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

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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 Harayey 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 Harayey 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 antiques 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.

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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).  

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 unprovenienced 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 Withthoft (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...
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, naively 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 Harahay, the protohistoric Pawnee. Brower then identified the center of Quivira as being in the Manhattan-Junction City area, and he located Harahay 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 (Upahm 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. 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.

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. This find actually consisted of the incomplete remains of an adult male and a
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 archaeologists "... 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 archaeologists was not altogether accurate, it did reflect, in part, the prevailing attitude of many prominent archaeologists 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
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 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 half-tone 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
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
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 processes.

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 party.

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 researchers. Wedel (1959:95) notes that one of Winchell’s contemporaries at the U.S. National Museum—and Wedel did not supply a name—pened 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 archeological 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 cultures. 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 reshipping 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, Whitaker 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 be 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 archeological 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 7,900 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. Convoluted, 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 archaeologists and collectors appointed in 1914 by the KSHS's Secretary William E. Connelley to a standing Committee on Archaeology (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 Pottawatomi 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. Griffin, 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 Harabey by Coronado in 1541 and Brower’s rediscovery of it. Keagy may have used some artifacts in his possession to practice flintmakinng 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 is is curious that Keagy read a draft of the monograph (Winchell 1913:65), 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). Uptham (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 archaeologist 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 deafen-
ing 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 imple-
ments 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 in-
dicate 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 cheselike 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 be-
thief that humans had long been established on the North
American continent.

Such a conclusion is potentially of more than histori-
ocal interest, especially in light of the ongoing debate on
the authenticity of the Kensington Runestone, as the pa-
tina thereon was also studied by Winchell (i.e., Winchell
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

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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 charcoal 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 recutting within the span of a decade, if not much less. By the time Winchell saw these artifacts in 1912, this recutting 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 were 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 archaeological materials.

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