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FROM PALEOLITHS TO PALEOINDIANS

In the fall of 1781, the governor of Virginia received a letter from the secretary of the French Legation, inquiring about the political institutions, natural history, and native peoples of his state. Times being what they were (hostile British forces were advancing on the governor's home) his answer was postponed until the following summer, after Cornwallis had surrendered at Yorktown and the governor had retired from office. "Great question has arisen," Thomas Jefferson then replied in his now-classic *Notes on the State of Virginia*, "from whence came those aboriginal inhabitants of America." For that matter, who were they, and when had they arrived?¹

These questions were deeply unsettling to a nation that viewed the past through a biblical lens, and which well realized American Indians were not mentioned in the chronicles of Moses. Still, on the presumption of monogenesis, the idea that "all the varieties of the human race were descended from a single pair, and that after the flood the earth was indebted solely to the ark of Noah for the replenishment of man and beast,"² American Indian origins were sought among historically known or imagined groups, among them wandering Egyptians, Phoenicians, Mongols, Welsh, Hindus (those *other* Indians), survivors of the Lost Continents of Mu or Atlantis, and the Ten Lost Tribes of Israel. The possibility that American Indians were Israelites had two great virtues: it explained who the Indians were, and where the Israelites had been lost all those years.

Yet few capable thinkers, and Jefferson was an extraordinarily capable thinker, paid these any mind. Diverse as the Native Americans were, Jefferson realized they

shared a common ancestry. Judging by their appearance, he supposed their ancestral homeland must have been eastern Asia. That being the case, how had they reached the New World? The answer, anticipated in the late sixteenth century by the Jesuit José de Acosta, was that they came by land, possibly via northern Asia and northwestern North America. But it was not until 200 years later that Jefferson was able to report that “the late discoveries of Captain Cook, coasting from Kamchatka to California, have proved that, if the two continents of Asia and America be separated at all, it is only by a narrow streight [sic].”³

Having found an easy migration route from the Old World to the New still left unsettled the question of when that migration had taken place. Jefferson had his suspicions. Native Americans spoke a Babel of languages (few of which were mutually intelligible or bore any resemblance to an ancestral Asian language), they did not all look alike—the usual stereotypes notwithstanding—and they had diverse cultural practices, all of which implied a long period of divergence from what was assumed to be a common ancestor. Could this variability have arisen in the biblically allotted 6,000 years? Jefferson was doubtful: such linguistic, physical, and cultural divergence from a common source seemingly required “an immense course of time; perhaps not less than many people give to the age of the earth.”⁴

As old as the earth? Jefferson’s political enemies howled, accused him of heresy, and branded him an atheist. The earth itself could turn out to be very old, perhaps tens of thousands of years old as astronomers and geologists were already insisting, but Jefferson’s critics demanded human history go no earlier than the 6,000 years allotted in the scriptures. And it didn’t.

At least not for another seventy-two years.

THE DISCOVERY OF DEEP TIME

Before 1859, the Bible was history, chronology, and ethnography all rolled into one: it was a detailed and sacred account of human genealogy and lifeways from Adam and Eve on down. Compiled by people who had either been present or had access to a supernatural informant, it linked modern life back to the very creation of heaven and earth. Well, almost to the Creation.⁵

By the late seventeenth century, scientists such as Isaac Newton began lobbying for more time, at least for earth history. They knew from physical evidence the earth had to be more than 6,000 years old, but not daring to reject Genesis entirely, they placated themselves by treating the first days of Creation as allegorical only, supposing that Moses was simply “accommodating his words to the gross conceptions of the vulgar,” as Newton put it, and that the Biblical chronology “relates only to the human race.” This was an argument easy to make. After all, as Edmund Halley (of comet fame) observed, if the sun was created only on the fourth day, how could the time before then be measured in “days” in the literal twenty-four-hour sense?⁶

So geologists got busy. From the strange fossils brought to light by exploring expeditions and the rapacious mining that fueled the Industrial Revolution, Georges Cuvier and others revived the wonderfully exotic plant and animal life of earlier periods of earth history. The most recent of those earlier periods was inhabited by fossil elephants, including North America's mastodon and mammoth. Cuvier had it on the good authority of American Indians—"nomadic peoples who move ceaselessly around the continent in all directions" and thus were in a good position to know—that these "creatures no longer existed."⁷

The "unconsolidated . . . layers of the earth" in which the fossils of these animals were found lay close to the surface, in what appeared to be sand and gravel laid down by water—Buckland's diluvium. It would be several decades before those deposits were recognized as the residue of once-vast continental ice sheets (Chapter 2) and, in turn, linked with Lyell's Pleistocene. Nonetheless, even by the early 1800s, Cuvier realized these were not truly ancient deposits, for there were still more primitive fossil elephants and other animals in deeper layers of the earth. He concluded that earth's history was one of multiple periods of creation of different kinds of animals, each of which came to a cataclysmic end. The animals of the diluvium represented merely "the last or one of the last catastrophes of the globe." But if these animals were not old in geological time, they were assuredly from the pre-modern world. The Bible made no mention of mammoths.⁸

Accordingly, no one was looking for human remains alongside mammoth fossils in Pleistocene deposits, nor expected to find any. This was a period unknown to human history, beyond range of the Mosaic chronicles, and thus a baseline against which human antiquity could be measured. The uncoupling of earth history from human history was a splendid compromise in a world fast becoming inconceivably old: humanity's best hope for divine origins and its own uniqueness in the animal kingdom lay in the affirmation of the Bible. Here, fortunately, Genesis and geology seemed to agree: humans were the most recent creation, or so it seemed on good authority.⁹

Still, through the first half of the nineteenth century, human remains were found alongside Cuvier's extinct animals at an increasing number of sites. But why accept those finds at face value?¹⁰ Most came from continental Europe and especially France, and for this reason immediately lost credibility with the more theologically conservative British who, since the French Revolution, had suspected the French of atheism and harbored a lingering distrust of their latter-day Enlightenment notions. Much of the evidence came from excavations by provincial amateurs who were looked upon, in John Lubbock's charitable Victorian parlance, as mere "enthusiasts."¹¹ They were hardly a trustworthy source of evidence on such a momentous question. No scientist was going to reject long-held beliefs on their say-so.

Compounding resistance, the bulk of finds were made in caves, settings regularly churned by burrowing animals that mix deposits of different ages. Caves are ill suited to demonstrating whether artifacts found alongside animal bones were the same age, or just accidentally associated. When the "Red Lady" of Paviland, an ocher-covered human

skeleton, was found in a cave with the remains of a mammoth, it was all too easy for the Reverend Buckland to explain (with a knowing wink) that a nearby Roman camp site threw “much light on the character and [antiquity] of the woman under consideration.” Ocher apparently wasn’t the only reason Buckland supposed the Lady of Paviland was red (as it happens, she was no lady either, but an Upper Paleolithic male, now radiocarbon dated to slightly over 26,000 BP).¹²

Finally, none of the evidence fit prevailing expectations of human history based on the Bible or geology, and thus there was no compelling reason to accept it. This isn’t dogmatic; it’s good science: one doesn’t reject a long-established worldview without compelling reason. As Lyell later confessed, “I can only plead that a discovery which seems to contradict the general tenor of previous investigations is naturally received with much hesitation.”¹³ Only when the counterevidence becomes too weighty to ignore does the model of the world get reassessed and, if needed, rejected.

That reassessment began in the summer of 1858 at Brixham Cave in southwestern England where, beneath a nearly impenetrable layer, a crack team of excavators under the supervision of Britain’s finest geologists (Charles Lyell among them) had uncovered stone tools in direct association with Pleistocene fossils. Either the team had failed to observe that stone tools had been mixed in from higher, younger deposits and had botched the excavation (a conclusion no one was keen to admit), or the theologically disconcerting was true: humans had occupied Brixham Cave during the Pleistocene, alongside now-extinct animals, and thus had an ancestry that predated biblically recorded history.

Brixham Cave’s revelations prompted another look at the long-standing claims of Jacques Boucher de Perthes, who for decades had been collecting stone tools and Pleistocene fossils in the Somme Valley of northwest France. Until that moment, he’d been mostly ignored because many of the “artifacts” he illustrated were clearly not artifacts at all, and what genuine evidence he did have was embedded in arcane theories that had long since been rejected. Charles Darwin was hardly alone in reading Boucher de Perthes’ work and concluding “the whole was rubbish.”¹⁴

Yet, some of the stone tools Boucher de Perthes illustrated looked a great deal like those from Brixham Cave and, better, had come from floodplain deposits, which are less prone to the mixing of deposits that occurs in caves. A procession of geologists and antiquarians made the pilgrimage to see Boucher de Perthes and his sites and collections, including Joseph Prestwich and John Evans, who in April 1859 witnessed and photographed a handaxe in situ at a locality in Amiens.

At a meeting of London’s Royal Society a few weeks after their return, Prestwich read a paper on the glacial age and stratigraphy of the Somme Valley gravels, and Evans spoke extemporaneously on the stone artifacts found in them. Listening to them, Evans recalled, “were a good many geological nobs . . . Sir C. Lyell, Murchison, Huxley . . . Faraday, Wheatstone, Babbage, etc. so [we] had a distinguished audience.” That’s an understatement. The audience included not only the geological elite (Lyell, Thomas

Henry Huxley, Roderick Murchison), but also one of the greatest experimental scientists of all time (Michael Faraday); the inventor of the stereoscope and a pioneer researcher in acoustics, electricity, and telegraphy (Charles Wheatstone); and one of the trio who revolutionized English mathematics in the nineteenth century and a dabbler in crypt-analysis, probability theory, geophysics, astronomy, and computing machines (Charles Babbage). It might not have been possible to gather in one place a more influential group of scientists in all of England. The question of a deep human antiquity was out of the closet and on center stage, and the favorable reception their audience accorded Prestwich and Evans ("Our assertions as to the findings of the weapons seemed to be believed"⁵) immeasurably helped the cause.

In the summer of 1859, Charles Lyell himself, foremost among those who had long challenged all claims of great human antiquity, made the pilgrimage to Abbeville. He too returned from France a convert, announcing, "I am fully prepared to corroborate the conclusions which have been recently laid before the Royal Society. . . . I believe the antiquity of the Abbeville and Amiens flint instruments to be great indeed if compared to the times of history and tradition."¹⁶ Humans had seen Agassiz's glaciers and preyed on Cuvier's fauna.

Only months before, the idea of a deep human antiquity had been the dubious claim of provincial amateurs. Now, it was almost universally accepted fact. As one contemporary joked, people were no longer insisting "it was not true" or that "it was contrary to religion," but that "it was all known before."¹⁷

The Paleolithic, or Stone Age, occupants of Brixham Cave and the Somme Valley predated history, and lived at a distant time "when man shared the possession of Europe with the Mammoth, the Cave bear, the Woolly-haired rhinoceros, and other extinct animals," using "rude yet venerable weapons" of stone, as John Lubbock put it.¹⁸ This was a human past about which the Bible said absolutely nothing, and which suddenly rendered it obsolete as the story of humanity's past. The Paleolithic would be knowable only through its silent artifacts and skeletal remains. To investigate that past required a new discipline of *prehistory*, with its own body of theory and methods, kin to geology and not sacred history.

As dozens more Paleolithic sites were found across Europe, humanity's roots were pushed back ever deeper in time: hundreds of thousands of years, some suggested, perhaps millions of years, others supposed. What had transpired over that span was poorly known, testament to the vast "chasm which separates the flint folks from ourselves." Julia Wedgwood, Darwin's niece, saw "something dreary in the indefinite lengthening of a savage and blood-stained past,"¹⁹ but however dreary or theologically unnerving, those rude artifacts were nonetheless vivid testimony of the savage depths from which humanity had climbed. They became for many Victorians a triumphant demonstration of social progress.

The demonstration of a deep human antiquity came at virtually the same moment as Darwin published his *Origin of Species*, which laid out the theory of evolution by natural

selection. Although the two had independent origins, they soon and ever after were linked. The *Origin* had no more to say about the evolution of the human species from our animal forebears than the understated one-liner, “Light will be thrown on the origin of man and his history.” But everyone knew what *that* meant: ancient human ancestors and ultimately a pedigree shared with other primates. One could scarcely accept the implications of Darwin’s views of human evolution without the deep past that prehistory provided. Darwin well recognized that, and a decade later when he finally tackled the topic of human evolution, he knew precisely where to start: “The high antiquity of man has recently been demonstrated by the labors of a host of eminent men, beginning with M. Boucher de Perthes; and this is the indispensable basis for understanding his origin. I shall, therefore, take this conclusion for granted.”²⁰ It was rubbish no more.

And that’s when the search began on the other side of the Atlantic.

THE RISE AND FALL OF THE AMERICAN PALEOLITHIC

The discovery of the European Paleolithic fired American scientific imaginations: why couldn’t American prehistory be just as old? After all, the geology of the two continents seemed so very similar. But archaeologists quickly realized that here in America there were no deeply stratified river valleys or caves with human artifacts indiscriminately mixed with Pleistocene fauna.²¹

No matter, they soon had another inspiration: if artifacts were ancient, they ought to look the part, so ones similar to the tools of Paleolithic Europe must be the same age. The catalyst for these studies was Charles Conrad Abbott, a New Jersey physician (and coincidentally, nephew of Timothy Abbott Conrad, who first found traces of the Ice Age in America). By all accounts—including his own—Abbott had a dreadful bedside manner and was unable to earn a living as a physician. But a living had to be made, for although he came from a prominent, land-holding family, his was not a wealthy branch of the family tree. Abbott made a half-hearted stab at farming and the occasional odd job to help support himself and his family, but his livelihood came to revolve around authoring popular books on the natural history and archaeology of the Trenton area. Abbott soon realized the artifacts he was finding near his Trenton home appeared to be as “rude” as those of the European Paleolithic (Figure 10). Better still, some came from geological deposits that hinted of considerable antiquity, and perhaps they were even Pleistocene in age.

We now know, based on archaeological work done at the Abbott farm since, the artifacts he was finding were not very old at all, and certainly not glacial in age. But Abbott didn’t know that, nor did most of his peers. In February 1877, Harvard University geologist Nathaniel Shaler studied the Trenton ground with Abbott and pronounced the artifact-bearing gravels to have been deposited during the Ice Age. In his diary that night, Abbott recorded his triumph: “*I have discovered glacial man in America*”²² (emphasis in the original).



FIGURE 10.

Charles Abbott searching for paleoliths in the Trenton gravels, New Jersey, ca. 1880s. (Photograph courtesy of the Peabody Museum, Harvard University. © Harvard University, Peabody Museum 2004.24.31046.)

Heady stuff, this, and it inspired Abbott's compatriots. Harvard archaeologist Frederic Ward Putnam hired a local man to walk the freshly dug railroad right-of-ways and sewer trenches of Trenton watching for more "Paleolithic" artifacts. As Abbott's work in the Delaware Valley gained prominence, so, too, did the American Paleolithic. Throughout the 1880s, reports came in of other paleoliths from the eastern seaboard into the Upper Midwest. Some were found under geological circumstances that suggested a Pleistocene antiquity, but even in the absence of secure geological evidence, these artifacts so readily mimicked European paleoliths of undeniable antiquity, they must be as old. As Reverend George Frederick Wright, an Oberlin College theologian and geologist, put it, in Paleolithic times, as today, American fashions followed the Paris line.²³

By the end of the 1880s, Abbott and his colleagues possessed the proof they needed that the first Americans had arrived thousands of years ago, when northern latitudes lay shrouded in glacial ice. The American Paleolithic spawned a formidable literature of symposia, feature articles, and books. Abbott was lionized here and abroad as "America's Boucher de Perthes." Everyone believed the first Americans had arrived in the Pleistocene; the only question remaining was how much further back in time that prehistory extended. It had to be deep, Abbott insisted, for were it only 10,000 years old, he would be "compelled to crowd several momentous facts in American archaeology into a comparatively brief space of time."²⁴

But the consensus surrounding the American Paleolithic proved short-lived. Scarcely a year later, it was under withering fire from USGS geologists and archaeologists at the Smithsonian Institution's Bureau of Ethnology (later, the Bureau of American Ethnology,



FIGURE 11.

William Henry Holmes sitting in “an ocean of paleoliths” at the Piney Branch quarry, in Washington, D.C., ca. 1890. Holmes was only kidding about those being paleoliths. He used the cobbles and workshop debris from Piney Branch to show that the “primitive-looking” and supposedly ancient artifacts of the American Paleolithic were mere quarry refuse. (Photograph courtesy of the Smithsonian Institution Libraries, Washington, D.C.)

or BAE). By late 1889, the archaeological community was humming with rumors that BAE archaeologist William Henry Holmes’s excavations at a prehistoric stone quarry along Piney Branch Creek in Washington, D.C. (Figure 11), were set to demolish the conceptual foundations of the American Paleolithic.

At the Piney Branch site (which, astonishingly, remains intact just a few miles north of the White House), prehistoric groups found quartzite cobbles suitable for making their artifacts. Scattered across the site surface were the debris of tool manufacture, ranging from cobbles with just a few flakes struck from them, to nearly finished items that had broken or otherwise been rejected in the final stages of preparation. Many of the rejects bore an uncanny resemblance to Paleolithic artifacts. Holmes had an *aha!* moment: the manufacturing sequence at Piney Branch mimicked the long evolution of stone toolmaking from primitive to refined. Thus, just as the most ancient stone tools were little more than barely sharpened cobbles, so, too, were stone tools in the early stages of manufacture. That being the case, he reasoned, an artifact jettisoned prematurely during manufacture would naturally resemble a primitive stone tool, even if it was scarcely a few hundred years old.

In late January 1890, Holmes hurled his opening salvo in the *American Anthropologist*: artifact form had no inherent chronological significance.²⁵ Proponents of an American Paleolithic were mistaking primitiveness for antiquity, failing to realize their artifacts were merely manufacturing failures. An artifact's age, he insisted, must be determined by its geological context—and not because it happened to look like one illustrated by Boucher de Perthes or John Evans. That included Abbott's Trenton paleoliths, which to Holmes's eye looked very much like manufacturing failures. Still, at that moment, he was feeling charitable, and he wasn't suggesting the Trenton specimens were recent in age. Not yet, anyway.

Word soon reached Abbott of Holmes's paper, and he dashed off a letter to Henry Henshaw, editor of the *American Anthropologist*, requesting a copy. He got one, along with an invitation. Would Abbott like to come to Piney Branch, preferably immediately? "It seems to me," Henshaw wrote, "that you would be particularly interested in the matter since you have done so much work at Trenton, and a visit here just now could not fail to prove instructive. Mr. Holmes would be very glad to see you, and we will all do what we can to make your visit pleasant and instructive."²⁶

"Instructive," Henshaw said. Twice. But Abbott wasn't looking for instruction. He was looking for confirmation. But he went to Washington anyway, and toured Piney Branch with Holmes in early 1890. A smug Holmes later recalled that

on parting Abbott said, "I have learned more arch[aeology] in three hours than ever before in three months." This I was content to think of as a pleasant compliment but from my subsequent studies and increased wisdom I concluded that he probably meant what he said.²⁷

If Abbott indeed said that, he hardly meant it, for he left Washington unswayed. As he later wrote, Holmes may well have been correct in his archaeological interpretation of Piney Branch, but Abbott failed to see that the story at Piney Branch had any relevance to either Trenton or the larger question of human antiquity in America. After all, Holmes's "so-called failures" were "not identical with the true American Paleolithic implements of the Delaware River Valley," which were found in what Abbott deemed undisturbed Pleistocene glacial deposits. Even if geologists did quibble over their precise age, Abbott was sure "no verbal jugglery" could make the Trenton gravels that much younger.²⁸ If Holmes aimed to reject Trenton as a Paleolithic site, he would have to go to Trenton.

The lines were drawn in the sand (and gravel). The Great Paleolithic War began.

Holmes spent the next several years examining the alleged Paleolithic sites in eastern North America, including Trenton.²⁹ In each case, Holmes solemnly proclaimed that mistakes had been made. All alleged paleoliths were manufacturing failures having no appreciable antiquity, and were merely the debris of historically known Native Americans. None, in his view, were Pleistocene in age, and those allegedly found in glacial deposits must have fallen down rodent burrows or cracks in the earth, and

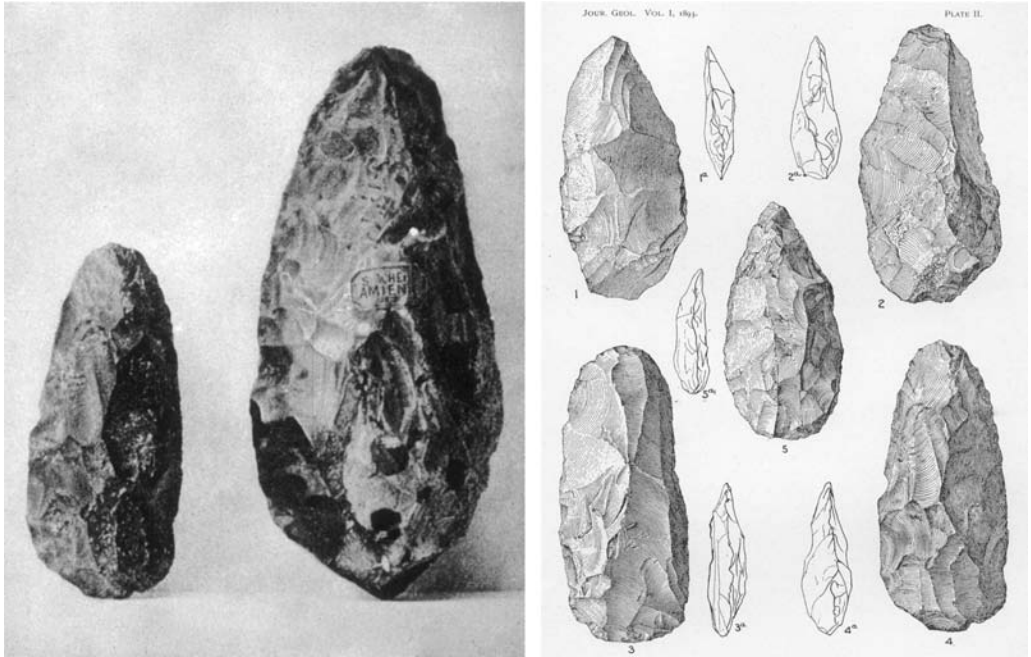


FIGURE 12.

Two perspectives on the Newcomerstown, Ohio, "paleolith." Left, G. F. Wright's (1890) composite of the Newcomerstown paleolith alongside a European paleolithic biface (reduced to one-half size) from Amiens, France. Right, Holmes's (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.

fortuitously settled in those older deposits (Figure 12). And if the specimens were actually in situ in "Pleistocene" gravels, Holmes could say, on the supreme authority of the USGS's Thomas Chamberlin and WJ McGee, that those gravels were not Pleistocene age at all.

Abbott was furious. If the critics were correct, why were only primitive-looking specimens found in gravel deposits, and not the more refined artifacts of the American Indians? If these were quarry rejects, where was the flake debris resulting from their fashioning? And who were these self-styled "expert geologists" to lecture *him* about the Trenton gravels? None had spent a fraction of the time he had walking the banks of the Delaware, and assuredly could not have greater knowledge of its geology than he possessed. "I lay a claim to a smattering of gravel-ology," Abbott declared in *Science*, and if "up pops some 'authority' and declaims the possibility that the ground was washed from beneath the big stone and the implement slipped in. Well, we can go on supposing till the crack o'doom, but as to proof, that is another matter."³⁰

Perhaps. But as Holmes snorted in reply two weeks later, demonstrating that artifacts were truly found in glacial gravels required the skills of “competent and reputable observers of geological phenomena.” That Holmes believed no glacial-aged artifacts had yet been found made it quite clear he did not consider Abbott’s claims to “gravel-ology” equal to competence. A testy Abbott immediately fired back with doggerel virtually guaranteed to offend the humorless Holmes:³¹

The stone are inspected,
And Holmes cries “rejected,
 They’re nothing but Indian chips”
He glanced at the ground,
Truth, fancied he found,
 And homeward to Washington skips.

They got there by chance
He saw at a glance
 And turned up his nose at the series;
“They’ve no other history,
I’ve solved the whole mystery,
 And to argue the point only wearies.”

But the gravel is old,
At least so I’m told;
 “Halt, halt!” cries out WJ [McGee],
“It may be very recent,
And it isn’t quite decent,
 For me not to have my own way.”

So dear WJ
There is no more to say,
 Because you will never agree
That anything’s truth
But what issues, forsooth,
 From Holmes or the brain of McGee.

Holmes was not amused (though the editor must have been, since it was highly unusual for *Science* to publish verse). The American Paleolithic, he snarled, was little more than the blunders and misconceptions of “amateurs” with little scientific understanding of stone toolmaking, let alone of geological age and context. If Abbott still didn’t get the message, Holmes spelled it out in a lengthy, sharply pointed critique,

which asked the decidedly nonrhetorical question, “Are there traces of man in the Trenton gravels?” Holmes’s answer was “No.”³² And then the controversy turned ugly.

In the fall of 1892, USGS and BAE scientists attacked George Frederick Wright’s just-published *Man and the Glacial Period*. Wright’s wrongs were obvious enough: he had spoken in favor of the American Paleolithic; he had challenged one of Chamberlin’s (and by extension the entire USGS Glacial Division’s) intellectual monuments—the demonstration there had been multiple glacial events; and, worst of all, he had put those heresies in a book written for a general audience. Wright’s critics, orchestrated by Chamberlin, set out to destroy his credibility as a glacial geologist, as an archaeologist, and especially as a public spokesman for science. They launched a barrage of vicious reviews of *Man and the Glacial Period*, which were unprecedented in number and savagery. “No one,” Chamberlin thundered, “is entitled to speak on behalf of science who does not really command it.” WJ McGee, a one-time staunch supporter of the American Paleolithic but by now a zealous convert, was especially bloodthirsty, labeling Wright’s work absurdly fallacious, unscientific, and an “offense to the nostrils,” then dismissing him as “a betinseled charlatan whose potions are poison. Would that science might be well rid of such harpies.”³³

The maliciousness of the attacks appalled Wright’s colleagues, as well as many who hardly knew him or his work. To Wright’s allies, the near-simultaneous appearance of the reviews and their “sameness of tone” smelled of a conspiracy. They were right. It was. Yet, the assault on Wright was more than just personal. It was a thinly veiled proprietary dispute in which BAE and USGS scientists sought to impose their vision of archaeology and geology on those fields, and contrast their brand of science against those, such as Wright and Abbott, they deemed rank amateurs.

Because the BAE and USGS scientists were richly funded and backed by the power of the federal government, at a time when the government dominated American science (the balance of scientific research and power would shift to universities only in the twentieth century), the atmosphere was charged with accusations that arrogant, heavy-handed federal scientists were conspiring to crush state and local practitioners. Those accusations reached Capitol Hill in 1893, when Congress was deciding the annual budgetary fate of the USGS and BAE. Neither agency could afford such bad press, since their appropriations were already threatened by the economic fallout from the Panic of 1893. Paleolithic proponents got a measure of revenge: the BAE and USGS budgets were slashed.

After savaging each other in meetings and in print, the warring parties suspended hostilities in late 1893, though more from battle fatigue than by truce, for both sides had hardened beyond compromise or retreat. They met again in the summer of 1897 in Toronto at a joint meeting of the American and British Associations for the Advancement of Science. There, Paleolithic proponents suffered a devastating blow: Putnam handed Sir John Evans a set of Trenton artifacts. With barely a glance, the dean of Stone Age archaeology dismissed the lot as not Paleolithic at all. Putnam desperately tried to

A MAMMOTH FRAUD IN SCIENCE

The boldface headlines of the September 8, 1894, *Philadelphia Inquirer* nearly screamed aloud: “DR. HILBORNE CRESSON TAKES HIS OWN LIFE.” As the paper indelicately put it, Cresson “blew his brains out in a park in New York City.” He had gone insane, the *Inquirer* reported, his mind disordered as a “result of scientific study” and the “too close application of esoteric principles.”

Cresson was an archaeologist.

He began his career an expatriate student of art and archaeology at the *École des Beaux Arts* and *École d'Anthropologie* in Paris in the 1870s. Apparently, he had a talent for recreating prehistoric art. He returned to the United States in 1880 and began collecting artifacts and excavating around Philadelphia. By 1887 he'd attracted the attention of Frederic Ward Putnam, who hired Cresson as an archaeological field assistant.

In December 1889, Cresson appeared in Putnam's office bearing a whelk (*Busycon*) shell pendant, on which was engraved the figure of a mammoth (Figure 13).



FIGURE 13.
The Holly
Oak pendant
(Delaware), and
as drawn by
W. C. Sturtevant.
Note that there is
ample room on
the shell for feet;
that they are not
shown was one
of the hints that
the mammoth
image was made
from a book, and
not from real
life. (Adapted
from Griffin et al.
1988.)

Cresson reported he'd dug up the specimen as a schoolboy years earlier near Holly Oak, Delaware. Putnam, a staunch champion of the American Paleolithic, would have instantly understood the pendant's significance: if the mammoth had been engraved from life, it would be a tidy solution to the question of human antiquity in the New World, and would show that Paleolithic Americans had created works of art to rival in age and beauty those of Paleolithic Europe. How timely that Cresson should appear with a specimen that could tip the balance of the Great Paleolithic War, which Putnam knew was looming on the horizon.

Even so, one searches Putnam's papers in vain for the happy announcement his cherished beliefs had been vindicated by this remarkable evidence. At Cresson's request, Putnam showed the Holly Oak pendant at the February 1890 meeting of the Boston Society of Natural History, yet was ever after silent about it. But Putnam left behind a telling clue of his opinion: a photograph of the Holly Oak pendant alongside a drawing of the La Madeleine mammoth, which had been engraved on a segment of mammoth tusk that was discovered by Eduoard Lartet in 1865 in the Dordogne region of France.

Any good archaeologist, and Putnam was a good archaeologist, would have instantly seen the similarities between the Holly Oak and La Madeleine engravings. To be sure, in Putnam's day there were few mammoth depictions known from Europe, and none from America. Possibly, he may have concluded the similarity between the two merely reflected the fact both depicted mammoths, as opposed to both depicting the *same* mammoth (nowadays we know of thousands of European Paleolithic depictions of mammoth—still none from America—and those depictions vary greatly, as one might expect).

Putnam's suspicions might have been further aroused when Cresson reported his discovery had been made in 1864, conveniently predating La Madeleine's by a year, and claimed that his French tutor, allegedly involved in the Holly Oak discovery, had been a student of Lartet's. Cresson never satisfactorily explained why it took him twenty-five years to reveal the pendant's presence.

Cresson suggested to Putnam that he look at Charles Rau's book, *Early Man in Europe*, to see how well the Holly Oak mammoth resembled the mammoth skeleton it illustrated. In doing so, Putnam would have seen that the book also provided an illustration of the La Madeleine engraving, one that differs from better reproductions of the original in ways strikingly similar to Holly Oak: the contours of the back and tusks and trunk can be overlaid; they have the same orientation and overall posture, especially the leg positions; they treat the feet in a similar way. Most mammoths are shown with bulbous feet, but the La Madeleine feet are cut off by a break in the original specimen, and the Holly Oak feet terminate in precisely the same way, despite the fact that there is ample room on the shell where

feet could have been engraved. Putnam, almost certainly suspecting the Holly Oak pendant was a fraud, quietly ignored it.

For that matter, there was hardly any mention of the pendant in the thousands of pages published during the Great Paleolithic War. That silence was more damning than even the rare voice condemning the pendant, that of archaeologist Henry Mercer who, busy peddling his own mastodon-engraved forgery, sneered that Holly Oak was a fraud and Cresson a liar. Since the archaeological community in those days was so very small, its members must have known of the pendant. One can safely surmise they doubted its authenticity, and were just too polite to say anything about it. And why bother? Everyone knew the story.

Unfortunately, that story died with its participants, though the pendant itself survived in obscurity in a drawer at the Smithsonian Institution, only to emerge in 1976 on the cover of *Science*.³⁴ An accompanying article by John Kraft and Ronald Thomas reported that geological work in and around what is now Claymont, Delaware, revealed deposits that were at least 10,000–40,000 years old. They asserted, in the absence of any evidence that the pendant actually came from those deposits, it must be just as old, thereby making it “definite evidence of association of early American man with the woolly mammoth” and the only known example of North American Paleoindian art.

The *Science* cover caught the eye of Bill Sturtevant, a curator at the Smithsonian, who was instantly reminded of the La Madeleine mammoth. When he read the pendant came from Delaware, not the Dordogne, he straightaway suspected fraud. Knowing of my interests in Paleoindians and the history of American archaeology, he asked me to help investigate.

We quickly established there were concerns about unrelated but equally suspicious archaeological discoveries of Cresson’s that, like Holly Oak, could never be field checked. We learned, too, Cresson was not trusted by his peers, and for good reason. In late 1891, Cresson was working in Ohio on Putnam’s archaeological field crew when his supervisor, Warren Moorehead, caught him stealing artifacts from the excavations and shipping them to his home to Philadelphia. Cresson was fired on the spot.

Reading the details of Cresson’s pilfering in Moorehead’s diary, I was reminded of a comment made to me a few months earlier by James B. Griffin, one of the deans of American archaeology. Griffin thought it odd that the Holly Oak pendant, ignoring its engraved mammoth, bore such a striking resemblance to shell pendants found in Fort Ancient period sites, which generally postdate AD 1000. In virtually the same moment, I remembered that prior to revealing the existence of the pendant in 1889, Cresson had worked for Putnam on Fort Ancient-age sites and museum collections. What better way to pass off a forgery

than to carve a mammoth on a genuine archaeological specimen stolen from a collection that at least *looked* old, but in the late nineteenth century could not be independently dated?

Of course, in the late twentieth century it could be, and when accelerator radiocarbon dating came on line, we submitted a tiny fragment of the pendant's shell. While we waited for the dating results, Smithsonian archaeologist Bruce Smith organized a pool to guess the pendant's age. The resulting radiocarbon age, when calibrated, came to approximately AD 875 (with a one standard deviation range of AD 760–990). Our prediction of its age was off by just a couple of centuries, probably attributable to the margin of error in dating marine shell; more precise calibration would require knowing where the shell came from in order to account for the local carbon reservoir, but on this point the trail is cold.

No matter. The Holly Oak pendant was engraved from looking at an image in a book, and not at a live mammoth. Bruce Smith won the pool.

Archaeology, not being a hard science but a difficult one all the same, rarely yields the kind of unequivocal results obtained in the Holly Oak case. When it does, it is gratifying—regardless of whether one happens to be right or wrong. But let's be honest: most of us want to be right. Science is like that. After we published the radiocarbon date on the shell, many colleagues claimed they had known all along the Holly Oak pendant was a fake. Science is like that, too.

Cresson had been desperate to place his name on the rolls of American science, and evidently believed that if he made a discovery so wonderful that it resolved the most bitter dispute then facing American archaeology, the archaeological world would beat a path to his door. The flaw in that strategy is that spectacular finds cannot be made on command, unless one is blindly ambitious and utterly dishonest. So it was that a stolen Fort Ancient shell pendant in the hands of a skilled artist schooled in French prehistory and with an illustration of the La Madeleine engraving in a book in front of him, became for a moment the first American Paleolithic art object. But only for a moment.

Whether it was the disgrace of being summarily fired, or the dismissal of his pendant (more by stony silence than public censure), or some other event that started Cresson on his downward spiral into insanity, we will never know. But perhaps Cresson knew. Among the items in his pockets at the time of his suicide was a note in his handwriting that he feared he was "suspected of counterfeiting and that Secret Service detectives were continually on his track."

change Evans's mind and undo the damage, but failed. Critics gloated. Proponents were badly shaken. Only the faithful remained undaunted. A bitter Abbott blamed Putnam for the "unfortunate Toronto business."³⁵

Nevertheless, the active search for the deep past continued, though following that pivotal summer, no more claims were made on behalf of American paleoliths. However, the new century brought a new kind of evidence to center stage.

NEANDERTHALS IN AMERICA?

In December 1899, Putnam's hired hand in Trenton found a human femur (upper leg bone) deep in what appeared to be Pleistocene-age gravels at Trenton. Hope for a Pleistocene human antiquity was renewed, but Putnam was worried. "We must not make any blunder about it," he warned, and for good reason. Government critics quickly scorned the fast-traveling news of the find. Putnam turned the femur over to Aleš Hrdlička, a young physician turned physical anthropologist, whom he hoped would show that the bone had some antiquity. But Hrdlička was none too impressed by the specimen, which looked no different from femurs of recent American Indians. Perhaps, he supposed, the geology of the site might shed light on its age. Wright had examined the geology, and thought the specimen's Pleistocene age was so clear that there was scarcely anything to discuss. Wright, however, was hopelessly optimistic. By then, the Trenton gravels had such geological notoriety that any agreement on their age was impossible.³⁶

The Trenton femur proved to be the first of many human skeletal parts found over the next twenty-five years in apparent Pleistocene-age deposits. Over that period, Hrdlička would emerge—after 1903 in the employ of Holmes at the Smithsonian—to challenge each and every claim, "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 one of his contemporaries put it.³⁷

Hrdlička's position was this: if the earliest Americans had arrived in the Pleistocene, they should look like a Pleistocene-age fossil human—like Neanderthals, say, and not like the American Indians who inhabited the region. In structure this was no more than Abbott's argument (if it's old, it should look primitive) applied to skeletons. But there was one significant difference: the argument worked for Hrdlička. He was fast becoming the premier physical anthropologist of his day, and few could challenge his considerable knowledge of human variability and evolution. To claim a human skeleton was Pleistocene in age, one had to play by Hrdlička's rules. So were there Neanderthals in America?

On a farm along the Missouri River just outside Lansing, Kansas, in February 1902, brothers Michael and Joseph Concannon were digging a tunnel to store fruit and vegetables on their father's farm. Seventy feet into the hillside and twenty feet beneath the surface, they shoveled into two human skeletons. The bones were pushed aside—there was a tunnel to be dug—but after a few months, word of their discovery reached the City Public Museum in nearby Kansas City.

The museum's curator visited the Concannon farm, and seeing that the bones came from under a layer of apparent glacial loess, he alerted geologists Warren Upham and Newton Winchell of the Minnesota Historical Society, who rushed to Lansing. The two men were stunned by what they saw: Pleistocene loess atop the bones. Within days, Upham prepared a paper announcing that Lansing proved a human presence in the New World prior to the last episode of ice advance in North America: in round numbers, perhaps 30,000 years before the present.

To his friend Wright, Upham happily chirped that Lansing vindicated the claims made in *Man and the Glacial Period*, then sent his hastily written paper to Chamberlin and Holmes, inviting "verification or correction of our view."³⁸ Verification, naturally, was what Upham preferred and briefly thought he had. Chamberlin and Holmes (and later Hrdlička) visited Lansing, and soon afterward Upham heard rumors they had endorsed the site's great antiquity—rumors Upham vigorously fanned.

But the rumors were false. Unbeknownst to Upham, even before Chamberlin left for Lansing, he was already grouching about Upham's "fundamental untrustworthiness." Chamberlin's mood hardly improved at the site, where he concluded Upham—his former USGS employee—had badly misread the stratigraphy and geology. Back in Chicago, Chamberlin blistered Upham in a string of letters, lecturing him on the attributes of loess, scientific ethics (like Wright, Upham had "gone public" with his claims about Lansing), and even ominously accused him of "direct falsification."³⁹

Battle lines were drawn once more: Upham, Winchell, and Wright fighting for a Pleistocene age for the loess overlying the skeleton, with Chamberlin and Bohumil Shimek, the Midwest's leading loess expert, dismissing the Lansing deposit as neither true loess nor Pleistocene in age. Holmes and Hrdlička joined Chamberlin, chiming in that the Lansing skulls were no different than crania of American Indians of the region, and surely no more than a few hundred or a few thousand years old. To Abbott watching from the sidelines, it all recalled "the merry old days of earnest work . . . [and] the controversial days that embittered me."⁴⁰ Just like in the old days, neither proponents nor critics backed down.

Scarcely four years later and 195 kilometers up the Missouri River, the exercise was repeated. At the Gilder Mound, just outside Omaha, Nebraska, human crania—Neanderthal-looking to some—were plucked from apparent Pleistocene loess. Eight years later, attention shifted to the tar pits of Rancho La Brea, where a human skull was extracted from the asphalt ooze along with bones of an extinct (Pleistocene) condor. Two years after that (1916), it was Vero, Florida, where human remains rolled out of sand deposits yielding the bones of extinct mammoth and sloth. Nearly a decade later, it was a similar story at Melbourne, Florida.

In each case, a swarm of archaeologists and geologists descended on the site to inspect the skeletal remains and their geological context, Hrdlička usually leading the charge (Figure 14). Testifying to the duration of the dispute, there was now a second



FIGURE 14.

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

generation of participants. Literally, a second generation: geologist Rollin Chamberlin visited Vero in the place of his father, Thomas.

There were the usual disagreements over whether the human skeletal remains at each site were in primary context, or contemporary with the apparently ancient loess or extinct mammal-bearing deposits in which they were found. Hrdlička took a cue from Holmes: since human beings bury their dead and because bone is so easily broken and moved in the earth, the odds were that any bone in ancient deposits came from later times. “Perhaps,” paleontologist Oliver Hay tartly replied, “we get a clue here to the reason why civilized people nail up their dead in good strong boxes.”⁴¹

Besides, Hrdlička continued, even granting human remains had not moved in the ground, what assurance was that of their great age? Geologists’ opinions were utterly divided. The Lansing and Gilder Mound specimens were either in true loess or not in true loess, and thus either Pleistocene or post-Pleistocene in age, while the Vero and Melbourne fauna was either Pleistocene or post-Pleistocene in age—it all depended on which geologist one heeded.

Hrdlička naturally followed the Chamberlins, but his true allegiance was to the bones. What did they have to say? Only if they spoke of an anatomically distinct pre-modern human could they be Pleistocene in age, never mind the geology. But they said no such thing.

Chamberlin (senior) was disappointed in Hrdlička's low opinion of geological testimony, but no more so than in many of his geologist colleagues. In fact, the irreconcilability of interpretations soured relations all around. Anthropologists and archaeologists bickered among themselves over what a Pleistocene-aged human should look like, then argued with paleontologists about the timing of mammal extinctions. Paleontologists wrangled with geologists about where to draw the line between Pleistocene and post-Pleistocene formations. Geologists fought each other over the number, timing, and evidences of glacial history. Even linguists got in the act, clucking with disapproval at everyone's failure to provide them sufficient time to evolve the great diversity of native North American languages (Chapter 5).

Once again, the situation reached an angry impasse. Holmes darkly pronounced the evidence from Vero "dangerous to the cause of science." Journalist Robert Gilder, who had found the Nebraska "Neanderthals," called Hrdlička a "liar."⁴²

So it went. Over the decades, scores 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 Holmes, Hrdlička, and others. In this wide-open field, there were few rules of engagement, and the dispute exposed deep rifts over what constituted legitimate proof of human antiquity. At its worst, Frank Roberts darkly admitted, "the question of early man in America [became] 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." Archaeologist Nels Nelson advised his colleagues to "lie low for the present." Shrewd advice and many, such as Alfred Kidder, followed it: we "comforted ourselves," he later confessed, "by working in the satisfactorily clear atmosphere of the late periods."⁴³ Ironically, the key to resolving the dispute had, since 1908, quietly lain exposed in an arroyo in New Mexico.

IN THE BELLY OF THE BEAST

On August 27, 1908, torrential rains fell on Johnson Mesa in a remote corner of northeastern New Mexico. The thunderstorm broke in early evening, yielding a beautiful sunset, but then, unusually, fired up again. After dark, from a ranch just below the mesa, a frantic phone call went out to Sarah Rooke, the local telephone operator: the Dry Cimarron River was rising fast. Everyone downstream in the town of Folsom needed to head to higher ground. From her switchboard, Rooke began calling the townspeople. Many heeded the warning. Others could not be saved, including Sarah Rooke, who stayed at her post sounding the alarm until the floodwaters tore her small operator's shed from its foundations and washed it away. Her body was found in a field the next spring, buried in mud.⁴⁴ The flood forever changed the town of Folsom. So, too, American archaeology.

Sometime after the flood, George McJunkin, the foreman on the Crowfoot Ranch below Johnson Mesa, went out to check his cattle and fences, and came across a new and deeply incised portion of Wild Horse Arroyo, a tributary of the Dry Cimarron. Looking down, he noticed bones jutting out near the base of the arroyo. Most cowboys would have passed them by: bones are hardly an uncommon sight in ranch country. But by all accounts, McJunkin was no ordinary cowboy. Born a slave in pre-Civil War Texas, he was befriended at an early age by his plantation owner (Jack McJunkin), who taught him to read and supplied him with books. In his teens, George moved to Midland, taking a ranching job using the McJunkin name. By the time he was in his twenties, he was working on the Crowfoot Ranch.

Precisely what McJunkin, a self-taught naturalist, thought of the bones in the bottom of Wild Horse Arroyo is not known. But they obviously piqued his curiosity, for he told others about them. One was Carl Schwachheim, the blacksmith in nearby Raton, who had built a fountain in front of his home using the antler racks of two bull elk that had become entangled in a mortal contest. McJunkin had passed by the fountain, saw that its builder was a kindred spirit, and on trips to Raton would stop in to talk to Schwachheim—at some point, he must have described the bones in Wild Horse Arroyo.

Swachheim did not visit the Folsom site until December 1922 (after McJunkin died). He and Raton banker Fred Howarth collected a few of the bones, which they took to the then Colorado Museum of Natural History in Denver. Jesse Figgins, the director, turned the bones over to the museum's paleontologist, Harold Cook, who identified them as being from an extinct species of bison. Figgins and Cook visited the site in March 1926 and decided to excavate, with the aim of acquiring a bison skeleton for museum display. They were not looking for, nor did they expect to find, any archaeological remains.

Still, they were well aware of the human antiquity controversy. Cook was the discoverer and namesake of *Hesperopithecus haroldcookii*, a fossil he had found in the early 1920s near his family ranch in western Nebraska, which was identified as a pre-Pleistocene form said to resemble what was then the oldest-known human ancestor, *Homo erectus* (Java Man, as it was then known).⁴⁵ Sadly for Cook's hopes of taxonomic immortality (Cook Man?), *Hesperopithecus* proved on closer inspection to be a fossil pig that had become extinct millions of years before our ancestors appeared on the plains of Africa. But Cook paid that little mind—the tooth might well be from a fossil pig, but he was convinced some of the bones found with it had been broken by humans. Ever the optimist, Cook was sure humans had been in America a very long time.

He saw further evidence of that at the site of Lone Wolf Creek, in Colorado City, Texas. Figgins had hired workers there in 1924 to extract the bones of an extinct bison for display (Figure 15). Unfortunately, they made a mess of the task, lopping off the ends of bones to make them fit packing crates, rather than just getting larger crates. Worse, Figgins learned only afterward that three artifacts had been found with the bison. The artifacts were neither left in place nor photographed; one was missing.

The following spring, Figgins dispatched Cook to the site to assess the geology, and even though the evidence was long out of the ground (and badly mangled), Cook still confidently concluded that Lone Wolf Creek provided “good, dependable definite evidence of human artifacts in the Pleistocene in America,” perhaps as much as 350,000 years old.⁴⁶

That was a daring claim at a time when most were unwilling to push human antiquity in America back to 10,000 years ago. But because of the sloppiness of the discovery,



FIGURE 15.

H. D. Boyes and Nelson Vaughn at their excavations at the Lone Wolf Creek site, Texas, 1924. Note the bison remains in place. (Photograph courtesy of the Heart of West Texas Museum.)

there was little reason for confidence in Lone Wolf Creek, no matter how vigorously Cook tried to promote it. And he tried very hard indeed. But that hardly convinced skeptics like Holmes, who immediately asked about Cook's scientific reliability. The answer from Cook's old mentor was none too flattering: "Harold has a somewhat optimistic temperament, and I find it necessary to discount his geological conclusions more or less." Holmes and Hrdlička did as well.⁴⁷

Cook persisted. In a broadside attack on Hrdlička published the next year, Cook invoked Lone Wolf Creek and *Hesperopithecus* to bolster his claim of a Pleistocene or even earlier human presence in the New World. In Hrdlička's angry eyes, Cook's latest paper was just "another head of the hydra,"⁴⁸ and he moved swiftly to decapitate it. But like a hydra, new heads kept popping up.

In early 1927, Cook was called to a gravel quarry in Frederick, Oklahoma, following a report of mammoth and other extinct mammal bones found alongside grinding stones in apparent Pleistocene gravels. "Strangely enough," Cook remarked, "these implements show a degree of culture closely comparable with that of the nomadic modern Plains Indians."⁴⁹ He assessed the geology and concluded the site was about 365,000 years old. But as at Lone Wolf Creek, crucial details of what was found, and where, rested on the unreliable testimony of an inexperienced collector. Worse, an independent assessment of the site's geology concluded the deposits were "not necessarily more than 10,000 years old, and might be somewhat younger."⁵⁰ That was followed by a searing critique of the archaeology that ridiculed the absurdity of Pleistocene-age grinding stones (even today, such are rare).

Yet, Cook and Figgins paid the skeptics little mind, writing that *Hesperopithecus*, Lone Wolf Creek, and now Frederick pushed human antiquity back "by hundreds of thousands of years."⁵¹ By then, however, few were taking them or their sites very seriously. It was in this harshly skeptical climate that their Folsom work emerged.

Schwachheim was hired by Figgins to excavate at Folsom, and began in May 1926. By early July, he was down to the level of the bison bones, and in mid-July, an artifact was found. This was no "rude" paleolith, but a delicately made spear point with a distinctive central groove or *flute*. Unfortunately, the point was out of the ground before he spotted it, so whether it was associated with the bison bones was uncertain. Notified in Denver, Figgins instructed Schwachheim to watch "for human remains and then in no circumstances, remove them, but let me know at once."⁵² He wanted to inspect the remains in place. The remainder of the summer, Figgins waited anxiously for word. None came.

Nonetheless, he and Cook were convinced this was another Pleistocene archaeological site. That fall they wrote matching papers for *Natural History* magazine that, as Figgins boasted to Oliver Hay at the Smithsonian, were "a deliberate attempt to arouse Dr. Hrdlička and stir up all the venom there is in him." As Figgins explained, "Everyone seems to think Hrdlička will attack [and if he] tears a chunk of hide off my back . . . there is nothing to prevent my removing three upper and two lower incisors,

black one eye and gouge the other, after I have laid his hide across a barbed wire fence. I am daring the whole miserable caboodle of them.”⁵³

Brave words, and they inspired Hay to march down the hall to Hrdlička’s office to arrange “a showdown” with Figgins in Washington. When Hay reported what he’d done, Figgins backpedaled fast, declaring it would be much better if Cook went to Washington to “be the [sacrificial] goat.” In the end, Figgins checked his faltering bravado and boarded a train for Washington. By the time he arrived at Hrdlička’s office, he was in a fearful lather. Yet, much to his astonishment and relief, Hrdlička was courteous, “extremely pleased” to see the Folsom point Figgins carried with him, and even offered some advice: if additional points appeared during the coming excavation season, they should be left in place and telegrams should be sent around the country inviting “outside scientists” to come and examine them in the ground.⁵⁴

Good advice, Figgins thought, and he left with newfound respect for Hrdlička. What he didn’t appreciate, however, were Hrdlička’s motives for offering that advice. Hrdlička didn’t trust Figgins or Cook for a moment, and would not be convinced by anything they said about Folsom’s age, or any possible association of artifacts with extinct animals. He wanted others called in to judge the evidence.

Schwachheim resumed excavating at Folsom in the spring of 1927. In late August, he uncovered a Folsom point, this time firmly in place between a pair of bison ribs. Figgins was alerted, and immediately broadcast telegrams around the country announcing, “Another arrowhead found in position with bison remains at Folsom, New Mexico. Can you personally examine find.” Schwachheim was commanded to guard the point night and day until the visiting dignitaries arrived. He dutifully awaited the parade of “Scientists, Anthropologists, Archaeologists, Zoologists, or other bugs.”⁵⁵

It began a few days later with the arrival of Figgins, paleontologist Barnum Brown of the American Museum of Natural History, and archaeologists Alfred Kidder of the Carnegie Institution of Washington and Frank Roberts, a young colleague of Hrdlička’s at the Smithsonian (Hrdlička was invited, but was in Alaska at the time; Holmes by then had retired) (Figure 16). All agreed this was no accidental association of artifact and bone: human hunters had killed this now-extinct Pleistocene bison. Here, finally, was proof in those pre-radiocarbon days of an Ice Age human presence.⁵⁶

That certainty, however, gave way to uncertainty about the site’s absolute age, since the taxonomy and timing of bison extinction were still not altogether clear. Even so, within weeks Kidder announced publicly what he’d always hoped for privately: the first Americans had arrived some 15,000–20,000 years ago (now he could rebut those who claimed the Americas hadn’t been occupied long enough for its civilizations to have developed on their own). The announcement, subsequently elaborated by Brown and Roberts, electrified the scientific community.

Brown, in fact, was so taken by what he saw that he returned to Folsom in 1928 to expand the excavation. That July, when fluted points were again found alongside bison



FIGURE 16.
Carl Schwachheim (left) and Barnum Brown posing with the first in situ Folsom point, September 4, 1927. The point is the one shown in close-up in Plate 1 of this book. (Photograph courtesy of American Museum of Natural History.)

remains, telegrams were once more broadcast across the country, and in response, the find was seen by several of “the best men in the country,”⁵⁷ including some of a new breed of USGS geologists who mapped the region, and who independently affirmed Folsom’s late Pleistocene antiquity (based on my own investigations at Folsom seventy years later, which involved extensive radiocarbon dating, I can report their age estimates were off by only a couple thousand years: that’s quite an achievement, given they had no techniques for absolute age dating⁵⁸).

The decades spent searching for paleoliths or *pre-sapiens* fossils were over. In retrospect, that effort seemed strangely misguided. Folsom was late Pleistocene in age, but it looked nothing like what anyone expected a Pleistocene human occupation to look like.

HISTORICAL HOMILIES

Proponents of a great human antiquity in the Americas, recalling with fondness Holmes and Hrdlička apparently getting their comeuppance at Folsom, see in this history a vindication of their belief that critics hinder the recognition of bona fide early sites and retard the progress of science. That’s a serious charge, but it needn’t be taken too seriously.

Historian of science David Hull observes that “the least productive scientists tend to behave the most admirably, while those who make the greatest contributions just as frequently behave the most deplorably.”⁵⁹ Without question, Chamberlin, Holmes, and Hrdlička often behaved deplorably: they were merciless in attack and engaged in no small amount of behind-the-scenes skullduggery.

Nevertheless, far from hamstringing inquiry, these critics actually sped it along toward resolution. Their tough questions and criticisms crystallized debate on stone tool technology (the means of making tools), the relationship between tool form and age, human evolution, glacial geology, and dating techniques—critical issues all. Much was learned in the half century of dispute, not least what a solution to the human antiquity controversy had to look like. The swift acceptance of Folsom attests to that.

Besides, look at the sites the critics rejected. Today, Trenton, Newcomerstown, Lansing, Gilder Mound, Vero, and all the other allegedly Pleistocene sites are significant because of the battles fought there, not because of the great age of their archaeological remains. None of them were what they were claimed to be. Skepticism about a Pleistocene human presence was not arbitrary, but rather forged in the face of repeated cases that failed to withstand critical scrutiny (circumstances that will arise again).

The antiquity of the Folsom site, like that of Brixham Cave before it, was based on artifacts in close association with the remains of an extinct Pleistocene animal, the only secure means then available for telling time. That made Folsom unlike virtually every previous contender, where it had been impossible to demonstrate that the artifacts (or human skeletal remains) had been deposited at the same time as the Pleistocene-aged deposits or fauna with which they were found.

Equally important, Folsom was a kill site that would ultimately yield the remains of nearly three dozen bison and almost as many points. Consequently, its excavation provided several opportunities for scientific visitors to witness newly discovered points in place. Site visits to evaluate claims of great antiquity had been common since the 1890s. Yet, they were not always successful nor, for that matter, welcome. After a joint visit to Trenton in the late summer of 1897, Abbott griped, “I cannot say, looking back over the past four days, that I have enjoyed it. There is too much assumption of extra-carefulness, as they call it, which is simply a lot of childish twaddle. They cannot grasp the subject in its entirety and see the facts. . . . They may all be very eminent men, but it took me a good deal less time to learn that we had here evidences of man’s antiquity.”⁶⁰

Those earlier site visits were often exercises in incompatibility, never achieving consensus and serving largely to highlight differences in interpretation. This was hardly surprising: archaeological methods and techniques in those years were uneven, many discoveries were made under dubious conditions, training was spottier, more amateurs were in the mix, there was considerable disagreement about how to recognize Pleistocene-age deposits, the criteria for evaluating evidence were less explicit, and a

site's age had to be assessed in the field after careful examination of the stratigraphy and geology, artifact context, and the nature of the associated remains. At Trenton, Lansing, Gilder, and Vero, proponents and critics visited the sites, looked at the very same evidence, but came away with radically different views of what it meant. The 1927 visit to Folsom, where everyone agreed on what they were seeing, was the exception far more than the rule. But then by 1927, archaeology, geology, and vertebrate paleontology had become more professional sciences.

It was not inevitable that resolution of the human antiquity controversy would occur at Folsom, only that a site such as Folsom was needed, where the association of points and extinct animals was indisputable and could be repeatedly witnessed. That's critical, for the evidence at Folsom was seen—on Hrdlička's good advice—by members of the scientific elite.

Historian of science Martin Rudwick has shown that controversy in science, or at least nontrivial controversy, is not resolved by consensus across the community.⁶¹ Rather, resolution is brought about by a core elite within the field who are recognized as experts, even if they are not particularly involved in the research within that area. These elite scientists regard themselves, and are regarded by others, as competent arbiters of the fundamental questions of a discipline: for example, the antiquity of people in America.

That Kidder examined Folsom, then publicly announced his acceptance of the evidence, carried enormous weight, for in the 1920s, he was at the height of his considerable power and influence (like Lyell in the 1860s). He was not being immodest when he explained to Figgins that “as an archaeologist, I am of course not competent to pass either upon the paleontological or the geological evidences of antiquity, but I have paid great attention for many years to questions of deposition and association. On these points I am able to judge, and I was entirely convinced of the contemporaneous association of the artifact which you so wisely had left ‘in situ’ and the bones of the bison.”⁶²

Wise, indeed. Figgins, of course, had done so in order to convince two more members of the elite: Holmes and Hrdlička. We know their opinions mattered, and not just because they thought so. Cook and Figgins thought so, too. In every paper they wrote, they wrote for—or rather, against—the Smithsonian duo. They recognized, however much they disliked the idea, that it was only “right and proper [that Holmes and Hrdlička] should not take without question such basic evidence as may seem necessary to establish a given fact beyond reasonable question.”⁶³

Holmes and Hrdlička did accept the evidence from Folsom, though on their own terms. When asked directly, the eight-one-year-old Holmes understandably replied he was finally content to leave judgment to others (the old lion passed away a half-dozen years later). Hrdlička quietly probed for weaknesses in the Folsom case, but did so more out of habit than hope, for he respected Kidder's authority in such matters. It was not true, as one paleontologist teased, that Hrdlička would not accept that the Folsom bison were speared by humans unless he had “fired the arrow himself.”⁶⁴ But Hrdlička did

want to know who was there when the “arrow” was unearthed. When later confronted on the larger question of human antiquity in America some years later, Hrdlička stuck close to his original script: there was still no evidence of a pre-*sapiens* skeleton in America. In that, he was correct.

Holmes and Hrdlička for decades had been the scourge of claims for a deep human antiquity. That they said nothing about Folsom spoke volumes. No mea culpa was offered, but then no one expected them to admit to being wrong for so long.

But if Holmes and Hrdlička lost the war over a Pleistocene human antiquity in America, it was not because Cook and Figgins won the battle at Folsom. Much like the situation at Brixham Cave, there was a sharp divide between those who made the discoveries (Boucher de Perthes; Cook and Figgins), and those who were called upon to judge their significance (Evans, Lyell, and Prestwich; Brown, Kidder, and Roberts). In both instances, the opinions of the discoverers were largely ignored because of their propensity to make absurd claims about what they’d found. Figgins understood their place, privately admitting “our opinions are valueless.”⁶⁵ He was not being humble; he was being honest. At a time when virtually all archaeologists were skeptical of a Pleistocene presence in the Americas, he and Cook were campaigning for several spectacularly weak cases. Nelson lectured Figgins that if everything he and Cook said were true, “we shall have to revise our entire world view regarding the origin, the development, and the spread of human culture.”⁶⁶ Nelson was not ready to do that. Few were.

Even worse, it wasn’t obvious to Cook and Figgins, as it was to everyone else, that Folsom was the pick of the litter. In fact, Cook judged Folsom the “weakest and least conclusive of our localities,” and Frederick the strongest.⁶⁷ That they couldn’t properly evaluate their own evidence destroyed any remaining shreds of their credibility.

Cook and Figgins fell victim to what’s been called the *Matthew Effect*: “for whosoever hath, to him shall be given . . . but whosoever hath not, from him shall be taken away even that he hath.” Hrdlička’s reputation may have been roughed up by Folsom, but only slightly, and he continued to be a sought-after authority regarding human antiquity in the Americas. Not all scientists are created equal; some are more equal than others. And inequality is most visible during episodes of controversy, when the stakes are highest. In the end, and despite their crucial role in the discovery at Folsom, neither Cook nor Figgins was asked to interpret the meaning of what he had found, nor given the opportunity to participate in any of the half-dozen major symposia devoted to human antiquity that followed in the next decade, let alone receive the acclaim for his contributions. Unfair, perhaps, but at least there were these kind, albeit private words to Figgins from the ever-gracious Alfred Kidder: “Anthropology owes you a very great deal for having handled this material so carefully and so intelligently, and I think the researches of yourself and Dr. Cook will go far towards opening a new era in the study of the question of Pleistocene man in the New World.”⁶⁸ They did.

A MAMMOTH BARRIER

The Folsom find did not result in an antiquity comparable to the remote ages of Paleolithic Europe. But by 1927, it was obvious American prehistory did not extend that far back. Nonetheless, Folsom did show that the first Americans arrived at least by the end of the Pleistocene. They were not Paleolithic peoples, but assuming them to be ancestors of modern American Indians, they came to be called *Paleoindians*.

The Folsom find also taught archaeologists how to look for Paleoindian sites. The strategy was simple: look in arroyo channels or ancient lake beds, or track down reports of large and easily spotted bones of extinct Pleistocene mammals (Figure 17), then carefully comb those localities for any associated human artifacts. So it was that Clovis came to light. A road crew, mining gravel from an old pond near Clovis, New Mexico, had struck immense fossils bones. Word of their discovery reached Edgar B. Howard of Philadelphia's Academy of Natural Sciences, who began excavations there in the summer of 1933, and soon uncovered mammoth bones alongside fluted points that were longer, broader, and less finely made than Folsom points (Chapter 8). These "generalized Folsoms" soon became known as Clovis points.

Naturally, how archaeologists looked for Paleoindian sites predisposed what they found: almost all of the nearly two dozen found in the decade after the Folsom discovery were kill sites with bones of extinct animals and artifacts. "Boneless" Paleoindian sites were rare.⁶⁹ Not fully realizing how much this pattern was biased by their search strategies, archaeologists saw in the many sites littered with bones of bison



FIGURE 17.

A bison tibia (the larger of the two lower leg bones) exposed on the wind-swept floor of a now dry Pleistocene lakebed in far west Texas. Spotting bones eroding out in this manner led to the discovery of many Paleoindian kill sites, particularly during the 1930s Dust Bowl. (Photograph by David J. Meltzer.)

and mammoth the testimony that Paleoindians were top predators, who specialized in the killing of big game. An inspiring vision of gutsy hunters, holding a trumpeting and mortally wounded animal at bay, came to embody North American Paleoindians, and often established expectations of Paleoindian sites in areas environmentally and climatically different from the Great Plains, where virtually all the iconic kill sites had been found. Steven Simms, who works in the Great Basin, gives voice to many when he complains of “stereotypes of [Paleoindian] lifeways applied uncritically from the Plains.”⁷⁰ Big-game hunting makes for good copy, but the appearance masks a different reality (Chapter 8).

Sites with Folsom points soon proved to be limited in their distribution to the western plains, but Clovis points were more widespread—across the continent, in fact. They were also found in deposits below Folsom points, evidence the Clovis archaeological complex was older than Folsom. How much older would only be learned a couple of decades later, following the advent of radiocarbon dating (it would also become clear that by Folsom times, there were many other archaeological complexes in other parts of the continent, as detailed in Chapter 9).

But were Clovis groups the first Americans, or had people arrived earlier still? In the aftermath of the Clovis discovery, archaeologists sought traces of more ancient Americans, and soon found themselves again at loggerheads over the question of antiquity. As early as 1953, Alex Krieger warned his colleagues that having overthrown the Holmes-Hrdlička “dogma,” they were now in danger of replacing it with another. The first Americans would be permitted a late Pleistocene entry, but he feared that 10,000 years was fast becoming the new “allowed antiquity.”⁷¹

Yet, in 1953 he tallied a half-dozen sites that “may and probably do” break that barrier. In 1964 he upped the total to fifty sites in North and South America that he thought pointed to a human presence predating Clovis. Not all sites were likely genuine, as Krieger well appreciated. But what impressed him most was how many sites *looked* old. As he saw it, where there’s smoke there’s fire. The sites on his list included some with radiocarbon ages ranging from 21,000 to more than 38,000 BP. Others had bones of extinct animals that appeared split, burned, or broken by human hands. And then there were the Malakoff heads—giant sandstone boulders from deep in a Texas gravel quarry that had crude “faces” carved into them, looking like enormous versions of Mr. Potato Head toys. Krieger even tossed in several American Paleolithic sites, Trenton included, for they “cannot all be set aside as insignificant.”⁷²

In fact, many of the sites on Krieger’s list recalled the American Paleolithic, for they contained crude stone or bone artifacts. Krieger insisted he was not making Abbott’s mistake of equating artifact form with age, merely raising the possibility of a “pre-projectile (pre-Clovis) point stage.” Perhaps. But few were eager to follow Krieger out on his speculative limb. Others were busy sawing it off behind him.

The same year Krieger published his pre-Clovis compendium (1964), C. Vance Haynes reported the first secure radiocarbon ages for a half-dozen Clovis sites (including

the type site). Their ages fell in a very narrow slice of time, between 11,500 and 11,000 BP.⁷³ None were more than 12,000 years old, and none yielded evidence of a precursor or pre-Clovis population, despite having underlying sediments of the right age, and with ecological conditions that should have been favorable to occupation—had people been present in the area.

But if there was no pre-Clovis, how might one explain the “apparently sudden appearance” of Clovis over much of North America 11,500 years ago? Geology provided a clue. By the late 1950s, there were a raft of radiocarbon ages available on the timing of deglaciation, and in them Haynes spotted a striking concordance between the geological and archaeological records. It appeared that 12,000 years ago the ice-free corridor had opened, linking Alaska with the rest of the continental United States for the first time in 15,000 years. Was it mere coincidence Clovis appeared south of the ice sheet at this moment?

It all made perfect sense: the land bridge connecting Siberia and Alaska only emerged during glacial cycles, but once migrants reached Alaska, ice sheets had blocked their path south. Either the first Americans came before the Late Wisconsin ice advance, in which case they had to contend with crossing the open Bering Sea, or they came later and walked across the land bridge, then cooled their heels in Alaska waiting for the Cordilleran and Laurentide ice to retreat. The splendid chronological correlation between the disappearance of the ice and the appearance of Clovis surely favored the latter hypothesis.

The way Haynes saw it, Clovis progenitors probably were in Alaska some 12,500 years ago, moved out across the Arctic slope soon thereafter, and then down the ice-free corridor fast on the heels of its opening. The migration from Siberia into northwestern North America might have taken 1,500 years altogether, but once south of the glaciers, Clovis groups could have spread rapidly east and west, colonizing the entire continent in fewer than 1,000 years.

For Haynes the pieces were falling neatly into place. For Krieger, his worst fears were coming to pass (Figure 18). The notion that Clovis was one of the *older* occupations of North America was steadily losing ground to the idea that it was the *oldest* occupation in North America.

Still, in 1964 Haynes saw “good indications” there were people in America before 12,000 years ago. He just didn’t think they were related to Clovis, or perhaps there were just very few of them. But by 1969, he was losing enthusiasm for the idea of pre-Clovis.⁷⁴ His newly found skepticism was understandable. In the intervening years, he had learned the hard way that sites too good to be true often weren’t.

Tule Springs, located near Las Vegas, Nevada, had everything going for it: genuine artifacts, bones of Pleistocene megafauna seemingly broken by human hands, and a radiocarbon date of 28,000 BP. Anxious to learn more of this occupation, a team of archaeologists, paleoecologists, and geologists embarked on an ambitious excavation program, overseen by a blue-ribbon panel of scientists. Almost immediately Haynes,



FIGURE 18.
C. Vance Haynes and Alex Krieger examining
specimens at the 1970 Calico Conference.
(Photograph courtesy of David J. Wilson.)

the site geologist, noticed that the “hearths” at Tule Springs were nothing more than organically blackened deposits. That was welcome news, solving as it did a longstanding puzzle: if these were hearths, why were they full of unburned snail shells? Then the curious fracturing of the megafaunal bones turned out to be restricted to remains found in the spring vents, and likely had been broken by the frantic trampling of animals trapped in those quicksand-like sediments, and not by human hands. Their “burning” was merely groundwater staining. The only indisputable artifacts at Tule Springs were from deposits much younger than 12,000 years old.⁷⁵ Tule Springs was not pre-Clovis. Still, neither Haynes nor anyone else categorically rejected the possibility of an older human presence in the Americas. Just so, the seeds of skepticism were sown.

At the outset of the Great Paleolithic War, Charles Abbott had expressed the fervent hope that “the ‘doubting Thomases’ [would] be fewer by the year 2000.”⁷⁶ As it turns out, Abbott finally got something right. But it was no easy road getting there.