Folsom and the Human Antiquity Controversy in America

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Folsom played a pivotal role in the development of American archaeology. Most everyone knows this. What may be less well known is why this particular site, alone among dozens of localities championed since the mid-nineteenth century, including several bison kills, finally established that humans were in the Americas by late Pleistocene times (see Meltzer 1991b). What may not be known at all is why, in the decade after the breakthrough at Folsom, the site's investigators—Jesse Figgins and Harold Cook—were completely excluded from professional discussions of the site and North American Paleoindians.

As it happens, those issues are linked in ways that reveal much about the history and context of research into human antiquity in America, and about the nature of scientific controversy and its resolution. This chapter explores those issues, but two brief comments on what this chapter is not: (1) it is not intended to be a strict narrative of the history of fieldwork at Folsom (the necessary parts of that are given in chapter 4) but, rather, aims more broadly at this and other archaeological and paleontological localities being investigated in the 1920s, to show how events and actions elsewhere set the stage and influenced the work—and the perceptions of the work—here at Folsom; (2) this chapter is also not intended to be an overview of the human antiquity controversy, although it necessarily requires a brief summary of that long and bitter dispute in order to establish the intellectual backdrop against which the research at Folsom was inevitably set and the gauge with which the evidence from this site would be measured (see also Meltzer 1983, 1991b, 1994).

This chapter explores just what made the Folsom site so important, why it mattered, and what it meant for the discipline and those involved, by seeking to answer—invoking the spirit of Groucho Marx—a deceptively simple question.

Who's Buried in Grant's Tomb?

The Folsom discovery in 1927 triumphantly resolved a dispute over human antiquity in the Americas that reached back to the mid-nineteenth century. Today, we credit Figgins and Cook with the "breakthrough" at Folsom (e.g., Daniel 1975:275; Fagan 1987:50–51; Willey and Sabloff 1980:121; Wilmsen 1965:181; Wormington 1957:23–25). We do so for seemingly good reasons.

After all, it was Cook, then an Honorary Curator of Paleontology at the Colorado Museum of Natural History (CMNH), whose report on the Lone Wolf Creek (Texas) site evidently spurred Fred Howarth, a Raton, New Mexico, banker, to see the potential of the Folsom site and bring some of the deeply buried bison bones from the site to the Colorado Museum. It was field parties under Cook and Figgins—the latter then Director of the Museum—that in 1926 conducted the initial excavations at the site and discovered, though not in situ, the first Folsom projectile points.

It was Figgins who traveled east in early 1927 to show the Folsom artifacts to various skeptics, Smithsonian anthropologists Aleš Hrdlička and William Henry Holmes among them, in an unsuccessful effort to convince them of the age and association of the find (Wormington 1957: 23). And it was Figgins who again sent crews to Folsom the following summer, where his faith was rewarded on August 30, 1927, when the crew found a projectile point, this time embedded between the ribs of an extinct species of bison. It was Figgins who sent telegrams nationwide, inviting the scientific community to come view the Folsom artifact in position and confirm its age and context (Meltzer 1983, 1993).

In looking back at this episode we routinely lump the Folsom discovery and discoverers with the resolution of the human antiquity controversy. Cook and Figgins' dual 1927 publications in *Natural History* (Cook 1927a; Figgins 1927a) are routinely cited as marking this turning point in American archaeology—when our discipline finally found itself in possession of deep time (e.g., Willey and Sabloff 1980:121).

But consider this: Four months after the electrifying news from Folsom, the American Anthropological Association devoted one of the four symposia at its December 1927 annual meeting to "The Antiquity of Man in America" (Hallowell 1928). There Nels Nelson of the American Museum of Natural History (AMNH) spoke about the long-standing controversy over human antiquity in the Americas and the implications of the Folsom discovery for that dispute, while Frank H. H. Roberts of the Bureau of American Ethnology (BAE) and Barnum Brown of the American Museum addressed the site's archaeology and paleontology, based on what they had seen there when they visited Folsom in response to Figgins' telegrams that September of 1927 (Hallowell 1928:543).

Nelson and Brown were, of course, established figures in 1927: Brown was a well-known vertebrate paleontologist, while Nelson was an archaeologist with an involvement in the human antiquity issue that reached back over a decade (e.g., Nelson 1918, 1928a). Both of them had been following events at Folsom from the outset, but Nelson had not visited Folsom that September to see the point in situ (Nelson to Figgins, September 13, 1927, JDF/DMNS). Brown had, but then he had had little prior experience with Pleistocene faunas, and none with an archaeological fauna. In December of 1927, Roberts was a newly minted Harvard Ph.D. who had been at BAE only a year, and whose prior work was on Late Prehistoric sites in the Southwest (Judd 1967). He was not the Paleoindian archaeologist he would ultimately become—his reputation-building excavations at Lindenmeier were still years away. When he visited Folsom that September on behalf of the Smithsonian, his degree was scarcely two months old, and by December his sole experience with Paleoindian materials generally or Folsom in particular was the three days he spent visiting the site.

The AAA meetings that December in Andover were a triumph. The artifacts from Folsom held center stage, and "all the anthropologists and archaeologists present accepted the authenticity of the find, saying they established a definite landmark in the history of prehistoric man in America" (Brown to Figgins, January 10, 1928, VP/AMNH; Jenks to Figgins, January 4, 1928, DIR/DMNS). Yet, it was Brown, Nelson, and Roberts who spoke at that session, not Cook or Figgins. Neither of them was even asked to participate—only to loan photographs and specimens (Brown to Figgins, December 8, 1927, DIR/DMNS).

In fact, from 1927 to 1937 there were seven major symposia devoted to human antiquity in the Americas. In these, the fast-emerging fundamentals of Paleoindian chronology, artifacts, and faunal associations were being hammered out. Each of these discussions took place on national and even international stages. The symposia were held at the 1927 American Anthropological Association meetings (Hallowell 1928); the 1928 meeting of the New York Academy of Medicine; the 1928 Geological Society of America meeting, held jointly with the American Association for the Advancement of Science (AAAS); the 1931 AAAS meetings (Danforth 1931); the 1933 Fifth Pacific Science Congress (Jenness 1933); the 1935 meeting of the American Society of Naturalists (Howard 1936); and, finally, the 1937 International Symposium on Early Man sponsored by the Academy of Natural Sciences of Philadelphia (MacCurdy 1937).

Taking their turns at the center of these stages speaking about Folsom archaeology and geology, including the type site, were Kirk Bryan, Edgar B. Howard, John C. Merriam, E. H. Sellards, and Chester Stock, along with Brown, Nelson, and Roberts, among others. In almost every case, Hrdlička was invited to present his views on the subject and the site, and on several occasions he did so (e.g., Hrdlička 1928, 1937; see also Dixon to Hrdlicka, November 27, 1927; Boas to Hrdlička, April 21, 1931; Howard to Hrdlička, November 30, 1936, all in AH/NAA).

Yet, in all the planning that went into the selection of participants for these meetings, I have found only two occasions when Cook or Figgins was even suggested as a possible speaker. In both cases the suggestions were ignored (see Gregory to Boas, April 27, 1931, FB/APS; Howard to Merriam, November 16, 1935, JCM/LC).

Cook did receive an invitation to attend the 1937 International Symposium on Early Man in Philadelphia, where, had he gone, he could have listened to Bryan and Roberts discuss the Folsom site. But the press of business prevented his attending (Howard to Cook, December 31, 1936; Cook to Howard, March 15, 1937, HJC/AGFO), and perhaps that was for the best. Otherwise, he would have heard Bryan describe the Folsom finds as "discovered" by Figgins and Cook, but "confirmed by the masterly excavation of the site by Barnum Brown," and heard Gladwin give sole credit for Folsom to Brown and not even bother to mention Cook or Figgins (Bryan 1937:139–140; Gladwin 1937:133).

Cook and Figgins' complete absence from the postdiscovery discussions of Folsom, let alone of the human antiquity issue, and the fact that their contributions were completely ignored, if not devalued, by their peers—despite their continued hand in Paleoindian research (e.g., Figgins 1933a, 1934, 1935)—are rather surprising, at least given how we look back on the Folsom episode today. Yet, I think their absence and the contemporary measure of their contribution show that the Folsom *discovery* and the subsequent *resolution* of the human antiquity controversy were very separate events involving very different participants. They also show, when probed deeper, the sharp boundaries of scientific status and rank, and a clear lesson about the nature of the scientific enterprise. And, finally, they provide a cautionary tale of what might have been, had Figgins not visited Hrdlicka at the Smithsonian Institution in the spring of 1927 and showed him the first points recovered from the site; for Folsom was not the only locality championed as evidence of a Pleistocene human presence in the Americas, but it was the only one that was accepted.

Elsewhere, I detail the history and issues involved in the nineteenth- through early twentieth-century dispute over human antiquity in the Americas that provide the larger context for the Folsom discovery (e.g., Meltzer 1983, 1991b, 1993, 1994; Meltzer and Wormington 2005), and that ground need not be covered again, save in summary fashion.

Background to Controversy

The possibility that the arrival of people in the Americas might be geologically ancient was only seriously considered after 1859/1860, when the Old Testament barrier was finally broken in Europe (Grayson 1983). At Brixham Cave, England, and in Abbeville in northwestern France, human artifacts were found in direct association with extinct mammals: mammoths, cave bear, hyenas, and the like (Evans 1860; Prestwich 1860; also Grayson 1983, 1990; Gruber 1965). Although the absolute age of those animal remains was unknown—this was a century before the advent of radiocarbon dating—their presence in a deposit was widely accepted as marking an earlier geological period (the Pleistocene), one which predated the modern world (Lyell 1830–1833). That human artifacts were in those same deposits meant humans too had a past beyond history, a fact with deep and profound intellectual consequences (Grayson 1983; Stocking 1987).

The shock waves of that realization quickly reached America owing largely to Joseph Henry, first Secretary of the Smithsonian Institution. He issued a set of detailed instructions (Gibbs 1862) to military officers, missionaries, and other travelers in the "Indian country" on how to search for and record archaeological evidence that might reveal "analogous stages of the mental development of the primitive inhabitants of this country and those of Europe" (Henry 1862:35; see also Hinsley 1981; Meltzer 1983).

With that, archaeological questions of the origin, antiquity, and adaptations of the first Americans emerged in a recognizably modern form. They began with the hope that there would be evidence in America of stone tools alike in form, evolutionary "grade," and antiquity as those of Paleolithic Europe. Although naïve in retrospect, in the 1860s there was good reason to expect as much, for it was generally believed that there was an "exact synchronism [of geological strata] between Europe and America" (Whittlesey 1869).

By the 1870s stone artifacts, seemingly akin to ancient European Paleolithic tools, were reported by Charles Abbott from apparent Pleistocene-age gravels at Trenton, New Jersey (e.g., Abbott 1877). He had little doubt of their antiquity. After all, "had the Delaware River been a European stream, the implements found in its valley would have been accepted at once as evidence of the so-called Paleolithic man" (Abbott 1881:126–127).

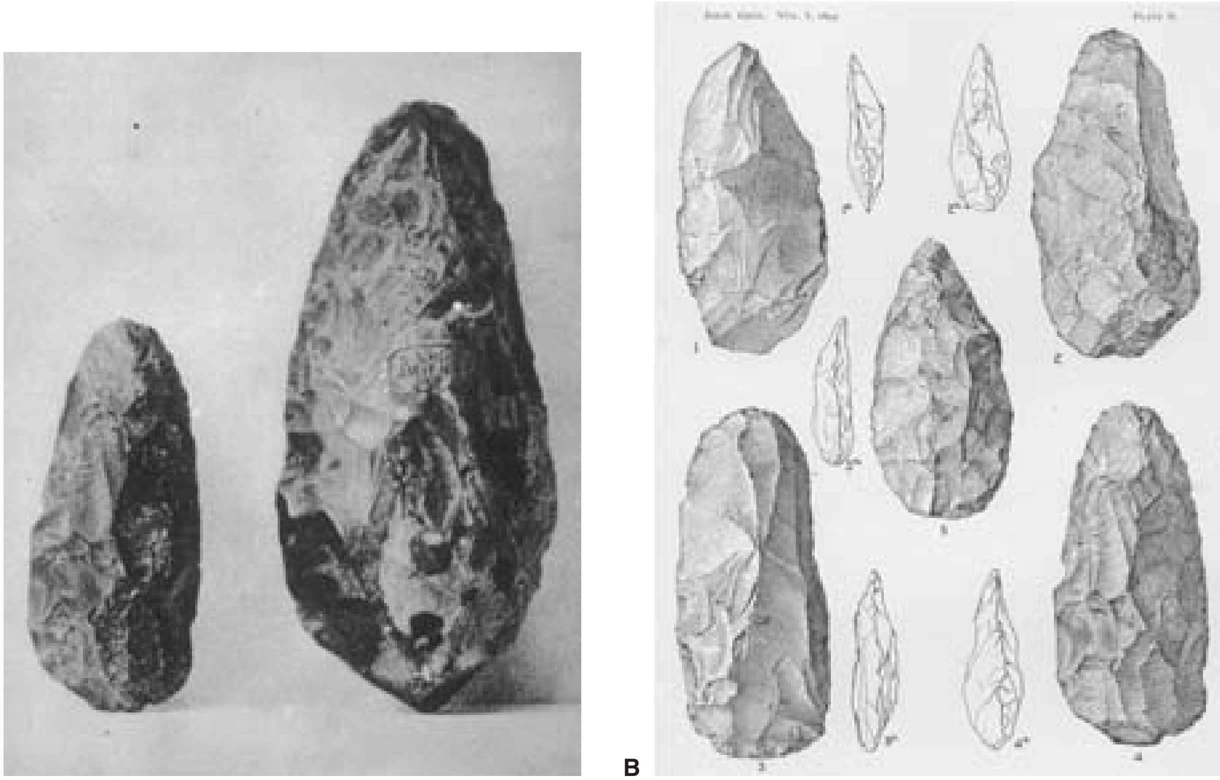
Abbott's discoveries were soon replicated by others. In the spring of 1883 G. F. Wright predicted that "when observers become familiar with the rude form of these Paleolithic implements they will doubtless find them in abundance." He was correct. Over the next decade, many more "American Paleolithic" artifacts were reported from surface and buried contexts at other sites, and their presence was taken as proof of prehistoric Americans living here thousands, if not tens of thousands of years ago, when northern latitudes were shrouded in glacial ice. It did not matter that the precise age of these tools proved difficult to pin down by geological evidence (Lewis 1881; Shaler 1876; Wright 1881, 1888, 1889b). These artifacts so readily mimicked European Paleolithic tools of undeniable antiquity that they were assumed to be just as old (Haynes 1881:135–137; Putnam 1888:423–424). Abbott (1881) concluded triumphantly that "the sequence of events, the advance of culture, have been practically synchronous in the two continents; and the parallelism in the archaeology of America and Europe becomes something more than "mere fancy" (517).

By 1889 the evidence for an American Paleolithic was almost universally accepted. If there was skepticism about it, it was well hidden. Indeed, British Paleolithic expert Boyd Dawkins (1883) himself proclaimed that the American "implements are of the same type, and occur under exactly the same conditions, as the river- drift implements of Europe" (347). The last years of the 1880s saw a parade of symposia, feature articles, and books, all testifying to the veracity of the American Paleolithic (e.g., Abbott 1889, Dawkins 1883; McGee 1888; Putnam 1888, 1889; Wallace 1887; Wright 1889). For many, the only lingering question was how early in the Pleistocene humans may have arrived.

Yet, scarcely a year later the American Paleolithic was under harsh fire, sparked by William Henry Holmes' (1890) studies of stone tools at the prehistoric Piney Branch quartzite quarry in Washington, D.C. He learned there that an artifact might appear "rude" merely because it was unfinished, not because it was ancient. To explain why that was, Holmes drew on the then-popular notion in biology that ontogeny recapitulates phylogeny, such that in the evolution of a species, ancestral adult forms become descendant juvenile stages. As Holmes (1894) translated this into archaeological terms, the "growth of the individual [stone tool] epitomizes the successive stages through which the species [history of stone tool making] passed" (137). Thus, if early on in the process of manufacture a stone tool was discarded or rejected, it would appear like the "rude" and ancient stone tools of Europe (fig. 2.1).

Thus, the analogical argument that the similar form of American artifacts with European Paleoliths implied a similar age and evolutionary grade, as was routinely argued by American Paleolithic proponents, was flawed. Artifact form, Holmes (1890) stressed, had no chronological significance whatsoever. Age must be determined independently, by the geological context of the artifacts (also Holmes 1892).

Paleolithic proponents like Abbott and Wright replied that the similarity between paleoliths and the Piney Branch quarry debris was purely accidental and thus irrelevant to the antiquity issue (Abbott 1892). Critics retorted that American and even European paleoliths sometimes looked



A B

FIGURE 2.1 Left, G. F. Wright's (1890) composite image of the Newcomerstown paleolith alongside a paleolithic biface (reduced to one-half size) from Amiens, France; right, Holmes' (1893b) depiction of the Newcomerstown specimen alongside "four ordinary rejects." Holmes left it to the reader to decide which of the five specimens was from Newcomerstown, and which were quarry rejects. (From Wright 1890, Holmes 1893b.)

alike only because some European paleoliths were themselves quarry rejects (Meltzer 1983). Each side recognized that the key to sorting unfinished rejects from finished paleoliths lay in whether the objects showed signs of use. But even though they examined the very same objects, they could not agree whether or not they were used.

The growing dispute over the American Paleolithic, Abbott himself would come to admit, was resolving itself as a geological matter, but geology was providing little guidance. There were questions of whether an artifact had actually come from a primary context, an ambiguity complicated by the absence of agreed-on field strategies for removing artifacts and reading their stratigraphic units and depositional contexts. Then there were questions about the age of the artifact-enclosing deposit, which in the 1890s and early decades of the twentieth century were thoroughly entangled in an increasingly contentious debate over how to recognize Pleistocene-age strata and a simmering controversy over the number and timing of the glacial periods (e.g., Chamberlin 1893b, 1903; Salisbury 1893b; Wright 1889a, 1889b, 1892).

That controversy and the American Paleolithic dispute exploded publicly in the last months of 1892, sparked by the appearance of Wright's *Man and the Glacial Period*, which advocated both an American Paleolithic and a single glacial period. Wright's book met an ugly reception at the hands of critics who, directed by Thomas Chamberlin, Chief of the Glacial Division of the United States Geological Survey (USGS), savaged the book's archaeological and geological claims and contents (e.g., Chamberlin 1892, 1893a; McGee 1893a; Salisbury 1892a, 1892b, 1893a). The critics, in turn, were counterattacked by Wright's allies and other Paleolithic proponents (e.g., Claypole 1893a, 1893b; Winchell 1893a, 1893b; Youmans 1893a, 1893b).

Yet, the battle over *Man and the Glacial Period* was only nominally about Wright's advocacy of an American Paleolithic and the unity of the glacial period. Instead, it thinly veiled a proprietary dispute between government and nongovernment scientists (Meltzer 1991b). Long-simmering resentment about the perceived heavy-handedness of USGS and BAE scientists boiled over, which, in the wake of economic hard times brought on by the Panic of 1893, triggered a new wave of attacks on profligate federal science (Rabbitt 1980; Worster 2001). While Wright and his defenders sought redress in the press and in Congress, Holmes stayed on the attack, systematically criticizing all Paleolithic claims, Abbott's Trenton gravels included (e.g., Holmes 1893a, 1893b, 1893c; Meltzer 1991b). By the August 1893 meeting of the AAAS, the talk of the Paleolithic was fiery, and the positions on either side had hardened beyond compromise (e.g., McGee 1893b; Moorehead 1893).

Following a few years of relative quiet on the rhetorical scene, the principals separately visited Trenton in June and

FIGURE 2.2 Aleš Hrdlička examining the stratigraphy in the Gilder Mound, Nebraska, January 1907. (Photo by E. Barbour, courtesy of Nebraska State Museum.)

July of 1897, then met to confront each other at the AAAS meetings that August. Once more, their positions were irreconcilable. But this time the BAAS was meeting jointly with the AAAS, and Sir John Evans himself, the Dean of the European Paleolithic, was there. At the meetings he was shown a set of the Trenton paleoliths but dismissed them as Neolithic, not Paleolithic (McGee 1897). It was a devastating blow.

Still, by decade's end contemporaries would call the American Paleolithic battle a draw. Nonetheless, on one point the critics undeniably won: By the turn of the century new discoveries of American paleoliths had virtually ceased, and advocates of the American Paleolithic could do no more than recycle very worn evidence. The active search for a deep past continued, however, and with the discovery in December 1899 of a human femur in the Trenton gravels, the character of the evidence for deep human antiquity, and the nature of the controversy, took an abrupt turn.

The femur was turned over to Aleš Hrdlička (1902), who at that time was noncommittal as to its age and affinities and had no particular opinion about human antiquity in the Americas. That would soon change. By 1902, in the employ of Holmes at the Smithsonian, Hrdlička embarked on a thorough vetting of all skeletal remains purportedly of great antiquity and came away convinced that none was anything more than recent in age. Hrdlička's criticisms rested largely on his strongly held belief that earlier forms of the human species would slowly dissolve into a mosaic of primitive features as one moved back in time. Thus, any allegedly Pleistocene humans found in the New World ought to look primitive. So far as Hrdlička was concerned, none did. Over the next several decades, like Holmes before him, and often with Holmes at his side, Hrdlička challenged each and every claim to a Pleistocene human antiquity: "Like Horatio at the land bridge between Asia and North America, mowing down with deadly precision all would-be geologically ancient invaders of the New World," as Hooton (1937:102) put it (also Kroeber 1940:461).

In structure, Hrdlička's argument in bone was no different from Abbott's in stone: If it is old, it should look primitive, and vice versa. But there was one significant difference—the argument worked for Hrdlička, who was fast becoming the premier physical anthropologist of his day. Few could challenge his broad and deep knowledge of human variability and evolution. To claim a human skeleton was Pleistocene in age, one had to play by Hrdlička's rules.

Over the next two decades, the discovery of human bones in apparently Pleistocene age deposits at places like Lansing (Kansas), Gilder Mound (Nebraska), and Vero and Melbourne (Florida). In each case, descending on the site to inspect the skeletal remains and their geological context was a swarm of archaeologists and geologists, Hrdlička usually leading the pack (fig. 2.2).

In each case, Hrdlička challenged the claim that the bones were primitive enough to be old. And taking a cue from Holmes, Hrdlička suggested that since humans bury their dead and because bone is so easily broken and moved in the earth, the odds were that any bone in ancient deposits had arrived there fortuitously, redeposited from younger deposits (Hrdlička 1907, 1918). To this, paleontologist Oliver Hay, who had long battled Hrdlička over the issue of human antiquity from an office just down the hall from his, tartly replied, "Perhaps, we get a clue here to the reason why civilized people nail up their dead in good strong boxes" (Hay 1918:460; see also Meltzer 1983:32-33).

So it went for nearly four decades. Dozens of purportedly Pleistocene-age sites were championed—some with stone tools, others with human skeletal remains—but all were suspect, and all faced withering criticism from Hrdlička, Holmes, and others (e.g., R. Chamberlin 1917a, 1917b; Holmes 1899, 1902, 1918, 1925; Merriam 1914, 1924; for details on several of the individual sites and how they played out in the controversy, see Meltzer 1983, 1991b). In the end, all the claims were deemed unacceptable—not because of flaws in the geology of the sites, although such was criticized by others (e.g., Chamberlin 1902, 1919), but because all the remains seemed to Hrdlička (1903, 1907, 1918) to fit within the anatomical range of modern or historically known Native Americans. Hence, they had to be recent in age—regardless of the geological context, apparent fossilization, or other signs of great age.

"Facts are facts," Harold Cook had assured John Merriam (Cook to Merriam, January 1, 1929, JCM/LC), but Cook was wrong. Facts were not just facts: They were theory-laden and "controversy-laden" observations about the empirical realm (Rudwick 1985:431). The empirical evidence was never viewed in quite the same way by all who saw it.

Once again, the situation reached an angry impasse. Journalist Robert Gilder (1911), who found the Nebraska "Neanderthals," called Hrdlička a liar. Holmes (1925) snarled that the evidence from Vero was "dangerous to the cause of science." Archaeologist Nels Nelson thought it best to "lie low for the present" (Nelson to Hay, April 5, 1920, OPH/SIA). Shrewd advice, and many followed it.

Ultimately, the dispute over a deep human antiquity in America ranged widely over archaeological and nonarchaeological evidence and issues, led to fiery encounters at meetings and in print, and exposed irreconcilable differences between advocates and critics. In this wide-open field there were few rules of engagement, the controversy grew bitter, and it left lasting scars. When it was all over, Frank Roberts (1940:52) darkly admitted that "the question of early man in America [became over those decades] virtually taboo, and no anthropologist, or for that matter geologist or paleontologist, desirous of a successful career would tempt the fate of ostracism by intimating that he had discovered indications of a respectable antiquity for the indian." Or as A. V. Kidder (1936:144) put it, "[We] comforted ourselves by working in the satisfactorily clear atmosphere of the late periods."

Yet, for all its ambiguity and bitterness and after defying resolution for more than half a century, the controversy over human antiquity in North America simply vanished at Folsom in early September 1927. Paying no heed to his own advice, Roberts went there, and Alfred Kidder too. Folsom, they saw, gave American prehistory a significant time depth, "even by Old World standards," though it was no Paleolithic (Kidder 1936). Indeed, after decades of dispute, nothing ended up looking quite the way it had at the beginning.

Forerunners to Folsom: Two Creeks, One Toad

Cook and Figgins actively entered into the controversy over human antiquity not at Folsom but, earlier, at several other sites, including Snake Creek (Nebraska) and Lone Wolf Creek (Texas) sites, and, slightly later, at Frederick (Oklahoma). Snake Creek is actually a series of localities, one of which was the type site for the apparently Pliocene-aged Anthropoid ape, *Hesperopithecus haroldcooki*, and some comparably aged bone "artifacts." Lone Wolf Creek is a Paleoindian (Plainview age) bison kill. Frederick produced artifacts—including metates—in apparent association with mammoths. Both Cook and Figgins were involved at Frederick and Lone Wolf Creek; only Cook was involved at Snake Creek. These early episodes do not just mark their initial forays into the human antiquity controversy; they strongly influenced the reception accorded them at Folsom, and thus it is important to understand what came out of those sites, and what was said of them—by Cook, Figgins, and everyone else.

Snake Creek

Agate Ranch, Harold Cook's family homestead in western Nebraska, sits amidst one of the most spectacular paleontological localities in the world (part of which became, in 1965, the Agate Fossil Beds National Monument). In 1908, Cook, along with American Museum vertebrate paleontologist W. D. Matthew, discovered promising fossil exposures in the Snake Creek drainage 20 miles south of Agate. Within what they took to be Lower Pliocene units, they collected molars and premolars that showed "a startling resemblance to the teeth of Anthropoidea" (Matthew and Cook 1909:390; the Snake Creek formation is now identified as Miocene in age [see Skinner, Skinner, and Gorris 1977]). They were not altogether surprised. The associated fauna included antelope, which, in deposits of similar age in Europe, co-occurred with Anthropoid apes (Matthew and Cook 1909:390).

Cook collected at this locality intermittently over the next decade and, in February of 1922, sent a molar tooth he had found in the Upper Snake Creek beds to Henry Fairfield Osborn at the American Museum of Natural History. Cook thought the tooth "agrees far more closely with the anthropoid-human molar, than that of any other mammal known" (Cook to Osborn, February 25, 1922, cited in Osborn 1922:1; also Cook 1927b:115; see Skinner, Skinner, and Gorris 1977:277–278). Osborn (1922:1, 4) agreed, hastily identifying the specimen as "the first anthropoid ape of America" and naming it *Hesperopithecus haroldcooki*. More detailed studies by William K. Gregory and Milo Hellman at the AMNH seemed to confirm that the tooth's "nearest resemblances are with 'Pithecanthropus' and with men rather than with apes" (Osborn 1922:2; also Gregory and Hellman 1923a:14).

Osborn's identification of a new genus was not universally accepted. "Responsible critics" offered alternative suggestions of its possible affinities, ranging from bear to horse to monkey (Gregory 1927; Gregory and Hellman 1923b: 526). Obviously, more fossil material was needed.

The summer of 1922 Osborn sent Albert Thomson, his longtime field assistant, back to the "sacred ground" of Snake Creek (or so Osborn viewed it) in search of more of *Hesperopithecus*: "This animal will have a remarkable skull, and heaven grant that you may secure a bit of it" (Osborn to Thomson, June 27, 1922, HFO/AMNH, quoted in Skinner, Skinner, and Gorris 1977:281). But Thomson's efforts were stymied by an uncooperative landowner, so he went elsewhere in the Snake Creek Beds in hopes of finding *Hesperopithecus*. He had no luck (Cook 1927b:115; Skinner, Skinner, and Gorris 1977:281).

A quick surface survey in 1925 back at the type site, however, yielded another specimen (an upper molar), but again, permission to excavate was refused (Skinner, Skinner, and Gorris 1977:282). However, in 1925 and 1926 Thomson, occasionally joined by Osborn, Gregory, and Barnum Brown, collected other material, which largely convinced Gregory that *Hesperopithecus* was actually *Prosthennops*, an extinct peccary (Osborn to Cook, January 27, 1926, HJC/AGFO; Gregory 1927:580). The case was sealed the summer of 1927, when Thomson's excavations at the type site recovered abundant *Prosthennops* fossils—*haroldcooki* had been a gigantic taxonomic blunder (Gregory 1927:518).

Nonetheless, in 1925, just as Gregory was beginning to have doubts about *Hesperopithecus*, radically different evidence of early humans was apparently recovered from the Snake Creek beds (Cook 1927b:115–116; Skinner, Skinner, and Gorris 1977:282). Cut, shaped, and drilled bone had been found by Thomson, which Cook and Osborn believed to be genuine Pliocene artifacts. These "artifacts," Cook admitted, were found amidst a large number of broken bones, and there was no "question *at all* that a great deal [of] natural wear, breaking and erosion has also taken a hand" (Cook to Osborn, January 8, 1926, HJC/AGFO; emphasis in original). But Cook insisted that not every specimen was the result of natural causes:

> Of course Thomson has a whole lot of doubtful scrap. Some of it may be artifacts, some probably is not. But excluding all the doubtful things, he still has a nucleus of material in which the evidence, viewed without bias, is so strong that it requires a straining stretch of a negatively inclined imagination to account for the conditions found in any way but the one which says "Made by Hand." (Cook to Osborn, January 8, 1926, HJC/AGFO; also Cook to Ingalls, Febuary 27, 1928, HJC/AHC)

In structure, this argument is little different from the one used half a century later to defend the authenticity of "artifacts" from the Calico and Old Crow sites (Irving 1985; see also Irving, Joplin, and Beebe 1986:59–60; Simpson, Patterson, and Singer 1986:99).

The other paleontologists at the American Museum were "very skeptical" about the Snake Creek "artifacts." Osborn advised Cook to proceed "with extreme caution, because it will not do to take up a position and then later be obliged to back down and apologize" (Osborn to Cook, January 27, 1926; also Osborn to Cook, May 16, 1927, HJC/AGFO).

Osborn followed his own advice. He would never publish on the Snake Creek "artifacts," but he did speak to them in Philadelphia at the December 1926 AAAS meeting and, again, the next spring at the New York Academy of Sciences. Cook was bolder, marshaling bits of *Hesperopithecus* and the Snake Creek tools into a *Scientific American* article on human antiquity in America, which took "rather sharp issue" with an article published in that same journal just five months earlier by Hrdlička (Cook to Barbour, October 3, 1926, EHB/NSM; Cook 1926; Hrdlička 1926). Cook's bravado was in part fueled by what he believed was corroborative evidence from the site of Lone Wolf Creek.

Lone Wolf Creek

In 1924 Nelson Vaughan, a fossil and artifact collector, wrote Figgins of his discovery of a "huge skeleton" of bison eroding out of the banks of Lone Wolf Creek, just outside of Colorado City, Texas (Vaughan to Figgins, April 1924, JDF/DMNS; cf. Figgins 1927a:229). Figgins was interested in obtaining the fossil for the Colorado Museum and hired Vaughn and rancher H. D. Boyes to collect the specimen. Since neither had training in paleontology, Figgins sent a letter instructing them on how to excavate and crate the specimen (Figgins to Vaughan, April 28, 1924, JDF/DMNS). The goal, as it often was for Figgins, was to "secure fossils of a character that can be mounted, that is, nearly complete skeletons," for such was Figgins' gauge of whether a fossil was valuable (Figgins to Vaughan, May 9, 1925, JDF/DMNS; Cook to Osborn, July 7, 1927, HFO/AMNH).

Figgins would soon regret his decision to supervise the excavations long distance. Vaughan and Boyes dug out the bison in the early summer of 1924 (fig. 2.3) but made a mess of it, failing to apply enough shellac to stabilize the bone. Worse, in the process of crating the fossil for shipping, Boyes took the bone-bearing slabs and

> trimmed [them] down to make them thin enough to go into the crates, instead of making deeper crates. This resulted in the processes being destroyed on a whole series of vertebra. (Figgins to Vaughan, March 31, 1925, JDF/DMNS).

So, instead of having multiple complete skeletons of what appeared to be an extinct, possibly Pleistocene-age species, the Museum "had but one and that had to be restored in many places" (Figgins to Vaughan, March 31, 1925, JDF/DMNS).

Less fortunate still, Figgins learned only afterward that three projectile points had been found with the bison skeleton. The artifacts had not been left in place or photographed, and only two of them actually reached Denver. The third was lost, or possibly stolen (Figgins to Hay, March 11, 1925, JDF/DMNS; Figgins to Vaughan, March 16, 1925, JDF/DMNS; Figgins 1927a:231). Boyes was suspected.

FIGURE 2.3 H. D. Boyes and Nelson Vaughn at Lone Wolf Creek, Texas, 1924. Note the bison remains in place. (Photo courtesy of Heart of West Texas Museum.)

Figgins grumbled that Boyes was just an "ignorant rancher" who felt he was there merely for the purpose of "getting fossils" and had failed to appreciate the site's archaeological significance (Cook to Hay, December 23, 1926, OPH/SIA; Figgins to Hay, December 21, 1926, OPH/SIA; Figgins to Vaughan, January 11, 1927, JDF/DMNS). But in truth, "getting fossils" was precisely why Figgins had Boyes and Vaughan excavating there. For that matter, Boyes may well have appreciated the significance of the artifact, as he had his own "small personal collection of arrowheads." Figgins suspected that Boyes pilfered the third Lone Wolf Creek artifact (cf. Figgins to Hay, December 21, 1926, OPH/SIA, and Vaughan to Figgins, January 27, 1927, JDF/DMNS).

Regardless, the pressing issue facing Figgins was whether the artifacts had been associated with the fossil bison, and he found himself in the awkward position of building the case for Lone Wolf Creek's antiquity, long after the evidence was out of the ground. Neither Figgins nor Cook believed that Boyes or Vaughan had either motive or knowledge to fake an association of points and fossil bison