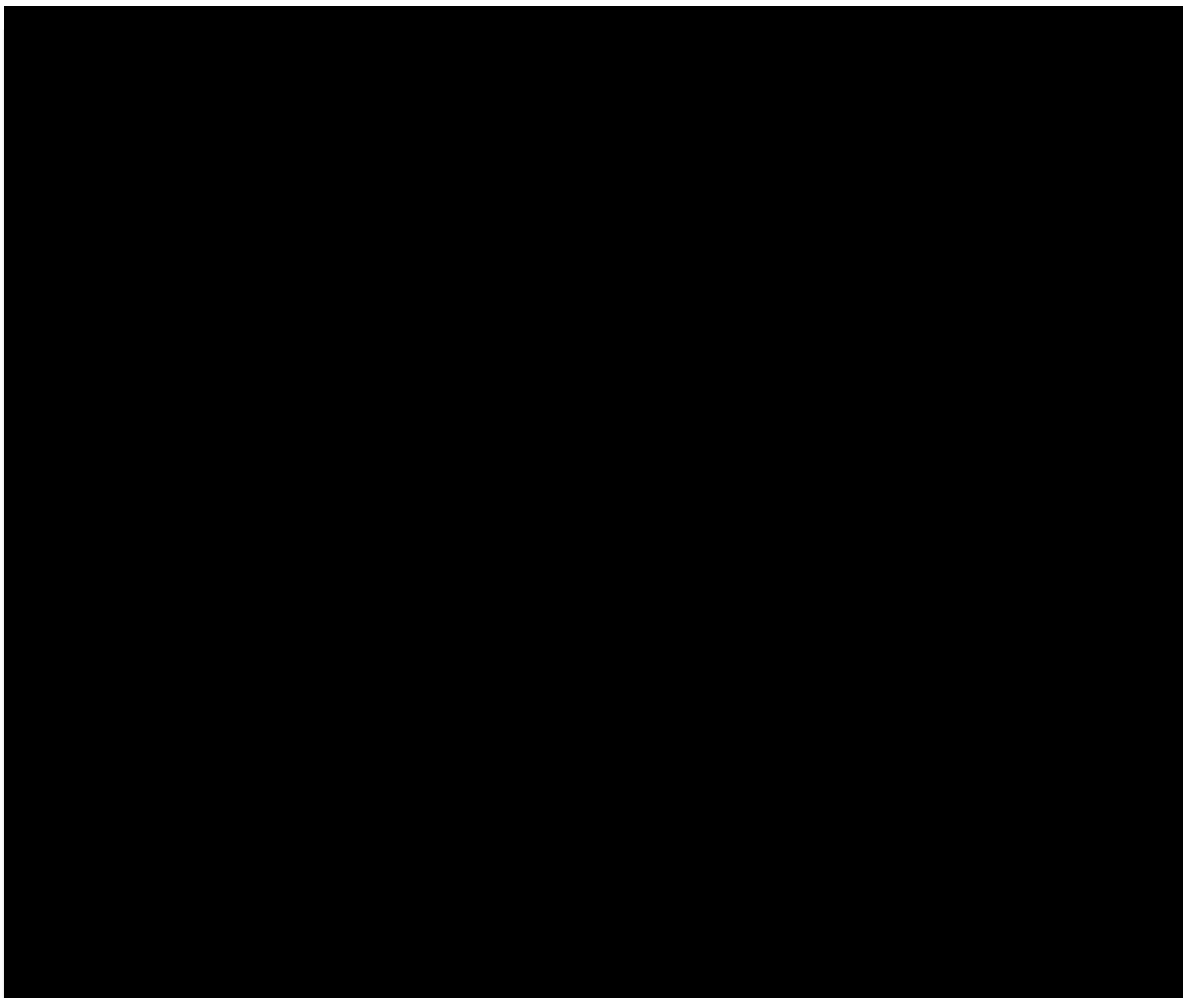


Figure 1. General location of the Glen Elder site.

ological project as part of the Interagency Archeological Salvage Program to recover information from archeological sites within the reservoir area. The Glen Elder site was the first to be investigated. A field party from the University of Nebraska (NU) Laboratory of Anthropology conducted the excavation under the supervision of Preston Holder during June and July 1963. The author was responsible for a report that was submitted to the National Park Service to satisfy contractual obligations. The same report was submitted to the NU Faculty of the Graduate College to partially fulfill requirements for a graduate degree (Marshall 1969).

Forty-five years have passed since the party of NU students descended upon the Glen Elder community. The students were Mary Ann Behlen, Robert Gellespi, Janet Goldenstein, Richard Kraus, John Phinny, Richard Reinmiller, Robert Schu-

macher, and James Sperry. Sperry and Krause were fellow graduate students with responsibilities of their own, but they made significant contributions to establishing a field residence and excavating the site. Sally Marshall cooked for the field crew and kept the residence in good order.

The hospitality extended by the community is remembered and appreciated, especially the assistance of Mr. Wayne Barnett, Mr. and Mrs. Ava Gentleman, Mr. E. W. Norris, Mr. and Mrs. Max Porter, and Mr. and Mrs. Francis Winkle. Several residents owned the land where the Glen Elder site was located, and they assented to the archeological excavations: Mr. Chester Dudley, Miss Sadie Gansel, and Mrs. Elsie Heperley. Engineers in the Bureau of Reclamation office in Beloit were always willing to help with the archeological project; their assistance with the mapping saved many hours of work.

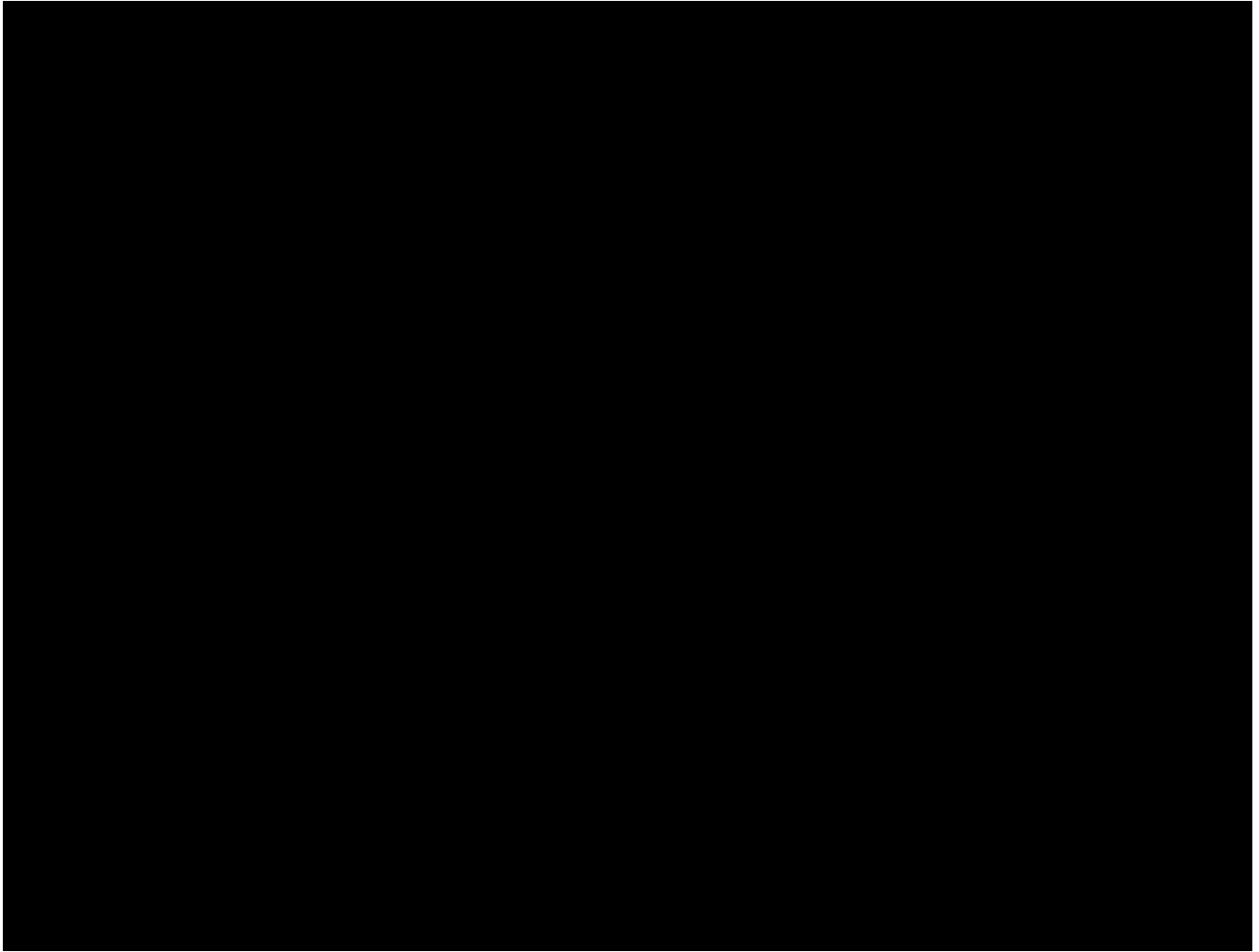


Figure 2. Aerial view of Glen Elder site in 1963. View is to the south.

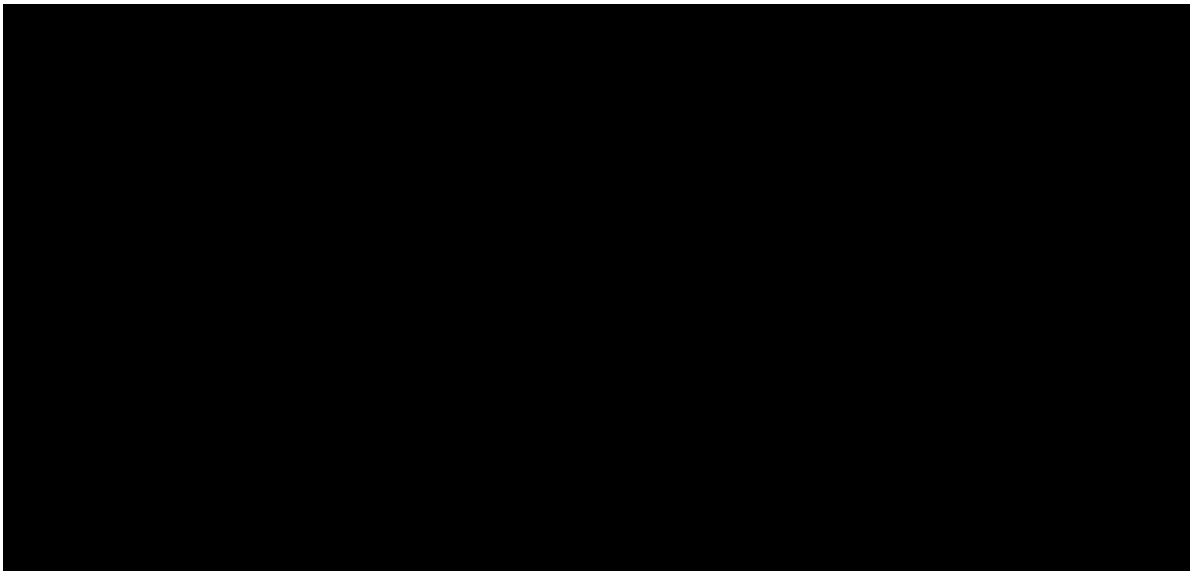


Figure 3. Aerial view of completed Glen Elder Dam. View is to the south.

INVESTIGATIONS OF THE GLEN ELDER SITE AND RELATED SITES PRIOR TO 1963

The Nebraska State Historical Society (NSHS) initiated annual statewide archeological surveys in 1933. A. T. Hill directed the program. Waldo Wedel led the 1934 survey to south-central Nebraska to conduct excavations on various archeological sites in order to better understand the Upper Republican pattern and its regional variations. After finishing on the Republican River in that year, Wedel continued his fieldwork into northern Kansas. The first Kansas site visited was the Glen Elder site or, as it was then recorded, Beloit 2. Beloit 2 is a site identification based on U.S. Geological Survey topographic maps; each site was numbered consecutively within the named topographical quadrangle on which it was located.

Wedel noted that the Glen Elder site yielded a light brown, smooth ceramic ware with incised body decorations and loop handles that was very unlike the Republican River wares (Wedel 1935:217). The landowner refused permission for the survey party to excavate, but a surface collection was made, and a local collector, E. W. Norris of Glen Elder, allowed his collection to be examined. It was learned that the site had been extensively collected over the years and that the Hastings Museum had similar material from A. T. Hill's private collection that he found near White Rock Creek in Jewell County.

The survey continued south to the Smoky Hill River where four days were spent at Salina 1 (Paint Creek site [14MP1]) in McPherson County, a site that became a suggested component of the Little River focus (Wedel 1959:571). Wedel reported on admittedly inadequate information that the pottery was characteristic of the Great Bend Lowland, and it was "likely that the ware is also related to ... Beloit 2 ... and to that found on White Rock Creek in Jewell County" (Wedel 1935:252). Following William Duncan Strong's example and using the newly devised Midwestern Taxonomic System, Wedel classified Beloit 2 and sites on White Rock Creek as questionable components of the White Rock Creek focus of the Smoky Hill-Great Bend aspect (Wedel 1935:252).

Wedel temporally positioned the Smoky Hill-Great Bend aspect within the Protohistoric pe-

riod. His evidence for doing so was collected from Salina 1 by pioneer Kansas archeologist and geologist Johan August Udden. Udden (1900) found and reported a pottery sherd identical to the type that was known for Pawnee sites on the Loup River in Nebraska and a fragment of chainmail armor. This ceramic cross-find and evidence of European contact from Salina 1 led Wedel to postulate a Protohistoric temporal position for the Smoky Hill-Great Bend aspect to which Beloit 2 was thought to be related.

The NSHS concentrated its 1935 season in southeastern Nebraska. The most significant site excavated was the Leary site, located on the Nemaha River in Richardson County. The Leary site archeological material was noted to be quite different from the more familiar Upper Republican and Pawnee materials but ceramically similar to the Kansas sites. In summarizing their thoughts on this newly discovered complex, Hill and Wedel noted that relationships existed between Leary and the sites that had been found in Kansas on White Rock Creek and at Glen Elder, but the relationship was not clear (Hill and Wedel 1936:40).

The Leary site was compared most favorably to the Oneota complex, an archeological culture that was discovered in Iowa. The Oneota manifestations in Iowa included European trade articles. On the basis of this trait and the rest of the archeological material, Hill and Wedel (1936:65-67) relatively dated the Leary site as belonging in the late prehistoric or early Protohistoric periods. They stated, "Oneota-like traits do not seem to appear in Kansas until protohistoric or at the earliest, very late prehistoric times" (Hill and Wedel 1936:69). It can only be assumed that the authors were thinking of the Beloit 2 site at Glen Elder and those on White Rock Creek as these were the only Kansas sites reported at this time as resembling the Oneota archeological culture and temporally established in the Protohistoric period.

The 1935 season's work included a short visit by George F. Lamb to one of the Jewell County sites on White Rock Creek: the White Rock site or 14JW1, using the contemporary system of designating archeological sites. His test excavation uncovered a fire pit and a cache pit, which he recorded as a house site. These features were relocated two years later when Lamb and Paul Cooper returned to

the site to conclude the 1937 survey by excavating what they concluded were two houses (Cooper and Lamb 1937). They also did some excavating at the Glen Elder site, but the results were not reported until Mary Kiehl Rusco (1960:71) defined the Glen Elder focus.

The rest of the archeological sites that are a part of this complex were excavated under the auspices of the Smithsonian Institution (SI) River Basin Survey (RBS): Green Plum, Blue Stone, Warne, and Intermill. A SI survey party found the Green Plum site in the Harlan County Reservoir area in 1946. A NU Department of Anthropology field school excavated the site in 1950. The Blue Stone site was found in the same reservoir area by the university in 1948 and was excavated in 1952. John L. Champe supervised both excavations (Rusco 1960:3). The White Rock, Warne, and Intermill sites came under the scrutiny of the 1951 SI survey of the Lovewell Reservoir area (Fenenga and Cooper 1951). A Smithsonian field party, directed by Robert W. Newman (1963:262), excavated the sites in 1956. The Glen Elder site was revisited in 1949 when a crew from the University of Kansas Museum of Natural History, led by Carlyle S. Smith, conducted a preliminary survey of the Glen Elder Reservoir area. A pottery sample was gathered from the site, and specimens were acquired from local collector E. W. Norris. Smith (1945:5-6) compared this pottery with samples from the Warne, Green Plum, and Blue Stone sites and suggested on the basis of their similarities that these complexes be classified as the Glen Elder focus. In 1952 a party from the RBS surveyed the site again (Solecki and Shippee 1952).

THE 1963 ARCHEOLOGICAL PROJECT

Condition of the Glen Elder Site in 1963

The immediate vicinity of the Glen Elder site had been disturbed over the years by gardening and pasturing activities. The western growth of the village of Glen Elder, the construction of the Missouri Pacific Railroad, and extensive gravel quarrying on the southern portion of the eastern ridge had contributed to the obliteration of the site. A few relatively undisturbed patches were noticed where native grasses still flourished, but most areas were

covered by invasive weed species as a result of pasturing farm stock and cultivation. The eastern ridge and a portion of the western ridge had been used to pasture goats since the 1940s. The rest of the western ridge was in corn. Amateur collectors had taken archeological material from the surface of the site for more than 30 years.

A thin layer of loam covered the ridges, ranging from about ½ foot deep on their crests to 1 foot at the edges. A band of Loveland loess lay under the loam. The loess band was quite irregular because the underlying limestone was just under the ground surface in some areas. Charles Osborn, regional geologist with the Bureau of Reclamation McCook Division, made a field identification of the fossilized mussel shell imprints in the limestone as Genus *Inoceramus*, Species *labiatus*. These shells are probably from the Pfeifer member of the Greenhorn limestone of the Upper Cretaceous in the Gulfian series (Landes 1930:23; Osborn, personal communication 1963).

Method of Excavation

In preparing for the 1963 excavation, the site was divided into four areas, noted with the letters A, B, C, and D (Figures 2 and 4). The Bureau of Reclamation had established a benchmark on the mid-portion of the eastern ridge. This benchmark was used as a primary datum from which baseline was sighted true north. At every 100-foot interval a 90-degree angle was turned to establish a point 100 feet east and west of the baseline. A 90-degree angle was turned at the 100-foot point on the baseline, and a line of sight 900 feet west established a station on the western ridge. A second north-south baseline was sighted along the western ridge, and a 100-foot grid system was marked off as was done on the eastern ridge. A station on the eastern ridge baseline that defined the southern limit of Area A was marked as Station 0, and from this station intervals were marked running to the north (Figure 5).

Area A extended north from Station 0 to an east-west-orientated road. Area B included the land north of the east-west road and a field west of a standpipe that was bordered by a north-south road. Area C covered the western ridge, and Area D was the gravel quarry on the eastern ridge south of Station 0 (Figure 4).



Figure 4. Contour map of Glen Elder site, showing areas A, B, C, D and location of 14ML301.

Exploratory test trenches were opened where cultural material seemed most heavily concentrated on the surface of the ground. These exploratory trenches were recorded as excavation units. When anything of archeological significance was exposed, the unit was expanded to define the unnatural complex and possible associations, and it was recorded as a feature. The excavation units were numbered in a 100 series, with each number preceded by the letter X. Features were numbered in a 200 series,

with each number preceded by the letter F. The excavation units were numbered in the order that they were opened, and the features were numbered in the order that they were noticed.

A tractor with a front-end scraper and backhoe attachment and a road grader were employed part of the time to strip off the sod over large portions of the site. When the road maintainer uncovered an unnatural complex, the point was marked and later positioned on the grid system. Windrows left by the

machine were cleared away by shovel. If the unnatural complex proved significant, it was given a feature number. This system allowed recording what the machine exposed without stopping it; therefore, shutdown expenses were not incurred.

Each member of the field party kept standard NU Laboratory of Anthropology record sheets. Specimens were bagged with pertinent information recorded on each sack. The specimens, numbering 4079, were washed and recorded in a field catalogue at a field laboratory where they were taken daily from the site. A photographic record was kept, and field maps were drawn of significant features. The specimens, original records with two copied sets, 220 black and white photographs, and 169 colored slides are on file at the Kansas Historical Society.

Excavation of a Mound Remnant

Immediately north and east of Station 0 in Area A, a mound remnant was uncovered (Figure 6). A concentration of 847 limestone rocks, laid in a circular pattern, measured 60 feet north-south and 55 feet east-west. The rocks were not evenly distributed. The heaviest concentration was at the southeastern corner of the feature. The stones gradually thinned out to the north, but fragments increased. Three profiles were recorded, starting from a point 31 feet north of Station 0. The rocks sloped 2.8 feet from the northwest to the southeast within a distance of 48 feet. As the profile line was moved toward a north-south direction, this slope decreased to a depth of 1 foot within a distance of 34 feet.

The soil covering the concentration of rocks was a dark loam heavily flecked with limestone fragments. The thickness of this soil band increased from .6 to 2.8 feet north-south. The rocks were laid on a light tan soil, identified as Loveland Loess, which was culturally sterile.

Skirting the southern edge of the rock concentration were remnants of an intermediate soil that was gray in color and had a harder or more compact texture than the loess or the loam coverings. There was no mixture of limestone fragments in this intermediate soil (Figure 6). In an area 17 feet east of the baseline, the intermediate soil was plated with a single layer of limestone rocks. The plating, 2 feet higher than the base of the mound, covered a

triangular area. The sides of the triangle measured 14.4 feet x 17 feet x 7 feet (Figure 6). The intermediate soil remnants were joined by an arching line, caused by the deposition of one soil on top of another. This effect is usually described as "feathering" (Figure 7).

The western edge of the mound could be defined by the limestone rock concentration, but the northern edge was quite vague except for scattered rock. A soil change was noticed twice at about 5 inches below the surface, but the soil dried out into an unrecognizable blend with the surface loam almost immediately after being exposed to the sun. The whole arc, however, formed by scattered rock and the extent of the loam mixed with limestone fragments, indicated quite accurately the limits of the mound.

South and east of the southern edge of the mound were more scattered rocks. These were interpreted as being part of the original mound, redistributed by plowing. The remainder of the mound remained intact because its depth protected it. The area of the plating on the intermediate soil and other pockets of the intermediate soil were crossed by a fence line, which protected these features from cultivation.

Four pits were noted as directly associated with the mound. Beneath the circular limestone concentration were four shallow straight-sided pits, labeled as Pits 1, 2, 3 and 4 (Figure 6). Each pit showed as a black area against the light tan loess at depths that varied from 4.8 to 8.4 inches below the ground surface. Diameters of the pits ranged from 2 feet to 4 feet, and their depths were between .6 and 1.6 feet. Pit 1 had a multi-level floor. The west half of the pit was 1.6 feet and contained 35 limestone rocks; the east half of the pit was 8.4 inches deep (Figure 6). The fill of all pits was culturally sterile.

A few pottery sherds, bone fragments, chert pieces, and a projectile point were recovered from the mound remnant overburden. After the intermediate soil beneath the rock plating was defined, the soils were removed in a search for more substructure features. Under the plating and mixed in the intermediate soil at a depth of 10.8 inches, 58 pottery sherds were found. The 2 rim and 56 body sherds are of the Walnut Decorated Lip type. Bone and shell were recovered with the pottery sherds. The bone was fragmentary and burned except for

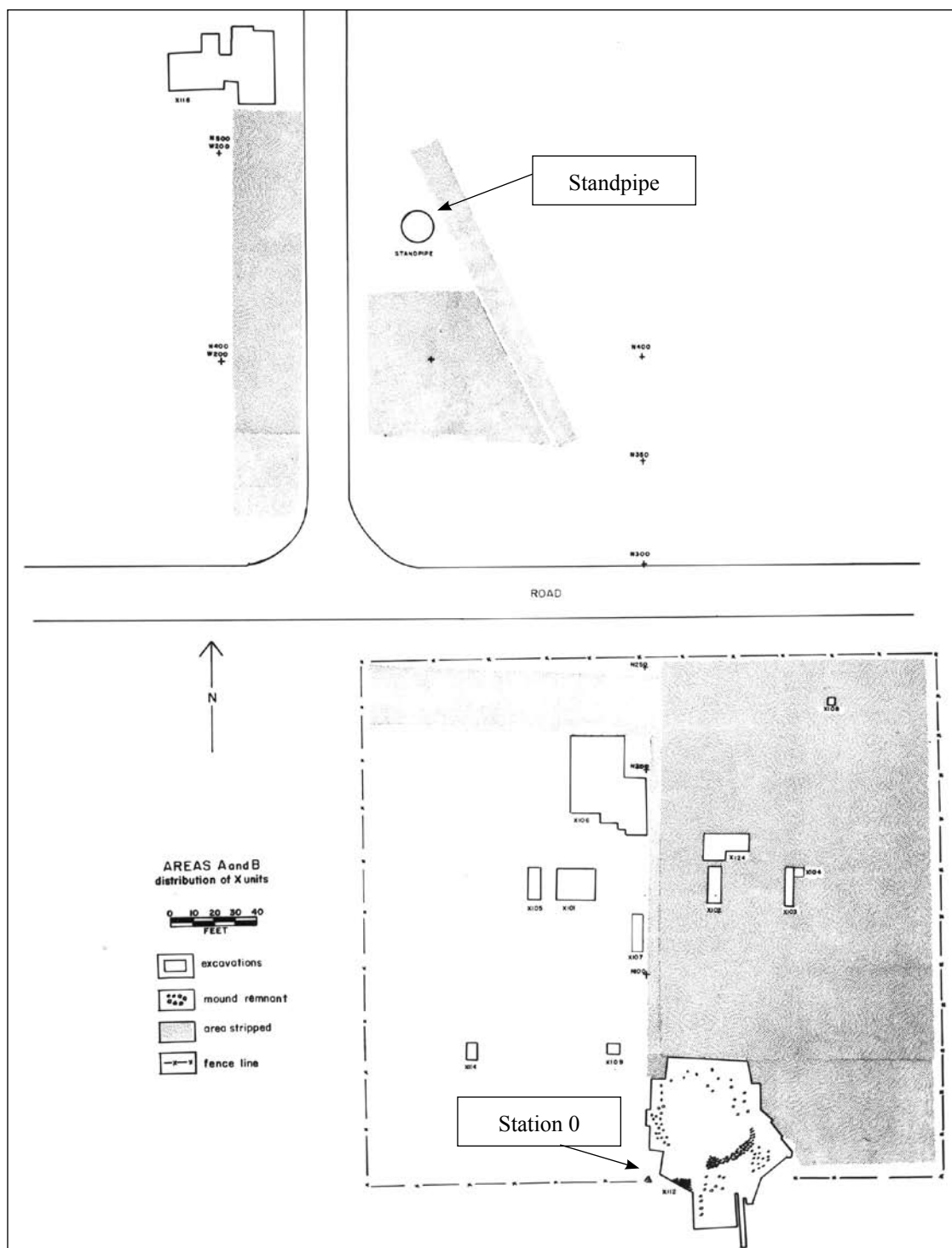


Figure 5. Map of excavation units in Areas A and B and mound remnant in Area A. Gray shading indicates areas where road grader removed top soil.

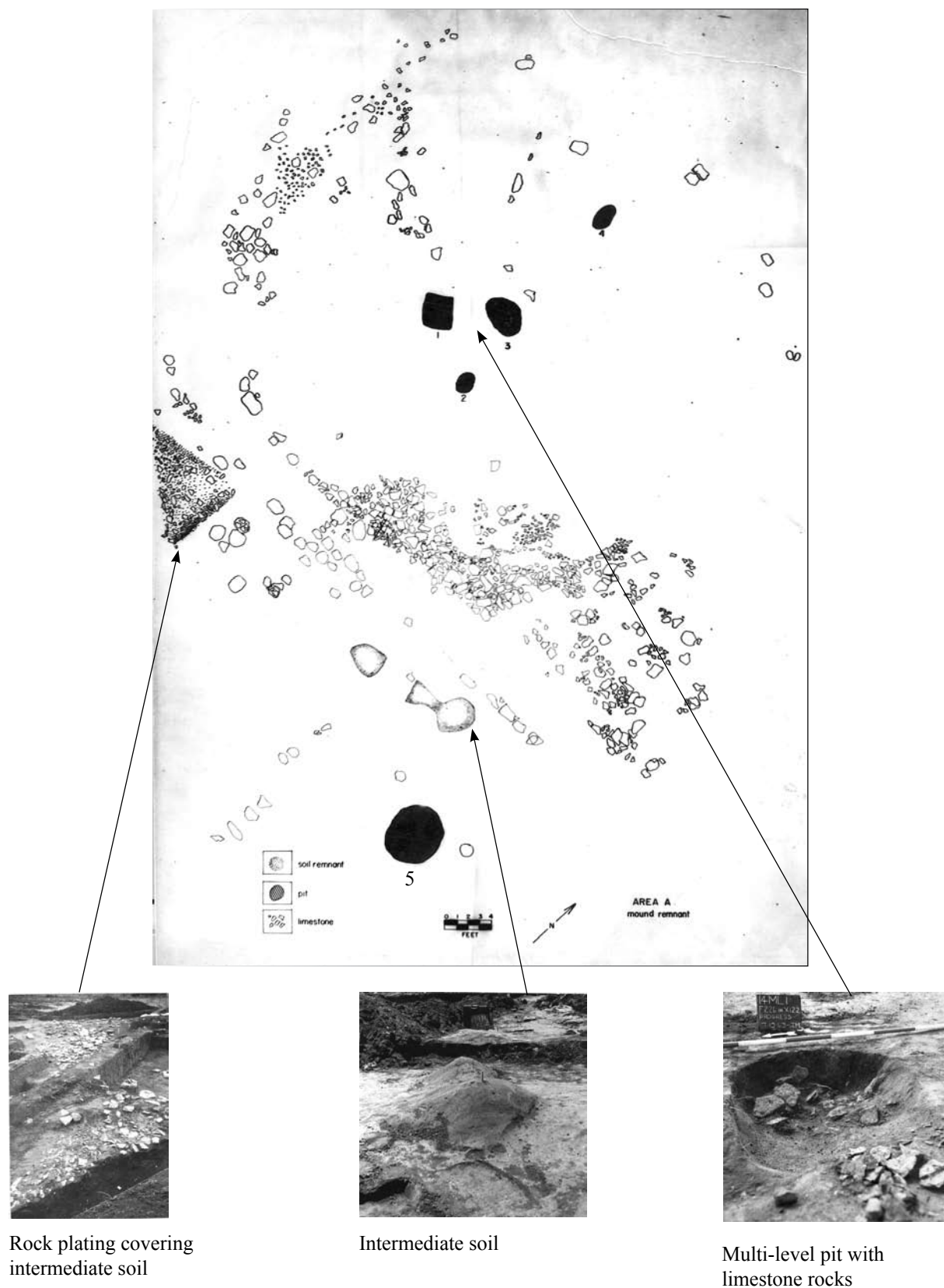


Figure 6. Plan view of mound remnant in Area A.



Figure 7. The “feathering” line.

the base of an awl. There was no indication that the soil had been disturbed by an intrusive pit or any other factor to negate the evidence that the pottery sherds had been mixed with the soil during mound construction.

There were two mounds on the Glen Elder site in addition to the mound remnant. One had been destroyed by the growth of the village of Glen Elder (Cooper and Lamb 1937). The other, 14ML301, still existed but had been badly disturbed by artifact hunters (Figure 4). No diagnostic cultural material was recovered, other than some sections of wire and a coal shovel found at the bottom of an artifact hunters’ hole. This hole extended down through the middle of the mound to a depth of 7 feet from the surface of the apex of the structure (Figure 8). At its summit the mound was approximately 25 feet in diameter and 3 feet high above the ground surface. The structure was built by interlocking slabs of limestone. The stones were laid horizontally at the periphery, but as the center was approached, the stones were turned on end, creating a “fanning” effect. Fragments of badly burned human bone and a scapula and clavicle of a hog or peccary were recovered from the rocks near the center of the mound. The animal bones were tentatively identified by Robert Eisele, a paleontologist on the staff

of the NU Nebraska State Museum (Robert Eisele, personal communication 1964).

Summary of Mounds in Relation to Glen Elder Components

The mound remnant excavated at the Glen Elder site is not a new feature. Paul Cooper (1955) suggested the possibility that mounds are a part of the archeological culture in reference to the Jewell County sites. Among the occupational sites (14JW1, 14JW2, 14JW202, and 14JW205) were two mounds: 14JW204 and 14JW207. One type of pottery sherd (Walnut Decorated Lip) was found near the mounds and on the occupational sites, leading Cooper to speculate that the mounds represented the burial practice for the occupants. But the mounds, he cautioned, might relate to some other manifestation (Cooper 1955:14).

An investigation in 1955 at the Russell mound (14JW207) produced evidence that the occupational site and the mound were two different manifestations. Pottery sherds recovered from undisturbed portions of the mound were found to be similar to specimens that had been recovered from a mound in Nebraska at the Weeping Water site (25CC34), which was classified as a component of

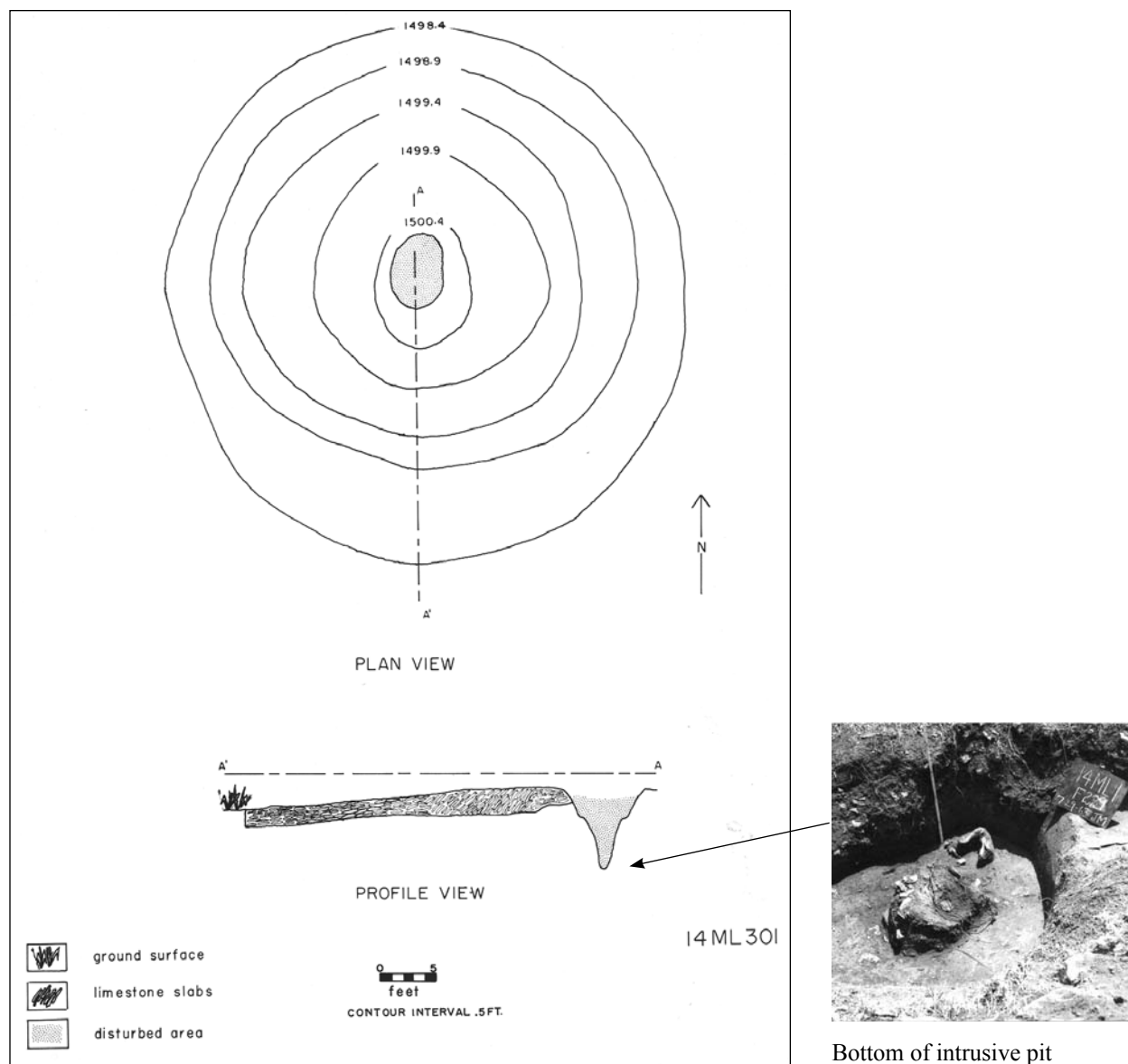


Figure 8. Plan and profile views of mound designated 14ML301.

the Nebraska focus, Aksarban aspect. Pottery sherds identified as Walnut Decorated Lip were recovered around the periphery of the mound (Newman 1963:296-297).

Three years earlier in 1953, Robert B. Cumming (1958:48-47) excavated Sweatbee Mound (14PO14) in Pottawatomie County. The mound covered a pit that contained seven individuals. Above this feature was a single, fully extended burial that seemed to be intrusive. No diagnostic cultural material was recovered. Cumming (1958:55) suggested

a Woodland affiliation for the mound, based upon similarities of the burial pattern with Woodland manifestations. Pottery similar to that collected at the Glen Elder site was found at the Spillway site (14PO12), located 500 yards distant from Sweatbee Mound. The Spillway site was considered to be an occupational site rather than a mound site.

The recovery of Walnut Decorated Lip pottery from the apparent undisturbed intermediate soil of the Glen Elder mound remnant is the best evidence that mound building may be included

as a cultural trait for this culture, as it is for some Oneota manifestations in Iowa and eastern South Dakota (Henning 1998:347). The mound can be interpreted to have been an earth-filled structure that was covered with a plating of rocks. Other mounds found on White Rock phase components have been constructed with rocks. These rock-filled mounds may have been tampered with by the western-like Oneota people, but the structures were built by other occupants, such as earlier Woodland people.

Excavation of the Glen Elder Occupational Site

Excavation of the Glen Elder occupational site uncovered 10 post molds, 10 basin-shaped pits, 5 fire pits, and 10 concentrations of midden debris. These features were all recovered at depths from 6 to 9.6 inches below ground surface.

The post molds never formed a particular pattern of distribution in relation to other features or with each other. Dimensions ranged from 4.8 to 7.2 inches in diameter and from 6 to 9.6 inches deep.

The pits were all basin shaped and ranged in size from .7 to 2.2 feet in diameter and from .2 to 1.6 feet deep. Contents consisted of midden debris. One larger pit, Pit 5, located just outside of the southern limit of the mound remnant, had a shallow basin shape and measured 5.2 feet in diameter and 6 inches deep (Figure 6). The pit contained midden debris, including Walnut Decorated Lip pottery sherds and fragments of bone and stones. Two pits differed from the rest in that they seemingly were left in their original state when the site was abandoned. The first pit contained 148 chipped stone specimens of which 3 were blades and 108 were scrapers; the rest were flakes. The dimensions of the pit were 8.4 inches in diameter and 4.8 inches deep (Figure 10). Two bison scapulae, lying side by side, were recovered from the second pit, which measured 1.4 feet in diameter and 7.2 inches deep (Figure 10).

The five fire pits were identified as such because the dirt that filled them had been extensively burned and was mixed with badly burned bone fragments. Four of the pits ranged in size from 1 to 1.8 feet in diameter and 3.6 to 8.4 inches deep. The fifth pit differed from the rest in size and seemed to be associated with a smaller pit located immediately next to it (Figure 10). The larger pit measured 2 feet

in diameter and was 1.1 feet deep. The dirt within it was consistently black from the surface to the floor of the feature. At 2.4 inches southeast of the edge of the large pit was the edge of a smaller pit. This oval feature measured 1.1 feet north-south and 9.6 inches east-west; the depth was 1.1 feet. The soil within the smaller pit was perfectly clean, meaning that there was no evidence of burning.

Concentrations of midden debris ranged from a few inches in extent to 7 feet in diameter. One concentration that measured 1.1 feet north-south, 9.6 inches east-west, and 3.6 inches deep was particularly productive in that whole specimens were recovered from it: a polished celt, an abrading stone, scrapers, and a container made of bone and shell. A shallow basin-shaped pit, located next to the concentration of artifacts, contained a milling stone and pieces of two bison scapula (Figure 10).

Summary of the Excavation of the Glen Elder Occupational Site

The excavation of the occupational site complements what has been recorded for other components of the White Rock phase before and since 1963. The feature inventory lists randomly scattered post molds, basin-shaped pits, fire pits, and concentrations of midden debris. Two unusual exceptions found at the Glen Elder site were a small pit filled with chipped stone implements and a fire pit that was paired with a smaller pit.

The small pit containing 148 chipped stone implements is thought to be a flintknapper's kit. The tools may have been in a leather pouch when they were stored in the ground (Figure 10). The small pit located immediately adjacent to a larger fire pit may have been a warming oven where food placed in the small pit was warmed by heat emitting from the fire pit. It is possible that the position of these two pits is a coincidence; one pit may have been unused when the other was dug, and they just happened to be adjacent to one another.

The post molds at the Glen Elder site never formed a pattern that would indicate a dwelling consistent with Rusco's (1960:41) findings at the Green Plum and Blue Stone sites. The evidence also concurred with Neuman's (1963:265, 287) findings at the White Rock, Intermill, and Warne sites. On the basis of the evidence collected from these sites

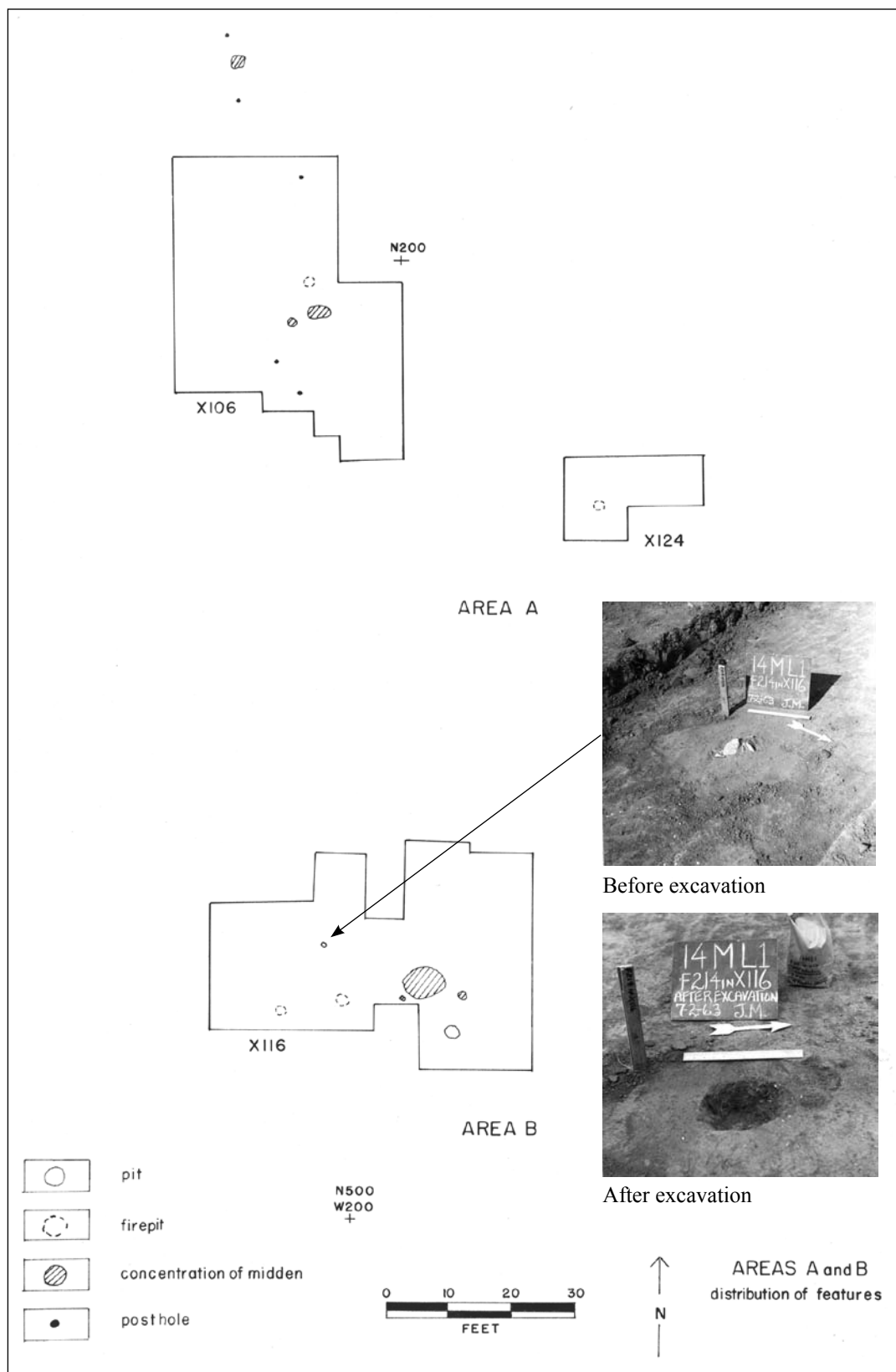


Figure 9. Plan view of excavation units and features in Areas A and B.

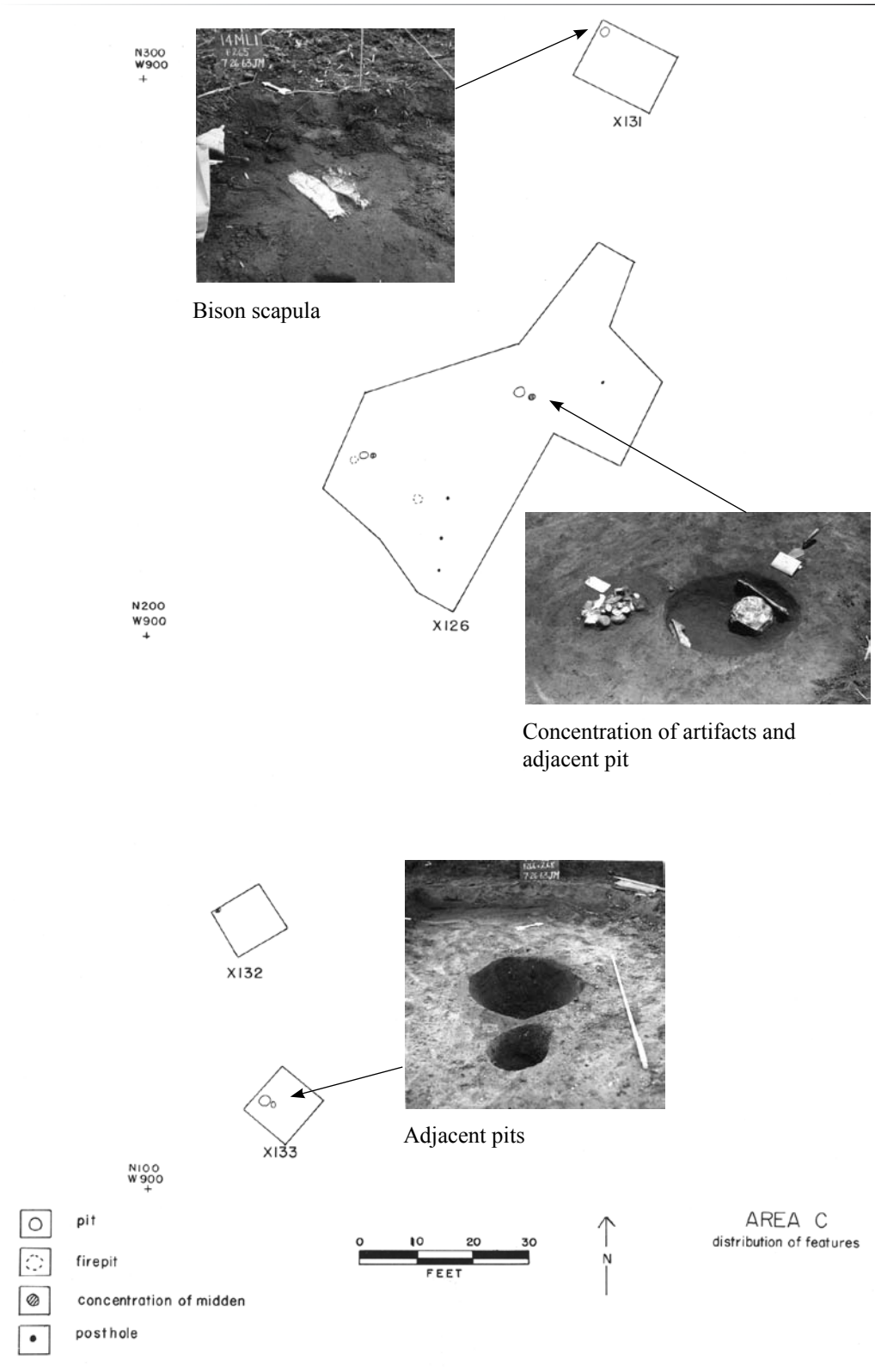


Figure 10. Plan view of excavation units and features in Area C.

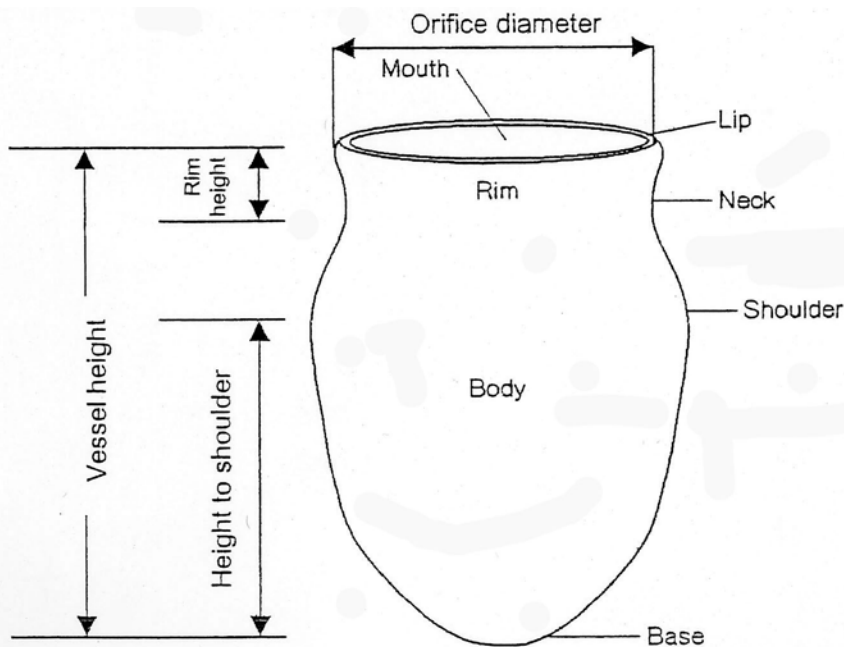


Figure 11. Terminology used in pottery descriptions. (Modified from Hoard et al. 2003:17.)

and the Glen Elder site, it is concluded that particular house structures were non-existent. Future excavations of White Rock phase components may reveal more convincing post mold patterns, and then a structure may be defined and the intended permanency of the structure may become apparent. The type of abode for the White Rock phase can only be inferred, and as has been noted, it is an inference that awaits testing (Logan 1995:91).

ARTIFACT DESCRIPTIONS

Pottery sherds, worked and unworked stone, bone, shell, and clay were recovered from the Glen Elder site. The description of the pottery follows a checklist that has been used extensively in Plains archeology (Wheeler 1954). The paste, surface finish, decoration, and form are discussed in that order. The hardness of the pottery was determined by the Moh's hardness test. A particular mineral, rated from 1 (Talc) to 10 (Diamond), leaves a scratch mark on the surface of a sherd. Calcite, which is rated number 3, or a mineral rated higher will leave a scratch on the surface of Walnut Decorated Lip pottery (Tong 2000). Description of the pottery wherein differs from that in the 1969 report in

that differences are more precisely noted. Ceramic cross-finds associate the Burkett site (25NC1) and the Fanning site with Glen Elder.

The worked stone, bone, shell, and clay specimens are grouped according to type of tool or implement, based on its assumed function and method of manufacture. The implements are listed as projectile points, scrapers, graters, knives, choppers, an axe, abraders, grooved mauls, hammerstones, milling stones, a grinding stone, celts, hoes, an awl, a bone and shell container, and a perforated disk. A few specimens made of modern materials, such as glass or iron, are not of Native American manufacture and are labeled as such.

Pottery Description

The total pottery sample consists of 187 rim sherds, 1,136 body sherds, and 17 handles. The rim sections were selected from a collection of 230 rims recovered from 14ML1 in 1937 and 98 rims recovered in 1963. Too small for meaningful description, 141 rim sherds were excluded from analysis. The pottery descriptions follow a nomenclature and checklist that has been used extensively in Plains archeology (Wheeler 1954; Figure 11).

WALNUT DECORATED LIP

Sample: 152 rim sherds and 931 body sherds

Paste:

Temper: grains of rounded quartz, volcanic rock, and muscovite or white mica

Texture: granular

Hardness: 3 (calcite)

Color: orange, buff, and gray or combinations of these colors

Surface Finish: Tempering particles protrude from the inner and outer surfaces, giving sherds an abrasive feel.

Decoration:

Techniques: decorative designs made with an incising tool and with finger or blunt instrument

Patterns: opposed diagonals of incised lines and vertically orientated, parallel, trailed lines. Elongated impressions, circular impressions, and finger impressions are also decorative motifs.

Distribution: Incised and trailed lines are restricted to vessel shoulders (Figure 12).

Elongated diagonal tool impressions, circular impressions, and finger impressions decorate lip portions of rim sherds. Lip surfaces on 95 specimens are decorated with elongated, diagonal tool impressions that run from left to right (Figure 12 A and B). Impressions on only two rim sherds run from right to left. Elongated diagonal impressions range in size from 3 mm wide and spaced 2 mm apart to 5 mm wide and spaced 10 mm apart. Lengths of impressions vary from 5 to 17 mm.

Lip interiors are decorated with elongated diagonal impressions on 47 rim sherds. Fifteen other rim sherds exhibit elongated impressions, which are vertically or horizontally positioned in relation to the lips, and different combinations of decorative patterns using circular and finger impressions. The surfaces and either the lip interiors or exteriors are decorated on five rim sherds. One combination is elongated diagonal tool impressions running from left to right on the lip surface and from right to left along the lip interior. Another combination is circular impressions on the lip surface with elongated diagonal impressions on the lip interior. Lastly, circular impressions decorate the lip surface and finger impressions are placed along the lip interior. Circular impressions measure 2 to 6 mm in diameter and are 4 to 6 mm apart. Wide, shallow finger



Figure 12. Rim sherds of Walnut Decorated Lip pottery type.

impressions average 12 mm in diameter, and the spacing varies from 2 to 9 mm.

Form:

Lip: flat or round depending on the placement of decorative impressions

Rim: 71 rims are straight and out-flaring (Figure 12 C). Dimensions of straight rims range from 11 to 29 mm high. Fifty-nine rims are recurved (Figure 12 A and B) and range from 11 to 23 mm high.

Thickness: All sherds vary from 3 to 5mm.

Body sherds (Figure 13): One hundred-two body sherds are large enough to show decorative patterns. These sherds are most likely from vessel shoulder sections. Sixty-four sherds exhibit incised lines (Figure 13 A). Thirty-six other sherds exhibit trailed lines (Figure 13 B). Two sherds have a decorative pattern that differs in that circular impres-

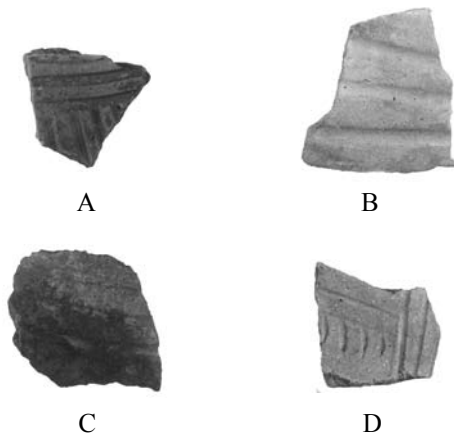


Figure 13. Body sherds of Walnut Decorated Lip pottery type.

sions or elongated impressions border incised lines (Figure 13 C and D).

Appendages: Handles attached to the vessels are both strap and loop varieties. Strap handles are wider than they are thick, while loop handles are thicker than they are wide. A very preliminary projection is that 22 percent of a collection of whole vessels will have handles, 75 percent of those handles will be attached to vessel lips, and 25 percent will be attached to the rims just below the lips (Grange 1968:66).

Strap handles: Two grit-tempered strap handles are decorated with vertical incised lines that run half of the handle length. One is decorated with four lines and the other, a handle fragment, has seven lines. The incised lines are 2 to 3 mm wide and are 2 to 3 mm apart.

Four grit-tempered strap handles are undecorated and 6 mm thick. Three are consistently broad, ranging from 26 to 38 mm wide. The fourth handle is wider at the junction with the lip (40 mm) than at the junction with the shoulder portion of the rim sherd (19 mm).

Two grit-tempered strap handles are decorated with single trailed lines that run vertically the length of the appendages. The lip on the specimen that is illustrated as an example is decorated with diagonal finger impressions that run slightly from left to right. The impressions are 11 mm long, 5 mm wide, and 5 mm apart. The trailed line that decorates the handle, which itself is 15 mm wide,



Figure 14. Decorated strap handle of Walnut Decorated Lip pottery type.

measures 4 mm wide. The handle approaches the form of a loop handle rather than that of a strap handle (Figure 14).

Loop handles: Two grit-tempered loop handles have been left undecorated. Both specimens are 10 mm wide and 10 mm thick.

Two handles are decorated with incised lines or with circular or finger impression. One example of decoration consists of both incised lines and circular impressions. Circular impressions decorate the lip portion of the grit-tempered rim sherd, and these continue over the back of the handle as four impressions placed between two incised lines. The impressions decrease in size from 12 to 7 mm in diameter, running from top to bottom. The impressions on the lip of the rim are 4 mm in diameter and 4 mm apart. The second grit-tempered rim sherd has an attached handle that is 16 mm wide and 10 mm thick. It is decorated with a series of four horizontal finger impressions that transverse the back of the appendage (Figure 15). The indentations are 10 mm long, 4 mm wide, and 3 mm apart.

On the last grit-tempered loop appendage (Figure 16), the back of the handle is decorated with three series of small elongated indentations, each of which is about 3 mm long and 1 mm wide. They are spaced 2 to 3 mm apart, running vertically over the back of the handle. The lip portion of the specimen is decorated with a left-to-right diagonal elongated impression that is 6 mm long and 2 mm wide. The handle is 13 mm wide and 10 mm thick.

WALNUT DECORATED LIP, SHELL TEMPERED VARIETY

Sample: 5 rim sherds and 13 body sherds

Discussion: Walnut Decorated Lip, Shell-Tempered Variety shares most of the characteristics of Wal-

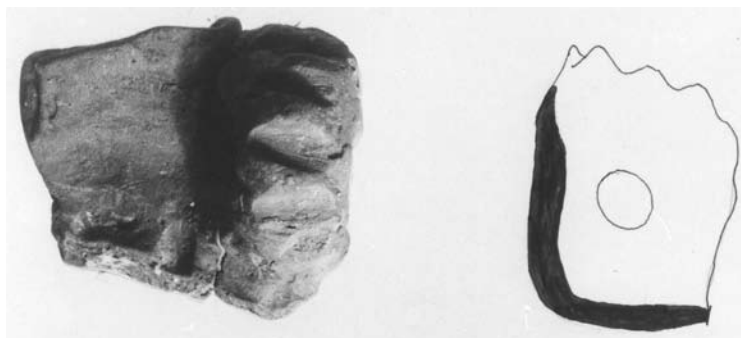


Figure 15. Decorated loop handle of Walnut Decorated Lip pottery type.

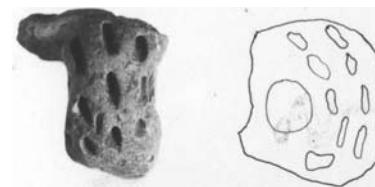


Figure 16. Decorated loop handle of Walnut Decorated Lip pottery type.

nut Decorated Lip pottery except for the tempering material. Shell particles mixed into the paste cause sherd cores to appear laminated. Sherd thickness is 8 mm, which is slightly thicker than the grit-tempered Walnut Decorated Lip sherds.

Appendages:

Strap handles: The handle on the shell-tempered rim sherd is attached to the lip and to the shoulder. Decorating the upper half of the handle are three vertical incised lines, each 6 mm wide and 6 mm apart. The handle itself is 44 mm wide and 4 mm thick (Figure 17).

A more elaborately decorated shell-tempered strap handle is 25 mm wide. The trailed line over the back of the handle is 15 mm wide. Two crossed lines are incised within the trailed line. The handle is welded to the rim below the lip and to the shoulder portion of the vessel (Figure 18).

Loop handle: The shell-tempered loop handle is attached to a rim with interior decorations (Figure 19). The rim interior is marked with three concentric chevrons. The incised chevrons are 2 mm wide and 5 mm apart. The back of the handle is aligned with seven indentations each of which is 7 mm long and 4 mm wide and spaced 3 mm apart. The handle itself is 16 mm wide and 10 mm thick (Figure 19).

FANNING PLAIN

Sample: 5 rim sherds and 13 body sherds. A section of a vessel with an attached appendage was reconstructed from fragments (Figure 20).

Paste:

Temper: mussel shell particles

Texture: fine. The paste in the sherd cores is laminated.

Hardness: 3 (calcite)

Color: dark gray

Surface Finish: smooth

Decoration:

Techniques: finger or blunt instrument

Pattern: trailed lines and wide, shallow impressions

Distribution: Decorative motifs are restricted to lip portions of rims and to handles. Lips are decorated with series of elongated diagonal impressions that run from left to right. Elongated impressions are 10 mm long and 5 mm wide and 5 mm apart.

Form:

Lip: flat

Rim: straight and out-flaring

Neck: constricted

Shoulders: well defined. A whole vessel might reveal that the diameter across the shoulders is greater than the vessel height.

Dimensions: Rim is 17 mm high, and remainder of vessel body is 8 mm thick.

Appendage: The appendage is a decorated strap handle that is 44 mm wide and 4 mm thick. Three vertical trailed lines, measuring 6 mm wide and spaced 6 mm apart, decorate the back of the handle. The handle is attached to the rim lip at the junction of the shoulder and the rim (Figure 20).

NANCE FLARED DECORATED

Sample: 5 rim sherds

Paste:

Tempering: grit

Texture: fine

Hardness: 3 (calcite)

Color: light brown or buff

Surface Finish: smooth



Figure 17. Rim sherd with appendage of Walnut Decorated Lip pottery type, shell-tempered variety.



Figure 18. Decorated strap handle of Walnut Decorated Lip pottery type, shell-tempered variety.



Figure 19. Decorated loop handle of Walnut Decorated Lip pottery type, shell-tempered variety.

Decoration:

Techniques: A sharply pointed incising tool was used to create narrow incised lines.

Pattern: various numbers of parallel, incised lines, and diagonal incised lines

Distribution: Parallel incised lines decorate the

outer rim surface and the neck portion of the vessels.

Form:

Lip: flat

Rim: straight and out-flaring

Neck: constricted



Figure 20. Section of vessel of Fanning Plain pottery type.

Shoulder: well defined but sloping

Description: Two of the rim sherds exhibit five incised lines that would circumscribe the vessel. One rim sherd, decorated with five lines, has a series of connecting chevrons between the fourth and fifth lines. Diagonal parallel lines decorate the neck and shoulder portions of the rim sherd. The incised lines are 1 mm wide or less, and the rim is 15 mm high (Figure 21). One other rim sherd is decorated with one incised line inscribed at the neck of the vessel.

UNNAMED POTTERY TYPE

Sample: 2 rim sherds

Paste:

Tempering: grit

Texture: granular

Hardness: 4 (fluorite)

Color: gray

Surface Finish: simple-stamped

Decoration:

Techniques: A small stick was used to apply narrow impressions, and a blunt instrument or the finger was used to create larger impressions.

Pattern: The decorative motifs are narrow elongated diagonal impressions or wide horizontally aligned finger impressions.

Distribution: The impressions have been applied to the interiors of the lips on the rim sherds. The

elongated diagonal impressions, which run from left to right, are 10 mm long, 4 mm wide, and 6 mm apart. The only example of horizontally aligned finger impressions on a rim fragment measures 14 mm long and at least 3 mm wide.

Form:

Lip: The lips have been slightly flattened and rolled over the rim exterior.

Rim: straight

Neck: undetermined

Shoulders: undetermined

Dimensions: The rims are about 30 mm high and 4 mm thick. The illustrated sherd is split longitudinally (Figure 22).

UNNAMED PLAIN VARIETY

Sample: 18 rim sherds

Discussion: The undecorated sherds have the same attributes that have been described for the Walnut Decorated Lip pottery type. The sherds are most likely from the body of a vessel below the decorated shoulder portion.

WALNUT DECORATED LIP, SIMPLE-STAMPED VARIETY

Sample: 103 body sherds

Discussion: The simple-stamped body sherds have the same attributes that have been described for the



Figure 21. Rim sherd of Nance Flared Decorated pottery type.

pottery type Walnut Decorated Lip except for the surface finish (Figure 23).

MISCELLANEOUS POTTERY SHERDS

All miscellaneous pottery sherds are body sherds. They are grouped into three categories that differ from each other, as well as from the types that have been described.

Group One: Group One consists of three small thin body sherds. They are gray in color, and their surfaces are smooth and feel like slate to the touch.

Group Two: The body portion of a vessel was reconstructed from 71 body sherds of this group (Figure 24). The sherds are a buff color and tempered with grit and an unidentifiable non-calcareous material. The sloping shoulder of the vessel is thicker (6 mm) than the base (3 mm). One similar sherd, apparently not a part of the reconstructed vessel, is 8 mm thick. The vessel overall would have a squat globular shape. The orifice diameter is estimated to be about 70 mm. The maximum extent of the shoulders is about 100 mm.

Group Three: Two body sherds are assigned to this category. The paste has been tempered with par-

ticles of grit. A reddish paint was applied to the smooth sherd surfaces (Figure 25).

Discussion of the Glen Elder Pottery Assemblage

The pottery assemblage from the Glen Elder site has been divided into four pottery types, three unnamed varieties, and a miscellaneous category, consisting of three groups of pottery sherds that differ from each other and from the other types and varieties that are described.

The Walnut Decorated Lip pottery type is the principle pottery of the White Rock phase and the primary identifying trait of the archeological culture. The pottery is defined by particular repetitive patterns of decoration that are most similar to pottery types that define Oneota archeological cultural units. A difference between the two is that Walnut Decorated Lip pottery is grit tempered, while the Oneota pottery types are primarily shell tempered. The use of different tempering materials, e.g., grit or shell—once considered to be significant—now is thought to be less so (Henning 1998:354-355). The blurring of the importance of differing temper material is apparent in the Glen Elder assemblage



Figure 22. Sherd of unnamed pottery.

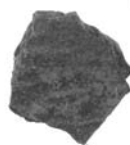


Figure 23. Simple-stamped body sherd of Walnut Decorated Lip pottery type.



Figure 24. Partially restored vessel.



Figure 25. Painted body sherd.

in that a shell-tempered variety of the pottery type has been defined.

Two other pottery types and unnamed types have been recognized in the Glen Elder assemblage. The Nance Flared Decorated (Grange 1968:50-51, 191, 235) and Fanning Plain (Wedel 1959:145-148) types represent distinct neighboring cultures with which the Glen Elder site occupants were in contact. The significance of the ceramic cross-finds will be discussed below as they relate to the introduction of simple-stamped pottery in the Glen Elder assemblage and help determine the temporal position of the Glen Elder component.

Chipped Stone Tools

PROJECTILE POINTS

Sample: 5 specimens

Description:

Plain triangular points: 3 specimens (Figure 26 F)

The lateral sides and the base of these points are straight, forming an isosceles triangle. The point

tips are missing, but reconstructed they range from 21 to 32 mm long, 13 to 16 mm across the base, and 2 to 3 mm thick. One specimen was made from a flake of an unidentified chert. The other two were shaped from flakes of Florence chert.

Corner-notched, stemmed point: 1 specimen (Figure 26 E)

The stem expands slightly, and the base of the stem is straight. Corner notches, cut into the blade, form distinct barbs. A portion of the blade is missing, but it is apparent that the lateral sides are slightly incurving. The length of the point is approximately 38 mm. The measurement across the shoulders of the blade is 17 mm. The maximum thickness is 3 mm. The chert from which the point was made is unidentified.

Large expanding stemmed point: 1 specimen (Figure 26 D)

This point, shaped from a flake of an unidentified chert, has an expanding stem and a concave base. The lateral sides of the blade are incurving, and the shoulders are slightly oblique but well defined. The point is 52 mm long, 28 mm wide across the shoulders of the blade, and 7 mm thick.

END SCRAPERS

Sample: 117 specimens

Description: The end scrapers were divided into three groups, differing only in the extent of modifi-

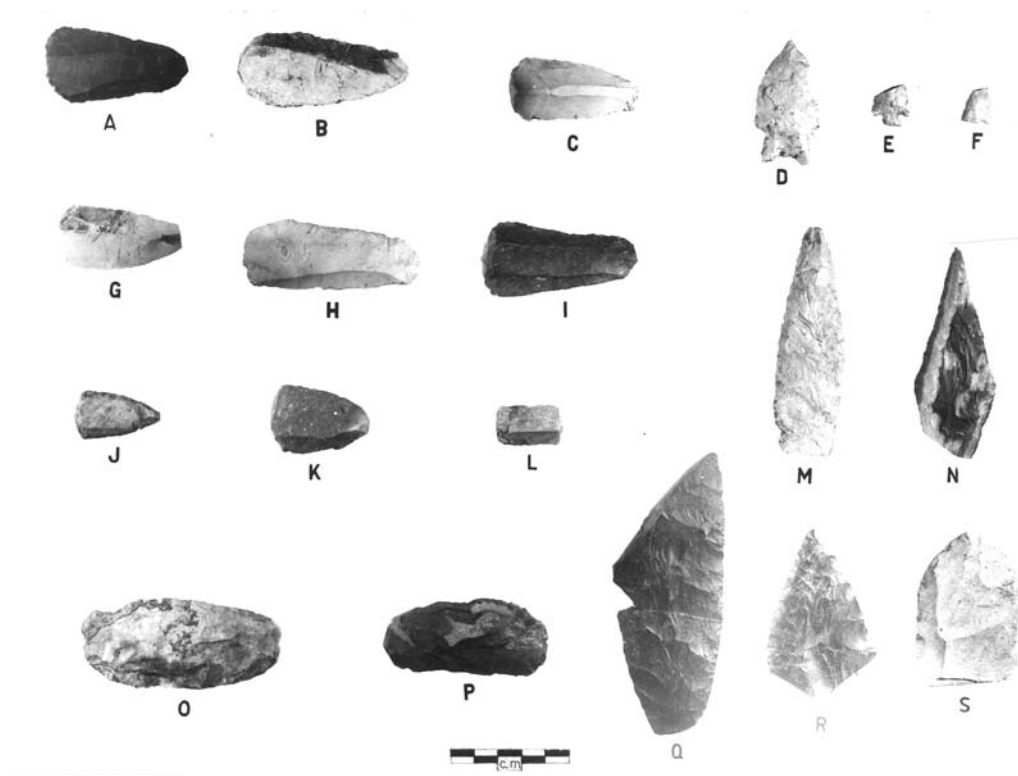


Figure 26. Chipped stone tools.

cation. End scrapers are oval flakes that have been retouched on the broad end to form a working edge. The shape of the scrapers, viewed in cross section, is plano-convex. The scrapers range in length from 28 to 84 mm; in width from 16 to 44 mm, and in thickness from 6 mm to 18 mm. A dorsal keel has been left intact on 49 scrapers (Figure 26 G and H). The dorsal keel has been removed on 57 scrapers (Figure 26 A, C, and I). Eleven specimens were totally modified into a symmetrical, oval, plano-convex shape by retouching the backs as well as the working edges (Figure 26 K and L).

END SCRAPER-GRAVER COMBINATION

Sample: 5 specimens (Figure 26 J)

Description: Five end scrapers made on oval flakes exhibit broad ends that were retouched to form working edges, and opposite, narrow ends that were shaped to points. These tools may have been shaped to be combination end scrapers and graters. The sizes of the tools range from 59 to 64 mm long, 16 to 19 mm wide, and 4 to 7 mm thick.

SIDE SCRAPERS

Sample: 12 specimens

Description: Side scrapers are made on large, rather shapeless flakes that were modified by unifacial chipping to form working edges along one lateral side of the flakes. Some scrapers received more attention in that the flake was chipped into an oblong shape (Figure 26 P). The side scrapers range in length from 65 to 130 mm, in width from 42 to 70 mm, and in thickness from 14 to 30 mm.

KNIVES

Sample: 14 specimens

Description: The collection of knives consists of blade sections. Six specimens are basal portions of oval blades, two are blade midsections, and four are blade tips. Two remaining specimens are sections of four-sided, alternately beveled knives.

The oval blades have convex lateral edges that taper to rounded or convex bases (Figure 26 Q, R, and S). An oval blade may have alternately beveled edges. Whole blades may have reached a length of

190 mm. The midsection pieces are 54 mm wide. Thicknesses average 8 mm.

Two four-sided, alternately beveled knives range from 140 to 157 mm long (Figure 26 M and N). At midsections their width is 30 mm and thickness is 6 mm.

CHOPPERS

Sample: 9 specimens (Figure 27 B)

Description: Choppers are chert cores that were formed into elliptical shapes by bifacial chipping. Sizes range from 73 to 108 mm long, 51 to 80 mm wide, and 15 to 24 mm thick.

NOTCHED AXE

Sample: 1 specimen (Figure 27 D)

Description: Two lateral notches separate the square bit-blade portion of the axe and the convex poll. The overall length is 102 mm. The blade is 70 mm wide, and the poll is 50 mm wide. The axe is 15 mm thick.

CHIPPED CELTS

Sample: 2 specimens (Figure 26 O)

Description: These implements have been bifacially chipped into roughly oval forms with working edges on the wider ends. Their sizes are 70 and 82 mm long, 32 and 38 mm wide at midsection, and 16 and 13 mm thick.

MODIFIED FLAKES

Sample: 24 specimens

Description: These nondescript chert fragments were retouched along lateral edges. They could very well have been used as scrapers or small knife blades.

UNMODIFIED FLAKES

Sample: 82 specimens

Description: These flakes have a uniform size and shape comparable to end scrapers, but they have not been retouched. They apparently are scraper blanks, cached for future use. Seventy-one are of Florence chert.

UNIFACIALLY CHIPPED CORES

Sample: 8 specimens (Figure 27 A and C)

Description: Cores are the raw material from which

flakes were removed to make chipped stone tools, or the cores themselves can be shaped into tools.

Ground Stone Tools

GROOVED MAULS

Sample: 3 specimens (Figure 28 B, E, and F)

Description: A groove circumscribes the midsection of each maul. Two specimens are made of Sioux quartzite, and one is made of granite. The mauls range from 93 to 162 mm in length, from 63 to 106 mm in width, and from 76 to 85 mm in thickness.

HAMMERSTONES

Sample: 3 specimens (Figure 28 C)

Description: These tools were made from Sioux quartzite cobbles. The surfaces exhibit striking scars. The cobbles have been somewhat modified, resulting in a disk rather than spherical shape. Their diameters vary from 71 to 105 mm; widths, from 62 to 96 mm; and thicknesses, from 39 to 68 mm.

GRINDING STONE

Sample: 1 specimen (Figure 28 D)

Description: One stone is partially flattened on one side and bears no striking scars on its surface. This single specimen, originally described as a gaming stone, resembles a hammerstone (Marshall 1969:58). It is 61 mm in diameter and 43 mm thick.

POLISHED CELT

Sample: 1 specimen (Figure 28 A)

Description: The polished celt is a diorite stone, shaped into an oval using a pecking and grinding technique. The celt is 139 mm long, 47 mm wide at the midsection, and 33 mm thick.

MILLING STONES

Sample: 3 specimens

Description: Large pieces of limestone and granite were used as milling stones. The stones were not modified except that a surface is quite smooth and worn to the extent that it exhibits a shallow concavity. The stones range in size from 190 to 210 mm in length, from 165 to 185 mm in width, and from 60 to 90 mm in thickness.

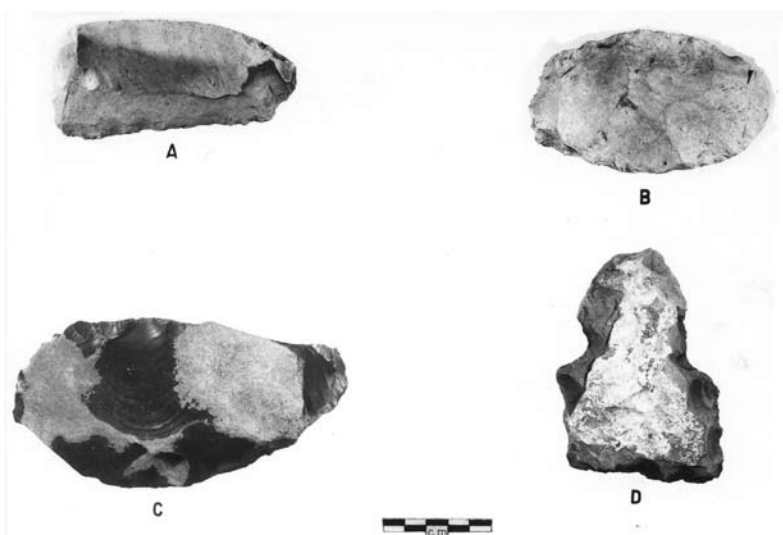


Figure 27. Modified core tools and axe.

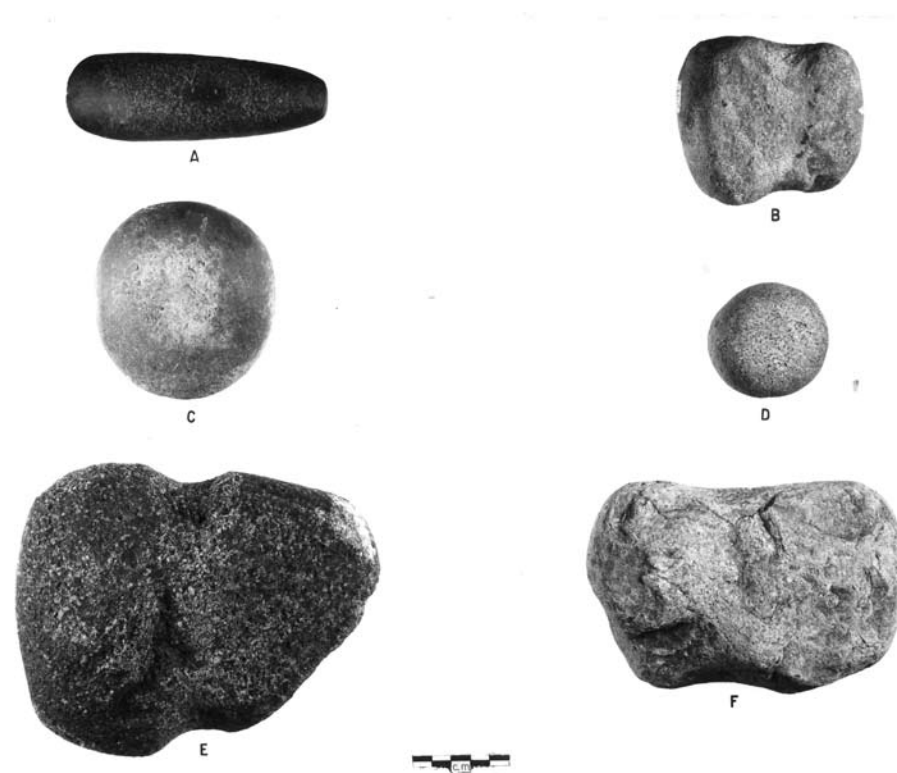


Figure 28. Ground stone tools.

ABRADERS

Sample: 4 specimens

Description: Iron-cemented sandstone was used for abrading tools. Three specimens are sections of narrow elongated tools that were used as arrow shaft smoothers and as sharpening stones for

small pointed implements, such as awls (Figure 29 A, B, and C). Each piece has a longitudinal groove worn into its surface that measures as narrow as 3 mm in diameter and as wide as 8 mm in diameter. One other specimen is a large paddle-shaped slab of sandstone that has three grooves worn into its

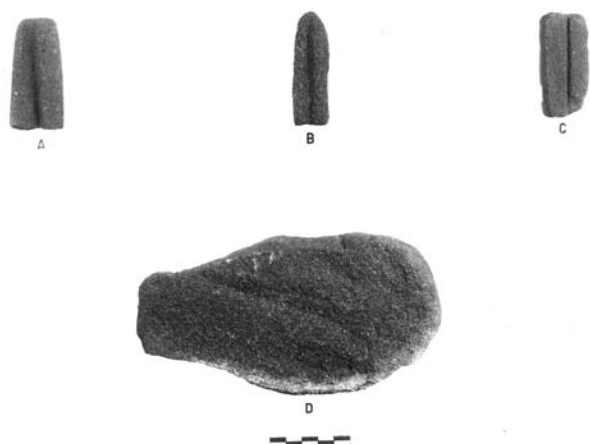


Figure 29. Abrading tools.

surface (Figure 29 D). The implement is 188 mm long, 93 mm wide, and 25 mm thick.

Miscellaneous Stone Fragments

Sample: 1021 specimens

Description: These fragments are waste material that was scattered over the Glen Elder site. The collection consists of 450 pieces of unidentified cherts, 350 pieces of Florence chert, 220 pieces of limestone, and 1 piece of Sioux quartzite.

Bone Implements

HOES

Sample: 2 specimens (Figure 10)

Description: Two bison scapulae hoes were found in a basin-shaped pit. They lay intact but shattered and could not be removed as whole specimens. The posterior and anterior angles were eliminated, creating a rounded edge. The anterior border and the acromion process were modified by removal of the two spines. The glenoid cavity was partially modified by removal of the left edge surface, exposing cancellous tissue. Both hoes are about 111 mm long.

AWL

Sample: 1 specimen

Description: A small section of bone is surmised to be the base of an awl. Wedel (1959:252) suggested that these particular tools were shaped from

the edge of a large rib or the posterior and neural spine of a bison.

Unworked Bone

The bone collection represents bison, deer, possibly dog, and human remains. There are nine distal ends of bison scapulae, one distal end of an ulna, metacarpal fragments, and a third phalanx or a hoof bone. Two horn cores, teeth, and carpals complete the inventory of bison bones.

The distal end of an ulna, section of a humerus, and section of an antler are deer. One humerus is possibly dog. Two bones, a radius and a fibula, were identified as human (Edward Fry, personal communication 1964).

Worked Shell and Bone

BONE AND SHELL CONTAINER

Sample: 1 specimen (Figure 30)

Description: This unusual object consists of an unmodified half of a freshwater mussel shell and a piece of scapula cut to fit the outline of the shell. The two pieces were found joined together to form a small container, which was filled with compacted soil. The container is 88 mm in length, 44 mm in width, and 21 mm thick.

PERFORATED DISK

Sample: 1 specimen (Figure 31)

Description: Half of a perforated disk may be a shaped body sherd of Walnut Decorated Lip pottery. The disk is 20 mm long and 4 mm thick. The perforation is 4 mm in diameter.

Materials of Modern Manufacture

Materials of modern manufacture were randomly scattered throughout the disturbed portions of the Glen Elder site, and none can be associated with the aboriginal occupation. Fragments of bottles, a section of a stoneware jug, a square nail, a screw, and unidentifiable pieces of metal compose the inventory for this category. The bottle fragments are all press glass, placing them at least within the nineteenth century.



Figure 30. Bone and shell container, (A) obverse, (B) reverse.

Discussion of the Non-Ceramic Artifacts from the Glen Elder Site

The description of non-ceramic artifacts that were recovered from the Glen Elder site repeats that section of the 1969 report (Marshall 1969:53-61). The collection has been reexamined, and a more thorough and detailed analysis has been published (Blakeslee, Peck, and Dorsey 2001:86-100). The authors of the recent analysis attempted to include all of the specimens that have ever been gathered from the site. They noted that some items of the collection, having been used as a subject of study over the years, have been misplaced or lost (Donald J. Blakeslee, personal communication 1995).

INTERPRETATIONS

Purpose of the Glen Elder Site and Related Components

The first-defined White Rock aspect archeological culture was interpreted to represent a closely related people living in semi-permanent villages who left their villages to establish bison-hunting camps. The semi-permanent villages represented

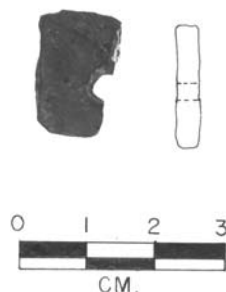


Figure 31. Perforated disk.

the Glen Elder focus, including the Glen Elder and White Rock sites. The hunting camps represented the Blue Stone focus, composed of the Blue Stone and Green Plum sites (Rusco 1960:72-75).

The closely related people were attributed to be carriers of a "western-like Oneota" manifestation (Marshall 1969:78-91). The semi-permanent village and hunting camp division was not a cultural variation; therefore, the two-foci construct was refuted, and changing the White Rock aspect to the Glen Elder focus was the appropriate classificatory designation (Marshall 1969:66-71). The semi-permanent village and hunting camp division was strengthened by comparison of the geographical localities of the two foci (Marshall 1969:71-72).

The Kansas sites, located in Mitchell and Jewell counties, are situated above the Solomon River and White Rock Creek on ridges that rises about 1,500 feet above mean sea level (AMSL) and higher. Site elevations are 1,493 feet AMSL for the Glen Elder site, 1,590 feet AMSL for the White Rock site, 1,620 feet AMSL for the Warne site, and 1,582 feet AMSL for the Intermill site. These four sites, all at least 90 feet higher than the floodplain, are certainly protected from flooding. The Blue Stone and Green Plum sites, located in Harlan County, Nebraska, which is 80 miles west by north of the Kansas sites, are on the floodplain of Prairie Dog Creek.

One cannot infer that this geographical grouping is accidental when the artifact assemblage is considered. The absence of evidence for horticulture and an abundance of bison bone at the Nebraska sites indicate that these were hunting camps. The presence of bison scapula hoes at the Kansas sites and corn specimens from the White Rock and Intermill

sites, in particular (Neuman 1963:269, 274)—at locales where floodplain horticulture would be inconvenient—implies that the occupants considered defensive advantages. On the Glen Elder site, for example, one has a commanding view of at least 6 miles to the east, west, and south (Figure 2).

An alternative explanation is that occupants of the Nebraska sites were abandoning their Kansas villages and moving to establish a new village. Such movement might have been seasonal when most villagers participated in the hunt, much like the practice of later historic tribes in the area (Rusco 1960:74). However, the sites' geographical distribution and locations favor the first conclusion: two kinds of habitation are represented, and the different activities that occurred at the sites resulted in the diversity of implements that have been recovered as archeological specimens.

The more thorough and detailed analysis of the Glen Elder artifact assemblage has led researchers to conclude that the Oneota-derived occupants of the Glen Elder site were indeed establishing a pattern of hunting bison that lasted into the Historic period. Scouting parties set out from the eastern Oneota villages to establish base camps, one of which is represented by the Glen Elder site (Blakeslee et al. 2001:100-102). They planted crops in preparing to receive the villagers during the autumn harvest season. Local cherts and other raw stone materials for fashioning tools were collected during the journey to the base camp. The bison hunt commenced, and the spoils of the hunt were carried from the kill sites to the base camp to be prepared for transportation back to the eastern villages.

Mounds as They Relate to the Glen Elder Site

The settings of the Glen Elder and White Rock sites at higher elevations coincide with the location where mounds are often found. These mounds are usually low, rock-filled structures that may represent the mortuary practices of some variant of a Woodland horizon (Wedel 1959:526). The excavation of the mound designated as 14ML301 offered little evidence to pursue an archeological cultural identification, nor was there any indication of the purpose for the structure. But the mounds are there. Woodland variants' burial practices might have involved

mounds farther west in the Central Plains than they are now are thought to be (Bozell 2006:102). The mounds also might mark the western periphery of this particular mortuary practice.

Two projectile points that are described in this study, the large expanding-stemmed point and a small corner-notched stemmed point, are similar to projectile points found in Woodland artifact assemblages (Figure 26 D and E). The grooved granite maul and perhaps the polished diorite celt may also be tools of an earlier Woodland occupation at 14ML1 (Figure 28 A and E).

The mound remnant that was excavated in 1963 is interpreted as an earthen mound that was plated with limestone rocks. Sherds of the Walnut Decorated Lip pottery type were recovered from an undisturbed portion of the earthen fill. Such evidence forces the conclusion that Oneota villagers built the structure. Underlying pits were devoid of cultural material, so the mound's purpose is unknown. Future excavations on White Rock phase components may not confirm or may even contradict the conclusion reached here, but at the very least the occupants tampered with the mound.

Temporal Position of the Glen Elder Component

Walnut Decorated Lip pottery was found at the Burkett site, a large (50-80 acres) village located on the Loup River in Nance County, Nebraska (Grange 1968:29, 66, 116, 229). The Pawnee Indians resided there early in the history of their settlements in Nebraska. Furthermore, a pottery type associated with 25NC1, recognized in this study as Nance Flared Decorated, was found on the Glen Elder site (Figure 21).

Roger T. Grange employed seriation methods to construct a temporal sequence of 15 major pottery types and variations of those types that were of Pawnee manufacture. The purpose of Grange's study was to connect the known historic Pawnee villages, ranging in time from about A.D. 1700 to 1876, to a Protohistoric period archeological culture that was designated as the Lower Loup focus (Grange 1968:130). The seriation study placed the Burkett site as the earliest in the sequence with a historical date of A.D. 1541 and an archeological date of A.D. 1500-1614, based on the suggested

dates for the White Rock aspect, a ^{14}C date of A.D. 1630, and an estimated date of A.D. 1500 (Grange 1968:130). After the study was completed, a charcoal sample that had been recovered from a storage pit at 25NC1 was submitted for radiocarbon assay to test the conclusion based upon the pottery seriation. The radiocarbon-derived date was 320 ± 100 B.P. or A.D. 1630 (Grange 1968:129).

Several foreign pottery types occurred in small numbers in the Pawnee pottery assemblage. Walnut Decorated Lip was one, found only at the Burkett site with a few Oneota-derived shell-tempered pottery sherds (Grange 1968:65, 115, 226-229). The cross-finds of Walnut Decorated Lip and Nance Flared Decorated pottery types between 14ML1 and 25NC1 are positive evidence that the Glen Elder site was occupied when the Pawnee resided at the Burkett village.

The appearance of simple stamping as a surface finish on Walnut Decorated Lip pottery seems to be a result of contact between the Glen Elder site occupants and the Pawnee Indians. Simple stamping is the most common surface finish on Pawnee pottery. The stamping consists of parallel straight grooves and ridges (Grange 1968:45). The Glen Elder rim sherd of an unnamed pottery type, illustrated as Figure 22, could belong to the Pawnee pottery assemblage. The rolled over lip resembles Nance Flanged Lip (Grange 1968:53, 194-195, 234-235), but simple stamping is not listed as a surface treatment for this pottery type.

The 18 plain rim sherds, discussed in the pottery section, are also an unnamed variety, although they do resemble Walnut Decorated Lip pottery. There is also a resemblance of this plain variety to the Pawnee pottery types named Nance Flared Plain, the most common type in the Pawnee assemblage, and Nance Straight Rim Plain (Grange 1968:47-49, 51-52).

The partially restored vessel that has been identified as the Fanning Plain pottery relates the Glen Elder site to the Fanning site in Doniphan County. The decorative motifs that embellish Glen Elder pottery handles are duplicated in the Fanning pottery assemblage. The only difference is that a Fanning handle is attached to the neck portion of the vessel just below the rim (Wedel 1959:146), while the upper end of a Glen Elder handle is attached to the vessel lip, as well as to the neck at a point just below the rim.

Wedel (1959:145-150) divided the Fanning pottery assemblage into two varieties, Fanning Plain and Fanning Decorated, with Fanning Plain being in the majority. The significance of this distinction has yet to be determined. Also recovered from the Fanning site was pottery of unspecified Pawnee types, but these rim sherds have not yet been related to the Pawnee pottery sequence. The pottery resembles Colfaxed Braced, which does extend into the Historic period of the Pawnee sequence (Grange 1968:58-61, 208-213; Wedel 1959:150-153).

In the Glen Elder pottery collection, Blakeslee et al. (2001:82) reported finding a rim sherd from a Jicarilla micaceous ware vessel that dates to roughly A.D. 1720-1750. The researchers also reported finding Solomon River phase pottery on the Glen Elder site. They do not elaborate except to note that 14ML1 is not badly contaminated with material from other cultural traditions.

Pottery specimens that are embellished with a red paint have been recovered from the Glen Elder and White Rock sites. One specimen is the pottery sherd described as Group Three under the miscellaneous category (Figure 25). The specimen recovered from the White Rock site is a partially restored vessel, consisting of portions of the shoulder and body. The shoulder portion is decorated with trailed lines and beneath this area of decoration is a chevron design of red paint (Logan 1995:61-62). The significance of painted surfaces on the pottery vessels cannot be explained by data that is now available.

A few seemingly trivial trade items of European manufacture, recovered from storage pits at the Fanning site, led Wedel (1959:170-171) to conclude that the village was occupied during the last years of the seventeenth century at the latest. In March 1963 William M. Bass and Jack M. Schock led a one-day salvage expedition at the Fanning site. The recovered items of European origin demonstrated that the trade was something more than trivial. The most significant specimen, a French-made, blond colored, plano-convex gunflint, is an early type that first appeared about A.D. 1675 (Shock and Bass 1966:207-215). Blue glass beads are of a type found in French sites around A.D. 1650 (Shock and Bass 1966:208-209). These items of French trade support Wedel's conclusion that the Fanning village had been abandoned perhaps as early as A.D. 1700.

Temporal Position of the White Rock Phase

The temporal position of the White Rock phase is defined by radiocarbon determinations and the presence of Walnut Decorated Lip pottery at the Burkett site. The span of time ranges from the fourteenth century to the beginning of the sixteenth century, which relegates the culture to the late prehistoric rather than the Protohistoric period (Logan 1996:135-136).

The supporting radiocarbon dates were determined from charcoal samples recovered from two disturbed pits, Features 4 and 8, at the White Rock site and from an undisturbed pit, Feature 1, at 14JW24, which is a site within the Warne locality. The 14JW1 pits were disturbed by a drainage ditch that runs alongside a roadway. The samples from Feature 4 were recovered in 1993, and those from Feature 8 were recovered in 1994-1995 (Logan 1995:14, 29-36). The 14JW24 sample was recovered in 1993 from a pit containing cultural debris (Logan 1995:13).

Two carbon samples from White Rock site Feature 4 yielded dates of 720 ± 70 B.P. (Beta-65893) and 510 ± 40 B.P. (Tx-7984). Two other samples from Feature 8 dated 720 ± 50 B.P. (Tx-8193) and 390 ± 60 B.P. (Beta-73924). The charcoal sample from 14JW24 Feature 1 dated 660 ± 80 B.P. (Beta-53612) (Logan 1966:136). The results from two different laboratories on two samples from each disturbed feature reverse one another with a late and early date. This may be a cause to present these dates with a note of caution, but an explanation has been offered that this reversal is acceptable (Logan 1995:99-101).

The first temporal span for the White Rock phase, based upon the radiocarbon determinations, was given as A.D. 1300 to 1450. Walnut Decorated Lip pottery from the Burkett site was recognized soon after, and the time span for the White Rock phase was extended another 50 years (Logan 1996:136). Walnut Decorated Lip pottery was reported as a foreign pottery type at the Booth site (14CM406) in Comanche County. The site represents a late component of a possible multi-component manifestation that was classified as a unit of the Wilmore complex. Two calibrated radiocarbon determinations produced dates of A.D. 1377-1661 and A.D. 1407-1638 (Bevitt 1999:36-60).

This review of the archeological investigation of the Glen Elder site and the comparison of interpretations of this manifestation with those presented by previous researchers offers evidence that the time span for the White Rock phase does encompass the Protohistoric period in the Central Plains. The evidence is not so tenuous as to be outrightly dismissed.

SUMMARY

The archeological material reviewed in this study represents an extension of the Oneota tradition into the Central Plains late in the prehistoric period, existing in time as a cultural unit of the Oneota Classic horizon (A.D. 1350-1650) (Henning 1998:353). Further discussion concerning the appropriateness of the classificatory designations is set aside with the suggestion that the cultural unit be perceived as an example of group continuity in which traditional activity was carried out within a region or locality by a band or tribe over a specific period of time (Henning 1998:353).

A span of time that has been determined for the archeological unit by radiocarbon-derived dates and pottery cross finds corresponds with the Oneota Classic horizon. The sites were occupied throughout the Protohistoric period. The occupation may have preceded the sixteenth-century Pawnee villages and may bridge the Protohistoric-Historic transition or the time of European contact with identified native cultures. The archeological cultural unit then becomes part of the ethnohistorical account of the identified native culture, so distracting classificatory interpretations become irrelevant.

The traditional activity of this western Oneota manifestation is primarily bison hunting. The components of the archeological unit appear to be widely distributed semi-permanent and permanent hunting camps that are peripheral to a village. The Glen Elder site is interpreted as a large base camp that may have been occupied by most villagers annually or biannually for periods of several months during which gardening was practiced as well as hunting. More months of the year may have been spent at these base camps than in the village. The evidence for the practice of mound building and evidence that a defensive posture was an important consideration in selecting a location indicate that the

Glen Elder site was a well-established permanent base.

ADDENDUM: KANSA INDIAN AFFILIATION

The Fanning site most probably was a Kansa Indian village (Wedel 1959:171). A comparative review of the pottery assemblages from 14ML1 and 14DP1 indicates an affiliation that bridges the Fanning site and the Protohistoric western-like Oneota manifestation. The identification of the western-like Oneota as being the antecedent to the Kansa Indians has already been stated (O'Brien 1984:67-69). The identification remains to be confirmed.

The Kansa Indians first appear in the historic record as the report of the 1673 Marquette and Jolliet expedition. By 1718 the Kansa are clearly recognized as occupants on the Kansas River or, as the river is labeled on the Guillaume Delise map of that year, "Grande Riviere des Canez" (Wedel 1959:26-28). The Kansa occupied villages along the Kansas River until their removal to the Neosho River in 1847. The historical, ethnohistorical, and archeological records concerning the Kansa have been reviewed and presented as an incomplete culture history (Marshall 2006:219-232).

The earliest, most illuminating record is Etienne de Véniard Bourgmont's eyewitness account of the Kansa as they were in 1724. Delise had access to Bourgmont's firsthand knowledge of the Missouri River from prior years when Bourgmont was living the life of a *cour de bois* with the Missouri Indians. This information was included in the map of 1718. Bourgmont's account reveals that the Kansa had already integrated the horse and gun into their culture. They were hunters and raiders who closely allied themselves with the French and were dependent on the French trade. The Kansa ranged the length of the Kansas River, and the western Podouca were a traditional enemy as early as 1724 (Marshall 2006:222; Wedel 1959:77-78). After a review of the historical record, Wedel (1959:28-34, 100-101) concluded that Bourgmont met the Kansa in a village now identified as the Doniphan site (14DP2).

A search to identify early Kansa village sites was one goal of Wedel's introductory fieldwork in Kansas during the summers of 1937, 1939, and 1940 (Wedel 1959:2). Excavations were conducted at the

Fanning and Doniphan sites, as well as at the Blue Earth Village (14PO24), which is a historically confirmed Kansa village occupied from at least 1800 to 1830 (Wedel 1959:29-30, 50-54, 98-171, 187-197). The intent was to find pottery or other artifacts of native origin at 14PO24 that would show a relationship with artifacts from 14DP1 and 14DP2, thus establishing a Kansa sequence and confirming Fanning and Doniphan as Kansa villages. The final result was inconclusive; nothing was found at the Blue Earth village that could be related to Doniphan or Fanning. Trade items had replaced objects of native manufacture, including pottery, during the 70 or so years between the Doniphan and Blue Earth village occupations (Wedel 1959:197). The interpretation of the historic record, which concludes that the Kansa village visited by Bourgmont is the Doniphan site, may be correct, but the actual village has yet to be precisely located.

The limited excavation at the Doniphan site uncovered a multi-component occupation. One component was a prehistoric manifestation that is identified by the classificatory taxon, the Nebraska phase (Steinacher and Carlson 1998:252-255; Wedel 1959:127-131). Radiocarbon-determined dates suggest that the Nebraska phase ranged in time from A.D. 950 to 1425 (Steinacher and Carlson 1998:252). Pottery sherds, already mentioned as being recovered from an undisturbed portion of a mound at the Intermill site, are thought to be Nebraska phase pottery (Newman 1963:296-297).

A historic component at the Doniphan site is represented by burials with associated European trade items and a few pottery sherds comparable to Fanning and Pawnee pottery types (Wedel 1959:120). The Pawnee pottery seems to be Wright Folded Lip (Grange 1968:57-59, 206-207), which is a minor type in the Pawnee sequence during the years A.D. 1650-1750 (Grange 1968:135).

In conclusion, the western-like Oneota cultural unit has been excavated more thoroughly than the sites that relate more closely to the cultural history of the Kansa Indians; however, a thread of evidence does indicate that the two Oneota manifestations merged to become the historic Kansa.

An archeological project having the objective to confirm a Kansa sequence would include the Doniphan and Fanning sites and perhaps the King Hill site located within present-day St. Joseph,

Missouri (Henning 1998:391-393). The Doniphan site is not an identified village in the Kansa ethnohistorical account, but there are other identified Kansa villages waiting to be located and confirmed by archeological excavation (Marshall 2006:219-232; Swanson 1952:292). A confirmed antecedent would only begin to fulfill the incomplete cultural history of the Kansa.

The occurrence of the Oneota migration to the Central Plains is a major field of research for archeologists (Ritterbush 2006:151-164). The Glen Elder component identifies one cultural unit of the migration. Each archeological study has had the goal of contributing to a typological refinement and has met with varying degrees of success. Still, the beginning and the end of the migration remain to be resolved.

ACKNOWLEDGMENTS

I return to Glen Elder with Preston Holder in my memory. He has been described, and was, a colorful iconoclast who was exciting to be with. We were looking at some pottery that we had recovered early in the excavation of the Glen Elder site, and I remember Holder asking, "Do you think a bunch of Quawpas wandered out here and got themselves lost?" Holder was a taskmaster and a marvelous teacher. I always have realized that I was privileged to have been one of his students.

This revised version of the archeology that was done at the Glen Elder site will, I hope, generate controversy, criticism, and interest in pursuing the cultural history of the Kansa Indians. Virginia Wulfkuhle introduced me to this area of research when she invited me to write a report on the archeology that was done at Hard Chief village. Now she has the task of editing the papers that I submit. I am thankful for and am very much in need of her editorial skills.

All of the Kansas Historical Society's staff archeologists are supportive of my research interest and for this I can only say thank you and strive to meet the high expectations that each of you demand of yourselves.

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Book Reviews

Archaeology for Dummies. NANCY MARIE WHITE. Wiley Publishing Inc., Indianapolis, 2008. 368 pages. \$21.99, paper. ISBN 978-0-470-33732-5. Reviewed by Donna C. Roper, Kansas State University.

As of May 2009, when this review was written, the online store (dummies.com) of the *For Dummies* book series listed 1,590 titles. Some of these are multiple editions of the same title or subject, but overall the series covers a formidable range of subjects from *Einstein for Dummies* to *Microsoft Office for Dummies* to *Bathroom Remodeling for Dummies*. Given this breadth of subject matter, an *Archaeology for Dummies* perhaps was inevitable. This is not necessarily a bad thing. Bookstores nationwide (even at some universities, although not as textbooks) stock Dummies titles. With their distinctive black and bright gold covers and simple titles that fit on the spines in large letters, these books stand out on a shelf and draw the browser's eye. Because of this brand-name recognition, *Archaeology for Dummies* should reach a wide audience, and a wider one than a book that is not part of a widely known and prominently displayed series might reach. We should all be rooting for archeology being made widely available in a user-friendly format like this. Before leading the cheers and automatically pronouncing *Archaeology for Dummies* a good thing, however, we must first find the contents to be consistent with what might be presented in a university classroom and with what working archeologists routinely practice. It also must promote stewardship of the archeological record, even as it promotes appreciation of that record. Against these criteria, *Archaeology for Dummies* gets mostly, but not entirely, high marks.

The book contains 21 chapters organized into six parts. Part I reviews what archeology is and isn't (it isn't dinosaurs, it isn't treasure hunting, and archaeologists aren't always digging—all good points to make in a book for popular consumption), the variety in what archeologists do, and an overview of how archeologists work (in essence, the scientific method as it is practiced in archeology).

Part II describes archeological fieldwork, providing advice on needed supplies and equipment, describing archeological survey, telling the reader how to conduct an excavation, and reviewing the legal, ethical, and safety concerns associated with field archeology. Part III continues the discussion of the investigation process by describing laboratory procedures, reviewing how to study artifacts, and how to report an excavation. Part IV is a four-chapter, not quite 100-page synopsis of two million years of human prehistory. Part V, "Archaeology Is for Everyone," reviews the uses of archeology, how to become involved in archeology, and some "Controversies and Sensational Findings," such as when the first people came to the Western Hemisphere or the discovery of Ötzi, the Neolithic Ice Man from Europe. These are legitimate controversies and valid finds, not the misguided preposterous claims and made-up controversies that can mislead the public about the human past. Finally, Part VI lists ten archeological sites to visit in the United States (with a few bonus extras—after all, there are so many worth visiting), ten archeological sites to visit outside the United States (also with some extras), and ten fun ways to experience archeology. *Archaeology for Dummies* thus is a comprehensive review of the principles and practice of archeology, as well as of the results of doing archeology across the entire span of human existence worldwide. The book's scope is at least as broad as that of many university Introduction to Archaeology courses, and its organization is overall reasonably similar to that of a university course. The major difference is the much less formal and usually less detailed presentation style.

The book's author, Nancy Marie White, is a professor at the University of South Florida. Her writing here reflects a firm grasp of the subject, not only in an academic sense but also in the practical sense of having worked at archeology in a variety of circumstances and places. One guesses that she also may have done considerable travel to archeological sites and museums throughout the United States and the world and acquired her broad knowledge of world prehistory by experiencing and not just reading about important places. Knowing the

subject, even in this way, however, does not necessarily mean one can successfully write a book such as this one. What also is required is the ability to write about the subject in a language that is clear and understandable by a wide audience and to do so without talking down to the reader. In this regard, the tone of *Archaeology for Dummies* is just right. A few too many exclamation points and overuse of the word “fancy” get a bit wearing, but there are worse sins, and they are avoided. The text is light-hearted and humorous, nicely walking the line between the formal style that would be used in a college textbook and the nauseating cutseyneess that some writers seem to think they have to use in works for a popular audience. Overall, White has done a very good job, and *Archaeology for Dummies* is a surprisingly good book. Well, maybe it should not be a surprise since *For Dummies* books are touted as how-to books and the series would not thrive if it did not give accurate advice on how to remodel your bathroom, knit, play golf, use a BlackBerry, run Microsoft Office, or any of hundreds of other things, presented by professionals who know their subjects.

The sticking point in *Archaeology for Dummies* concerns Part II “Archaeological Fieldwork: The Adventure Begins!” Remember that *For Dummies* is a how-to series. True to this, the front cover of *Archaeology for Dummies* proclaims “Learn to: . . .” and lists four bullet point items, the second of which is “Excavate a site, analyze findings, and draw conclusions.” The four chapters in it are accurately written and capture the essence of fieldwork. The question, of course, is whether they should be there, or at least whether they should be written as they are. The text directly addresses the reader—“you”—telling *you* what equipment *you* need, what *you* do on a survey, how *you* excavate a site. But, remarkably, it does not tell *you* how projects are organized: with a credentialed principal investigator in charge and, depending on the size of the project, trained field supervisors or crew chiefs, in addition to the crew members that presumably *you* are if you have no prior training or experience and are reading this book to learn about fieldwork. It also does not provide any illustrations to show how a survey is conducted, how sites are recorded, how maps are drawn, how a site is laid out for excavation, how profiles are drawn, what is recorded on excavation

level forms, and so forth. The reader who knows something about archeological fieldwork will know that, however accurate this fieldwork section is (and it is highly accurate), it is not complete; the reader who does not know how fieldwork is conducted has no way to realize this. Thus, intentionally or (presumably) not, it gives the appearance of instructing readers in how to conduct their own field investigations without explicitly discouraging them from undertaking an excavation on their own and without training and supervision. One resolution would have been to write in the third person. For example, *Anthropology for Dummies* also briefly discusses archeological fieldwork, but tells its readers how “the archaeologist” does his or her work. *Archaeology for Dummies* later discusses educational requirements for being an archeologist and opportunities for participating in archeology, obviously implying the need for training and experience. Maybe that discussion should have come *before* the section on fieldwork rather than 170 pages *later*. This might better imply what *you* would be doing when *you* joined a project, making the instructional tone of the fieldwork section less worrisome to this reviewer.

A colleague recently passed along the link to a newspaper article about the arrest of a looter on public lands, reporting that he was caught with “dinosaur bones and *other artifacts*” (emphasis added). Educating the public, apparently including some journalists, about archeology (and paleontology) remains an uphill climb. This is why it is important to have well-written books for a broad audience. *For Dummies* books attract readers, and in principle including this book in this series is exciting. It also can be problematic, though. Telling what archeologists do—and don’t do—is informative, and recounting world prehistory is valuable for understanding who we are as humans and how we got where we are. *Archaeology for Dummies* is in many ways a very good book, and someone interested in archeology can learn a lot from it. It certainly would go too far to say that the fieldwork part of *Archaeology for Dummies* does not promote stewardship of the archeological record, for it certainly does. Further, it is not likely that legions of shovel-wielding folks will take to the field, armed with this book in their field packs to tell them how to do it. Probably anyone who really intends to

plunder sites will anyway. However, a good explicit discussion—and it need not have been long—of when to know your limits in actually doing archeological fieldwork would have been welcome. Even

better, more urging of interested readers to join organizations such as the KAA and its counterparts in other states and to participate in archeology this way would have many benefits for all concerned.

Texas Civil War Artifacts: A Photographic Guide to the Physical Culture of Texas Civil War Soldiers. RICHARD MATHER AHLSTROM. University of North Texas Press, Denton, Texas, 2008. x + 546 pp., 333 figures, appendix, bibliography, index. \$60.00 ISBN 978-1-57441-251-2. *Reviewed by* Jim D. Feagins, St. Joseph Museum.

Units of Confederate soldiers from Texas camped and fought in well over 40 percent of the states then in existence, including Kansas. This volume contains abundant photographs with good descriptions of the material culture representing Texas soldier ingenuity. They fought long, grinding years with very limited resources. *Texas Civil War Artifacts: A Photographic Guide to the Physical Culture of Texas Civil War Soldiers* emphasizes many artifacts not shown in any other reference and gives examples of many previously identified. For each artifact illustrated (and there are hundreds), Ahlstrom lists other known sources where they are also described. If for that reason alone, the volume is very handy. It proves to be an excellent reference for archeologists and all others interested in military material culture. However, there is much more that makes the book interesting and useful.

All through the Civil War, the South faced severe shortages of materials to wage the war effort. The southern states had to adapt: recycling, making do, often using a hodgepodge of equipment, some badly outdated and some homemade. This was especially true of Texas. Many of the troops had to furnish much or almost all of their own equipment. Confederate government-furnished materials were commonly a mismatch, as it was impossible to completely outfit troops with regulation uniforms and/or equipment. Once worn out, the “uniforms” of Texas units were difficult to replace. A soldier in the 6th Texas Infantry humorously wrote: “In this Army one hole in the seat of the breeches indicates

a Captain, two holes a Lieutenant, and the seat of the pants all out indicates that the individual is a Private” (p. 5).

A few supplies and equipment were bought at greatly inflated prices from foreign suppliers in spite of the Union costal blockade. Also some equipment was obtained from the Texas Rangers and the state militias or left over from the Mexican-American War (1846–1848) or from start-up manufacturers within the state. There were pronounced differences in uniforms and equipment even within a single regiment. With this tremendous variety, it should be no surprise that there are hundreds and hundreds of artifacts (many of which are illustrated) that are unique to Texas.

The 333 figures (photographs) in this slick paper volume are generally sharp and clear and large enough to easily identify the attributes of most artifacts. The captions that accompany them are well done—usually giving provenance, type (name/description), dimensions, construction, remarks, and, as appropriate, cross-references where that artifact or closely related artifacts can be found. The dimensions are given in English units rather than metric units. It is unfortunate that both measurement systems are not listed, as converting back and forth when making comparisons with other sources is awkward. Also the thicknesses of illustrated specimens are not given. This information would be especially useful with some categories of artifacts.

Ahlstrom warns modern collectors and museum personnel about reproductions of Civil War/Texas artifacts. Also, he is careful to point out that it was not just Texas that used the lone star as a symbol during the war. The “Lone Star State’s” five-pointed symbol originated when Texas was a republic. In addition, “The states of Maine, Mississippi and Florida, for example, made wide use of the star. And the 12th and 20th Corps of the Union Army used the star as their symbol” (p. 6).

Among the types of artifacts illustrated are lone star hat (and other) pins, belt plates and buckles,

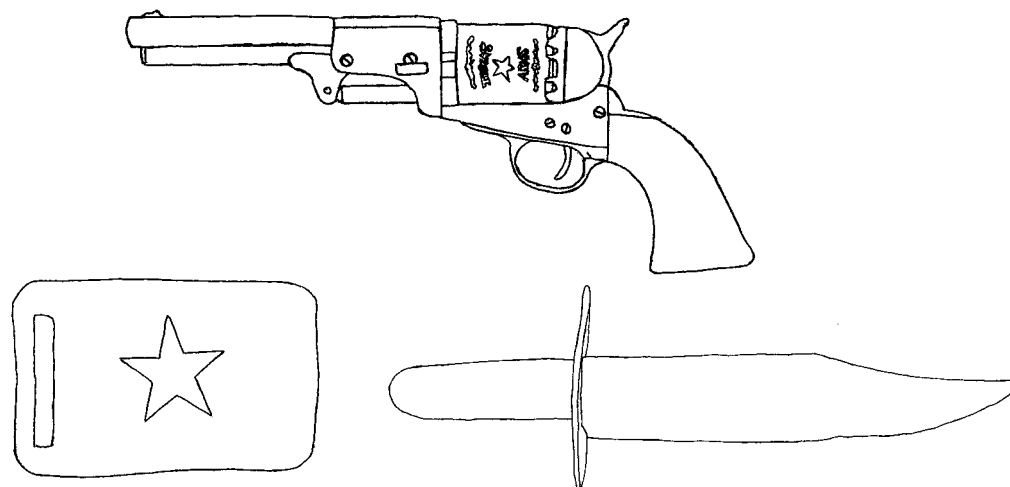


Figure 1. Top: Texas-made 44 cal. six-shot Dragoon model pistol made by Clark, Sherrard and Co. in Lancaster, Texas. Bottom Left: Brass Buckle—Texas made. Bottom Right: Bowie knife once owned by Sergeant John Sterns 12th Texas Artillery Battery—Civil War

horse furniture (saddle, harness, bits, martingales, equipment decoration, rosettes, spurs, and stirrups), Texas-made pistols and manufacturing equipment, other types of pistols used by Texans, shotguns, rifles, muskets, swords, knives, buttons (including some Republic of Texas buttons since they were also reused during the Civil War), leather accouterments (belts, holsters, percussion cap pouches, cartridge boxes, a map or order case, shoes with wooden soles), heel plates, canteens, a variety of uniforms and headgear. The button and pistol sections are especially good. There is also a short section labeled “Texas Soldiers in Pictures” that demonstrates some of this equipment in use.

The appendix is titled “Battles and Campaigns in Which Texas Units Fought.” The states are listed alphabetical and the battles or campaigns are arranged chronologically; the dates and Texas units are then given. The states listed are Alabama, Arkansas, Georgia, Kentucky, Louisiana, Maryland, Mississippi, Missouri, New Mexico, North Carolina, Oklahoma (Indian Territory), Pennsylvania, Tennessee, Texas, and Virginia. An impressive list, although Ahlstrom failed to include Kansas. The 10th Texas Field Artillery Battery was a unit in Confederate Major General Sterling Price’s Campaign late in 1864. While this raid is correctly listed for Missouri, Price’s retreat after the Battle of Westport in the Kansas City area, also took his army

into Kansas. There, along with some small actions near the Little Osage River, the Battle of Mine Creek occurred. In retreat the Texas unit continued to be a part of Price’s army as reaffirmed by Buresh (1977:218) and Arnold W. Schofield (personal communication November 13, 2008). Capt. H. C. Hynson’s Texas Battery lost their two rifled Parrot guns and one-fourth of the unit’s soldiers killed, captured, or wounded at this largest Civil War battle in the state Kansas.

Texas Civil War Artifacts: A Photographic Guide to the Physical Culture of Texas Civil War Soldiers is well written and edited and should be of interest to archeologists, historians (especially students of the Civil War), battle re-enactors, military and specialty collectors, museum curators, and to some degree the general public. Its fine bibliography (especially with the excellent cross-referencing in the captions) makes it a most useful tool for material culture researchers. The volume may be obtained through a local bookstore, by ordering online at www.tamu.edu/upress, or by calling 800-826-8911.

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Skeletal Biology and Bioarchaeology of the Northwestern Plains by George W. Gill and Rick L Weathermon (editors). Foreword by William M. Bass. University of Utah Press, Salt Lake City, 2008. xiv + 320 pp., 129 figs., 70 tables, references cited, contributors, index. \$50.00 (hardcover). ISBN 978-0-87480-928-2. Reviewed by Jim D. Feagins.

Once in a while a book comes along that just begs to be reviewed. *Skeletal Biology and Bioarchaeology of the Northwestern Plains* is just such a volume because of its usefulness; its new insights on Native American, soldier, and pioneer burials; its many innovative approaches to specific research questions; and its overall synthesis of burial research in the region. Gill and Weathermon have done a fine job of compiling and editing the contributions of 22 individuals, including their own writings, into a well organized whole. The volume (not counting the prologue and introductory chapter) is divided into two parts—bioarchaeology (Chapters 1–10) and skeletal biology (Chapters 11–19). Most of the chapters were first presented as papers in a symposium at the 2002 Plains Anthropological Conference in Oklahoma City.

This reviewer's criticisms of the volume are minor. Although it was rather self-evident that "skeletal biology" means primarily "human osteology," it would have been helpful to the non-specialist reader if it, and certain other terms, had been defined in the introduction or a glossary. Also, is simply "the analysis of burials" (p. 11) an adequate definition for "bioarchaeology?" As the volume contains a chapter focused on Crow Indian mummies, the term clearly means more than just osteoarchaeology as some would suggest. Should a reader be expected to know that bioarchaeology is a biocultural approach to the study of human burials or that it includes a number of biological techniques used to interpret behavior from the human skeleton? Just how do the subjects of "skeletal biology" and "bioarchaeology" differ and how are they alike? Clearly the contents of a number of chapters overlap these two subjects, but nowhere in the volume are they completely defined. In Chapter 2 Laura L. Scheiber described their relationship well: "Bioarchaeologists consider skeletal biology, mortuary offerings,

and the archaeological context in order to make interpretations about the past" (p. 22).

As elsewhere, burial research in the Northwestern Plains has come a long way. Recent advances in forensic methods and DNA analysis have an important place in future research and have stimulated forensic and other skeletal research, while at the same time bureaucratic red tape and political correctness have often had the opposite effect. The Native American Graves Protection and Repatriation Act of 1990 (NAGPRA) has served as somewhat of a two-edged sword to both stimulate (in the short term) and depress (in the long term) bioarchaeological and skeletal biology research. Of course, there are some exceptions.

The first numbered chapter, "Before Bioarchaeology: Early Day Records of Human Burials on the Northwestern Plains," is by George C. Frison. This remarkable man has compiled a considerable amount of information on Native American tree/platform and rock crevice burials that he discovered or learned of during his childhood and cattleman days, before he became a professional archeologist. During this time, long before the term bioarchaeology was coined, a more informal approach to the subject was used.

In "Life and Death on the Northwestern Plains: Mortuary Practices and Cultural Transformations," Scheiber presents the history of bioarchaeology in the region. Burials ranging in age from the Paleoamerican to the Protohistoric/Historic periods are summarized.

Weathermon and Mark E. Miller present the story of the development of the Human Remains Repository at the University of Wyoming in Chapter 3. They explain experiences with NAGPRA implementation and with forensic cases and how the collections were developed.

"An Introduction to the Archaeology and Human Osteology of the Benick Ranch Site (48AB571)" is written by Don P. Davis and Miller. This Chapter 4 is a detailed description of the burial site of one or two extended families that date to approximately AD 400–500. Associated with these remains were 275 tubular bone beads (Figure 1) made from the ulnae of sage grouse (*Centrocercus urophasianus*), 7 bone awls and 2 possible awl performs made from mule deer, 7 other bone and antler tools, a mano, 7 chipped-stone tools, and 67 unused flakes.

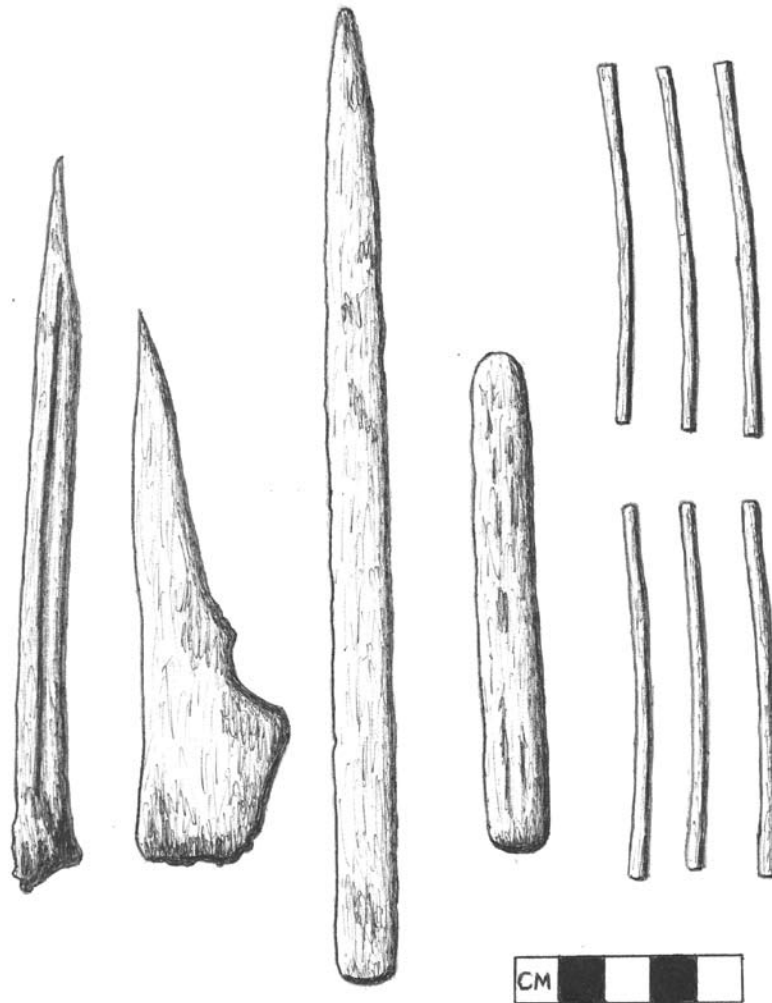


Figure 1. Selected Artifacts from the Benick Ranch Site. Left: two bone awls; middle: two bone/antler tools; right: six tubular bone beads. (Drawn by Jim Feagins from photo in book.)

Chapter 5, “The Korell-Bordeaux Site: A Rare Native American Cemetery in Frontier Wyoming,” is written by Thomas A. Furgeson and Anne K. Armstrong. The Korell-Bordeaux burials are located near the historic Bordeaux trading post along the North Platte River in southeast Wyoming. The chapter briefly summarizes the abundant trade and other goods (over 14,000 beads, 3 copper bells, 63 buttons, ceramics, 81 leather items, 580 metal objects, 126 textile pieces, almost 100 pieces of wood) and the 14 sets of human remains representing both frontier whites and Native Americans. All but one (Figure 2) are coffin burials at this mid-nineteenth century cemetery.

Three Smithsonian Institution staff, Douglas

W. Owsley, Karin S. Bruwelheide, and Laurie E. Burgess, authored Chapter 6, “Late-Nineteenth-Century Crow Mummies from Montana.” This study involves mummified remains of one Crow Indian child reportedly from a tree platform near the Big Horn Mountains in Montana and two mummified Crow remains from near Custer, Montana. These mummifications were unintentional; they were due to natural desiccation from the cold-dry environment. As background information, the authors also briefly discuss mummification from other regional sites—Pitchfork Rockshelter, the Marbleton Mummy, and the Blackfoot Collection in Chicago’s Field Museum. With some of the latter, the mummification was not from desiccation

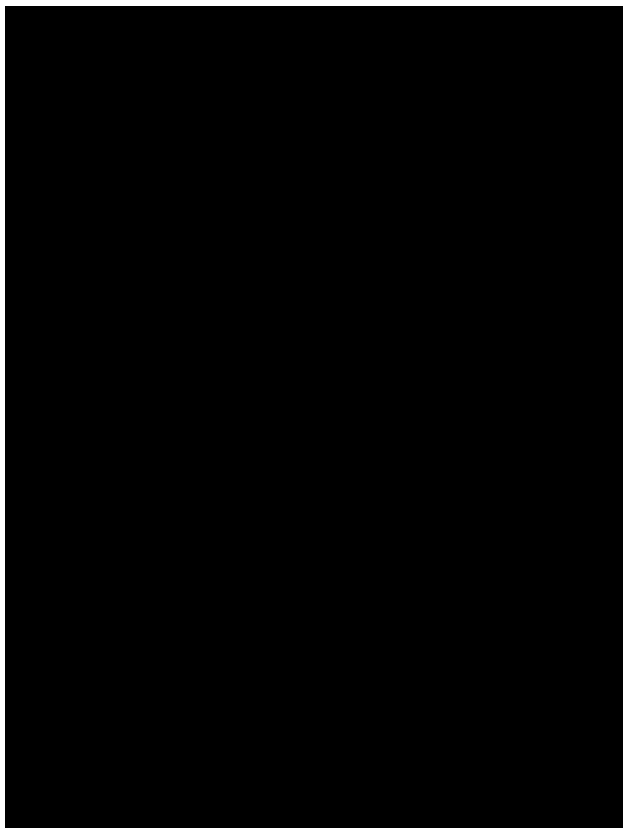


Figure 2. An Adult White Male from the Korell-Bordeaux Site—A Cemetery near the Bordeaux Trading Post SE Wyoming. Note perimortem gunshot hole in skull. (Drawn by Jim Feagins from photo in book.)

but from the inhibition of enzyme-induced lytic processes caused by the poisons associated from the near presence of copper-zinc containing ornaments. The three burials that were the focus of this chapter had well-preserved soft body tissues and associated artifacts that would ordinarily have been long decayed, enabling additional information to be gleaned from the analysis. The study was requested by the Crow tribe, with the understanding that the researchers would adhere to certain guidelines established by the tribe. Of course, these guidelines were honored.

Rennie Phillips Polidora is the author of Chapter 7, “Six Historic Chinese Burials from Southwestern Wyoming.” Additional insight was obtained concerning the health, occupation, clothing, and other burial materials occurring with six individuals who were brought from China to work in the United States. Considering the relatively minor mention of the Chinese in North American text books, studies

like this one can add to an increased understanding of the contributions made by the Chinese and their culture as they at least partially adapted to a new life on a different continent.

The next three chapters (8, 9, and 10) are associated with pioneer or military burials. In “Those Left Behind: An Analysis of Western Pioneer-Era Burials,” Weathermon uses historic accounts (mainly trail diaries) along with burial data in an attempt to identify the graves of individuals exposed during the last two decades along the trails in Wyoming. Weathermon discusses causes of death, the number of trails-era graves in the state, the concentrations of graves and their surface treatment, the expediency of grave and burial variations, and the intentional concealment of graves. Most of us tend to think of the Euroamerican migration westward; however, analysis indicates that there were a surprising number of individuals of Native American or Native American/white parentage who also came west and died along the trails. This chapter presents an especially interesting section on types of trail coffins, coffin hardware, and what pioneers did when there was no coffin material available or no time to build a coffin.

Bruwelheide, Owsley, and Gayle F. Carlson wrote “Military Burials at Cantonment Missouri, 1819–1820, and Fort Atkinson, 1820–1827, Nebraska.” The cantonment was the predecessor of the fort. These two sites are located in close proximity along the west bank of the Missouri River approximately 10 miles north of the modern city of Omaha. While this is an interesting and worthwhile chapter, the sites can hardly be said to be located in the Northwestern Plains. Four sets of human remains are described. Detailed forensic analysis and a record search made it possible to identify one set as the remains of Lt. Gabriel Field. This identification was aided by various military records, among which were those showing that Lt. Field had experienced an above-the-knee amputation not long before his death on April 17, 1823. The right femur of the skeletal remains had been completely sawed through. The amputated leg had been retained and then buried with the rest of the body.

Readers, like this reviewer, who are students of the Indian Wars, will be interested in Weathermon’s chapter titled “A Bioarchaeological and Historical Analysis of an Indian Wars-Era Casualty from the

Platte Bridge Station, Dakota Territory.” On July 26, 1865, an overwhelming number of Cheyenne and Sioux attacked troops of the 11th Kansas Volunteer Cavalry and the 11th Ohio Volunteer Cavalry as they attempted to relieve a military wagon convoy also under attack not far from the bridge. Earth moving activities by power equipment in 1990 uncovered a soldier’s burial from that engagement. A slightly deformed lead ball was found in the right mid-thoracic area, and the individual also had considerable evidence of perimortem skeletal trauma—a broken left ulna and powerful multiple blows (blunt force trauma) to the cranium. The soldier’s coffin had been constructed from expediency material: the pine wood and hardware from a wagon box. While not absolutely proven, the skeleton and its associated burial evidence, along with archival records, suggest the remains may have been those of Private George W. Camp.

The remaining chapters (11–19) are those that editor Gill has categorized under the heading of “skeletal biology,” as opposed to “bioarchaeology” for the preceding chapters (p. 4). Because of the personal interests of this reviewer, the focus of this review will be on the first part of the volume. Briefly, Chapters 11 and 12, “Northwestern Plains Indian Skeletal Remains: Metric Analysis” by L. Jaimeson Stuart and Gill and “An Analysis of Discrete Cranial Traits of Northwestern Plains Indians” by Martha Rogers, collectively consider migration, gene flow, and microevolutionary trends.

Chapter 13, “Disease and Injury among Northwestern Plains Indians” by Maxine J. Miller, discusses how subsistence strategies affect the skeleton, congenital disorders, skeletal changes due to aging, various types of trauma (types of weapon marks on males as opposed to those on females), trophy taking, and the effect of extensive horse riding on the skeleton.

Owsley and Bruwelheide wrote Chapter 14, “Cranial Trauma in Historic Period Central and Northern Plains Indians.” Encompassing most of the plains, the authors focus on interpersonal violence from 406 historic skeletal remains, including 217 from 11 known village and equestrian tribes: Arikara, Omaha, Pawnee, Ponca, Blackfeet, Brule, Cheyenne, Crow, Gros Ventre, Oglala, and Sisseton. More than 23 percent contain evidence of at least one skull injury. These injuries included those

caused by blunt force (such as a club), by sharp-force (edged or bladed trauma, such as swords or tomahawks), and from projectiles (such as bullets or arrows). Over two-thirds of the cranial injuries were caused by blunt force trauma, including crushing. The data was broken down according to age, sex, and if the individual came from an equestrian or village tribe. Additional evidences of violence “. . . include scalps, cuts on bone resulting from scalp-ing, decapitated skulls, and other human trophies” (p. 225).

Chapters 16, 18, and 19 focus on specialized portions of the skeleton. These chapters are “Sexual Dimorphism in Femoral and Humeral Head Diameters of Northwestern Plains Indians and Whites” by Kierstin K. Catlett, “Temporal Changes in Nasal Sill Development Within the Northwestern Plains Region” by Gill and Cresta Valentine Deeds, and “Temporal and Geographic Variation in Vault Height in the Great Plains, Great Basin, and Northwestern Plains” by Richard L. Jantz. Ancestry/race can make a difference in determining the sex of individuals based on the maximum femoral head diameters and maximum vertical humeral head diameters. The tables of metric data should be useful to other researchers when attempting to sex skeletal remains of Native Americans from the region. Within the last few thousand years in the Northwestern Plains, the nasal sill (at the lower margin of the nasal aperture) among native populations has evolved into a sharper margin. The vault height of skulls in the geographic regions described decreased during the period when agriculture usage intensified. Jantz points out that vault height variation was also greatly influenced by growth of the cranial base.

“Northwestern Plains Archaic Skeletons with Paleoamerican Characteristics” is the title of Chapter 17, which most Kansas readers should find quite interesting. Gill states, “A new direction in Paleoindian skeletal biology began to coalesce in North America after 1990 because of a number of more or less simultaneous occurrences” (p. 241). One of the first skeletal remains that made physical anthropologists really ponder this was from an Archaic period burial at Harlan County Lake in south-central Nebraska. Gill goes on to state, “This well-preserved skeleton shows . . . strongly developed Caucasoid skeletal trait patterns . . . ”

(p. 241). The 9,300-year-old Spirit Cave Man from Nevada, the 8,400-year-old Kennewick Man from Washington, and a host of Archaic skeletons from a variety of places have established important physical contrasts between Paleoamericans and later Native American populations. By the 1990s these differences could no longer be ignored. Gill (p. 241), quoting from Steele and Powell states, "In those respects where Paleo-Indian specimens differ from modern northern Asians and north American Indians, they tend to resemble southern Asian and European populations" (1992:303). With greater attention to biological details and with a greater focus on comparisons of skeletal traits through time, other researchers at about the same time began to notice differences in South America between the earliest Americans and later Native Americans (Neves and Pucciarelli 1991). While this information raises a number of questions about migration of populations to the New World, Gill's descriptive article suggests few answers to the broader questions this data implies. He feels that the important question, "... why some Paleoamericans and Archaic individuals clearly show less of the 'Mongoloid craniofacial trait complex' than later Native American groups ..." (p.260) might be answered

with more and better samples over time. Clearly the peopling of the New World is much more complex than the theory that generally has been accepted by archeologists during the last century.

While *Skeletal Biology and Bioarchaeology of the Northwestern Plains* was primarily written for professional archeologists, physical anthropologists, and historians, many chapters contain information that would be of more than passing interest to serious avocational archeologists and those interested in related fields. It has presented some new intellectual vistas for this reviewer. The University of Utah Press is to be commended for its publication.

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The Archaeology of Institutional Confinement.
ELEANOR CONLIN CASELLA. University Press of Florida, Gainesville, 2007. xx + 174, 32 black and white photos, 3 tables, bibliography, index. \$24.95 (paper). ISBN 978-0-8130-3139-2. Reviewed by *Mary E. Conrad.*

The Archaeology of Institutional Confinement is the second volume in the series *The American Experience in Archaeological Perspective*. After about 20 pages or so, the reader may wonder when the author will begin to talk about archeology. This reviewer quickly learned that the core of the information relating specifically to archeology does not begin until page 84. After a foreward and acknowledgments, 83 pages are devoted to the theories for incarceration with a historical overview of how these have changed over the last 400 to 500 years.

Author Eleanor Conlin Casella, an archeologist who has directed fieldwork in Europe, the United States, and Australia, focuses on confinements in the 13 American colonies and in the United States, although she includes the European backgrounds for such practices. The first chapter is a theoretical synopsis. Then in Chapters 2 and 3, Casella centers on why societies incarcerate. This theoretical outlook is quite comprehensive, and the explanations are detailed and thought provoking.

The researcher interested in the material culture of traditional prisons can find in Chapters 2 and 3 many relevant references within the historical overview of prison development. While the structures of prisons and artifacts are referenced in these chapters, the author stresses theoretical reasons for confinement to such a degree that some may have to pull out the facts and nuances related to artifacts. Casella devotes more space to the structures of prisons than to artifacts used within them. Granted,

the confining structures themselves are of archeological concern.

Chapter 4 quite specifically looks at the material culture of three American colonial and early state prisons before delineating the structure and representative artifacts of Alcatraz, an American military prison from the 1850s to 1932, when it became a federal penitentiary lasting into the early 1960s. Within this fourth chapter, Casella continues with types of structures and artifacts found in other institutions of confinement, including early asylums, almshouses, poorhouses, and poor farms; Civil War prisoner-of-war camps; American Indian schools; and Japanese American internment centers.

As shown repeatedly in the section on material culture, the discipline of archeology certainly can add to the understanding of places of confinement. The plethora of toothbrushes found at American Indian school sites serves to confirm tales of rigid tooth-brushing drills. At these same sites the presence of knapped objects from window glass, bottles, and white dinner plates reveals that Indian ways were not forgotten by all in those boarding schools.

Analyses of faunal remains from numerous sites of almshouses and poorhouses have shown that foods provided were frugal but adequate, while such an analysis at the Civil War-era Andersonville Prison confirmed that many prisoners of war were starving. Many historical accounts report that tinware was the sole dishware used in almshouses, yet archeologists have uncovered significant numbers of ceramic dishware at many such sites.

The presence of many fine familial heirlooms, such as porcelain figurines and dishes that did not end up in trash dumps, suggests that the residents of poorhouses and Japanese American internment camps could keep more personal possessions than had been thought. Furthermore, archeologists have found that Japanese American internment facilities were modified significantly to recreate reminders of home. The remains of gardens and ornamentations, along with rock alignments and sculptures, are distinctively Japanese American. Concentrations of school supplies confirm that Japanese Americans set up schools during their confinement.

While some think immediately of shackles and escape tunnels, such objects represent quite a narrow view of confinement. Archeologists want to

look at any available material culture from a confinement site, whether from a traditional prison or from another type of institution, to develop an overview of the place over time.

This book not only can be read in its entirety, but also can serve as a reference. A quick perusal of the index reveals topics of interest to many. A few of these are as follows: African Americans, Alcatraz, Andersonville, cemeteries, children, gender, Native Americans, and women. The 14-page list of references includes both historical and archeological studies. By skimming the titles, a researcher likely can pick out many archeological reports of interest.

Due to the title, this volume may be read only by archeologists and perhaps by cultural anthropologists. This is regrettable because many others, such as police, penalogists, social workers, historians, politicians, and more, could benefit from reading this detailed historical analysis of incarceration from the 1600s or so in Europe and in the 13 British American colonies up to Guantanamo Bay of the early twenty-first century. The book might be more appropriately titled *American Institutional Confinement with Historical and Archaeological Perspectives*. With such a title, the book would appeal to a wider audience.

Nonetheless, *The Archaeology of Institutional Confinement* is packed with insights that could be of interest to many. If anyone undertakes an archeological or historical study of any place of confinement, Casella's volume is a good place to start, i.e., to obtain an overview of general and specific concerns. To her credit, Casella does broaden the concept of institutional confinement from prisons to a wide variety of institutions. She proposes that almost any type of school, as well as various other institutions, can be considered sites of confinement. With this book in hand, researchers will be able to apply Casella's concepts to other places of confinement not delineated in this volume. Case studies of yet more types of confinement will add to the present understandings.

The Archaeology of Institutional Confinement may be obtained through local bookstores, online at www.upf.com, or by calling the University Press of Florida at 1-800-226-3822.

ABOUT THE AUTHORS

Mary Conrad



Mary Conrad, a KAA member since 1986, is a high school librarian at Sumner Academy in Kansas City, Kansas.

Jim D. Feagins



Jim Feagins is a private archeological consultant who also serves as part of the adjunct archeological staff for the St. Joseph Museum and is senior archaeologist for Environmental Advisors and Engineers, Inc. He received a B.S. degree from Pittsburg State University and a M.N.S. degree from the University of Oklahoma. After 30 years as a science instructor for Consolidated School District No. 4, Grandview, Missouri, he retired in 1995.

Active in promoting archeological education, he is president emeritus of the Missouri Archaeological Society. He also served as president of the Kansas City Archaeological Society and as a vice-president of the Missouri Association of Professional Archaeologists and the Kansas Anthropological Association (KAA). He is a life member of the KAA.

Marlin F. Hawley



Marlin is presently an archeology curator with the Museum Archaeology Program at the Wisconsin Historical Society. He has authored or co-authored papers in *The Kansas Anthropologist*, *Oklahoma Archeology*, *Plains Anthropologist*, *Southeastern Archaeology*, *North American Archaeologist*, *The Minnesota Archaeologist*, *The Wisconsin Archeologist*, *Current Research in the Pleistocene*, *Nutritional Anthropology*, *The Streamliner*, and the *Journal of Spelean History*. With Donald Blakeslee, he co-authored the chapter on the Great Bend aspect in the 2006 volume, *Kansas Archeology*, edited by Robert Hoard and Will Banks. He and Virginia Wulfkuhle also edited a collection of retrospectives by a number of Plains archeologists, which was published in 2006 as *Plains Archaeology's Past* as the 200th issue of *Plains Anthropologist*. Marlin, his wife, and their two children live in DeForest, Wisconsin.

Craig Kitchen



Craig Kitchen wrote this article as an undergraduate at Kansas State University. He is currently attending Southern Illinois University to earn his master's degree in archeology, specializing in Mississippian peoples.

James O. Marshall



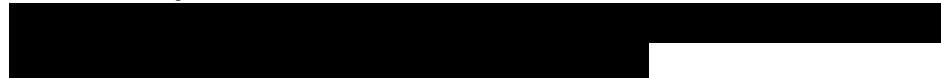
James Marshall has participated in archeological projects in New York, New Mexico, Nebraska, and Kansas, where he was employed by the Kansas State Historical Society. Almost all of the Kansas projects were a part of the Missouri River Basin Archeological and Paleontological Surveys. Major publications are

concerned with the historic and prehistoric cultures that have been found in Kansas, including the Kansa Indians.

John Reynolds
Deceased

John joined the staff of the Kansas State Historical Society in 1970. He became state archeologist in 1993 and served as assistant division director of the Cultural Resources Division from 1994 until his death in July 1997. He co-directed and regularly taught classes for the Kansas Archeology Training Program. He was a master flintknapper and developed an expertise in other aspects of experimental archeology, including grass house reproduction. John was a natural teacher and storyteller. He was an honorary life member of the KAA.

Donna C. Roper



Donna continues to contribute regularly to *The Kansas Anthropologist*. Readers who don't know her can consult recent issues for the boring straight facts about her background—degree, when, where, and all that sort of stuff. Her current work centers on analysis of late prehistoric pottery from the Central Plains. This has her analyzing decoration in excruciating detail, measuring as many whole vessels as she can find (“have calipers, will travel”) to study vessel form, mining the literature for comparative data, and, recently, investigating how using bison dung as fuel might affect ceramic firing. She also is involved with the current work at the Pawnee Indian Museum (14RP1). Crosscutting this, she has a mania for things like using sound reasoning and appropriate analysis methods for developing interpretations of the archeological record and for good stewardship of increasingly threatened archeological resources. This has led to, respectively, the article and the book review in this volume.

Randall M. Thies



Randy recently retired after many years as a highway archeologist with the Kansas State Historical Society. During that time he was involved in Kansas burials, both prehistoric sites and historic cemeteries, but currently he is working like crazy, both around the house and out back by the pond.

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The varied readership of the journal should be kept in mind when preparing papers. **Jargon should be avoided.** The style authority is *American Antiquity* (see Volume 57, Number 4, 1992). Professionals are expected to submit their manuscripts in this form; others who are not familiar with the *American Antiquity* style guide will receive editorial assistance.

Illustrations are encouraged; at least two or three should be included if possible. **Digital photography with original resolution less than 300 dpi is not optimal for print publications.** High resolution digital images are acceptable. The preferred format is 35-mm black and white or color prints or slides. All illustrations should be designed to fit within 6 x 8.5-inch margins, including caption. If protected by copyright, securing written permission for use is the responsibility of the author.

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Submit manuscripts to the address below. The editorial staff is here to help you. If you have any questions, please contact:

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