JOURNAL OF
ANTHROPOLOGICAL RESEARCH
(Formerly Southwestern Journal of Anthropology)
VOLUME 61 • NUMBER 4 • WINTER • 2005

THE SEVENTY-YEAR ITCH
Controversies over Human Antiquity and Their Resolution

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Several major turning points in the study of human prehistory have occurred at almost precisely 70 year intervals: from the initial establishment of a deep human antiquity in Europe in the late 1850s (at Brixham Cave and in the Somme River Valley) to the demonstration in the late 1920s at Folsom that American prehistory reached into the Pleistocene (albeit not very far) to the realization in the late 1990s, based on evidence from Monte Verde, that there was a still-earlier, pre-Clovis presence in the Americas. It is unlikely that the cyclical nature of these episodes is anything more than an odd coincidence. Still, there are patterns to those cycles of controversy and resolution beyond their timing that tell us a great deal about the evolution of and revolution in scientific knowledge. Moreover, in comparing these episodes, and the differences that emerge from that comparison, we can see clearly how much (and how little) archaeology has changed over the past two centuries.

In 1997, I began a long-term field project at the Folsom Paleoindian site in northeastern New Mexico (Meltzer 2006; Meltzer et al. 2002). Folsom is, of course, a very famous site, though not because of my work there. It is famous because of what happened there in 1927: crucial evidence was uncovered that finally resolved a decades-long and bitter controversy over whether the first Americans had arrived during the Ice Age, or only later in the Recent period.

As it happens, in 1997, Tom Dillehay published his massive, second (and final) volume on the Monte Verde site in Chile (Dillehay 1997), which provided crucial evidence that finally resolved for most a decades-long and bitter controversy over whether the first Americans had arrived in pre-Clovis times, that is, before 11,500 B.P.[1]

Journal of Anthropological Research, vol. 61, 2005

In 1927 and 1997, there were site visits by independent observers to both localities, and these played a role (of which, more below) in the acceptance of the evidence from each. In both instances, the push-back of human antiquity triggered sea changes in American archaeology (Meltzer 2004). Working at Folsom in 1997, I was amused by the realization that the two milestones happened precisely 70 years apart, and that my travels that year involved both places. In thinking more about it, it dawned on me that the Folsom resolution came almost 70 years after the resolution in Europe of the greatest of all human antiquity disputes, the long and bitter controversy over whether humanity had a past that predated the biblically allotted 6,000 years.

Three controversies over human antiquity—one in the Old World, two in the New—each resolved seven decades apart. The more I thought about that 70-year cycle of controversy and resolution, the more apparent it became that this stunning historical coincidence meant . . . absolutely nothing at all. After all, how could it?—not unless one views scientific disciplines as complex organisms with cicada-like life-histories, which I do not.

Nonetheless, in thinking about these three episodes, I was struck by the fact that despite being separated by 70 years, taking place on vastly different archaeological and intellectual stages, and involving unrelated generations of archaeologists, each episode played out in surprisingly similar ways, had many of the same elements, and even had participants reprising analogous roles. And that is no coincidence. Instead, this convergence reveals much about the way science works, no matter in the mid-nineteenth century or at the end of the twentieth. Of course there are differences—critical ones—which provide a gauge of how much archaeology has changed over these two centuries. Indeed, understanding the several historical episodes provides some useful insights into the post–Monte Verde world of studies of the first Americans.

I will elaborate on those themes here, with Folsom as the starting point. Importantly, this essay is not intended to be a detailed or comprehensive historical study of these particular episodes. There is no need in the case of the two earlier ones. Although they may not be as well known as the most recent episode, they have each been thoroughly investigated and need only be summarized here (on the establishment of human antiquity in Europe in the 1850s, see Daniel 1976; Goodrum 2004; Grayson 1983, 1990; Gruber 1965; Oakley 1964; Van Riper 1993; on the 1920s establishment of a Pleistocene human antiquity in America, see Hinsley 1976, 1981, 1985; Meltzer 1983, 1991, 1994, 2003, 2006; Wilmsen 1965).

The third and latest cycle of controversy and resolution will be better known to most readers, but it is so recent that it has not been the subject of comparable historical investigation, though there have already appeared a few retrospective commentaries (e.g., Adovasio and Page 2002; Fiedel 2000; Meltzer 2004). Indeed, it is important to tread carefully in assessing the Monte Verde case. Because it happened so recently, we lack the comfortable historical distance and analytical perspective that come with exploring decades- or centuries-earlier events, or of discussing individuals who are not one’s contemporaries, friends, and colleagues. Moreover, the rhetorical clamor (from both sides) has not entirely quieted down in the years since Monte Verde; echoes of the pre-Clovis controversy still resonate through any such discussion (surely, this one included). Finally, and following the points just made, such a discussion is inevitably biased by one's perspective on and position in the pre-Clovis dispute.

Thus, for example, some of the commentary on the pre-Clovis controversy and its dénouement at Monte Verde by individuals who viewed it from the outside (e.g., Downey 2000; Fedje et al. 2004; Fiedel 1999) bears only a passing resemblance to what I—as a participant in some of these events—think actually happened. In fact, not only do these comments appear to err on what happened publicly, they naturally miss much of what went on behind-the-scenes—and those who have surmised what our intentions were are completely wrong. As Edmund Leach remarked, history "as viewed by participant observers is quite different from the same history as viewed by non-participant observers, and further, that even among participant observers there are several different categories. The 'insiders' and the 'outsiders' participate in quite different ways" (Leach 1984:7). Likewise, I am under no illusion that my particular take on those events necessarily matches those of other "insider" participants (e.g., Adovasio and Page 2002), let alone that it is a full and accurate portrayal in any historically meaningful sense. This episode will surely be viewed differently decades from now by those who can see our landscape more broadly and perhaps more clearly than those of us who presently occupy it. *Caveat lector.*

ESTABLISHING A PLEISTOCENE ANTIQUITY IN AMERICA: FOLSOM, 1927

The controversy that ended at Folsom in 1927 had begun in the 1860s, when archaeologists went searching for evidence that the first Americans had been here since the Pleistocene.[2] European prehistorians had just demonstrated that their past reached back that far (see below) and American archaeologists, keen to show the New World was just as old, began seeking evidence to prove it (Meltzer 1983:5–6; also Hinsley 1981). They started with the hope and assumption there was an "exact synchronism [of geological strata] between Europe and America" (Whittlesey 1869:271–72), and thus there would be in America stone tools alike in form, evolutionary "grade," and antiquity to those of Paleolithic Europe.

By the 1870s, stone artifacts, seemingly akin to those ancient European Paleoliths, were reported by Charles Abbott from apparent Pleistocene-age gravels at Trenton, New Jersey (Abbott 1877). Abbott insisted such "rude" tools had to be old: not only were they unlike any in use by historically known Native Americans, but "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 1881:126–27, emphasis in original). To his delight, this was a view shared by eminent European prehistorians (e.g., Dawkins 1883; see Meltzer 2003).

Abbott's discovery triggered a cascade of claims of American Paleolithic artifacts (e.g., Babbitt 1883, 1884; Cresson 1889a, 1889b; Mills 1890; Wilson 1889; Wright 1890), even as far away as the Great Basin (McGee 1887). Yet, most were found on the surface and lacked geological supporting evidence of great antiquity. Still, they so readily mimicked European Paleolithic tools that it was asserted they surely were as old (Abbott 1881:517). By the last years of the 1880s, several syntheses of the evidence for the new-found American Paleolithic were published (e.g., Abbott 1889; McGee 1888; Mason et al. 1889; Putnam et al. 1888, 1889; Wright 1889). But its existence proved short-lived.

Within the year William Henry Holmes launched a withering counter-attack, having realized in his studies of a prehistoric quartzite quarry in Washington, D.C., that an artifact might appear "rude" merely because it was unfinished, not because it was ancient (Holmes 1890, 1892). The antiquity of archaeological remains was a matter of geology, not morphology, and he saw no evidence in any of the American "Paleolithic" sites of compelling geological proof of great antiquity (e.g., Holmes 1893a, 1893b, 1893c). Proponents of an American Paleolithic fired back (e.g., Haynes 1893, Winchell 1893a, 1893b; Wright 1892; Youmans 1893a, 1893b), but even joint site visits to examine the evidence failed to yield consensus (Meltzer 1994). Thus began what contemporaries called "The Great Paleolithic War"; by the latter part of 1890s any semblance of dispassionate discussion had dissolved into a rancorous, complex dispute over evidence, method, and theory, pierced by sharp rhetorical barbs over competence, status, and authority (Meltzer 1991). Within a few years, the two sides were completely irreconcilable (Fowke 1902).

Matters hardly improved when evidence shifted in the first decades of the twentieth century to human skeletal remains in apparent Pleistocene deposits, save that now the physical anthropologist Aleš Hrdlička took the lead in criticizing the evidence, largely on his strongly-held belief that earlier forms of the human species should show a mosaic of increasingly 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 (Hrdlička 1907, 1918). Nor was there secure geological evidence to buttress claims of deep antiquity, as became clear in site visits to purported Pleistocene age localities such as Lansing (Kansas), Gilder Mound (Nebraska), and Vero and Melbourne (Florida).

"Facts are facts," Harold Cook had assured John Merriam (Cook to Merriam, January 1, 1929, JCM/LC),[3] 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.

So it went for 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 (for details on several of the individual sites and how they played out in the controversy, see Meltzer 1983, 1991, 1994). In this wide-open field, there were few rules of engagement; all claims were deemed unacceptable, and archaeologists, physical anthropologists, geologists, and linguists fought among and between themselves, which exposed deep conceptual 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" (Roberts 1940:52; see also Kidder 1936:144). So nasty did matters become that 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. As A.V. Kidder put it, we "comforted ourselves by working in the satisfactorily clear atmosphere of the late periods" (Kidder 1936).

Yet, events that would ultimately resolve the controversy had already been set in motion. On August 27, 1908, 15 inches of rain fell on Johnson Mesa in northeastern New Mexico. Below it the Dry Cimarron rose out of its banks and rolled down valley, sweeping away lives and property in the small town of Folsom, New Mexico. Sometime later, George McJunkin—the foreman on the Crowfoot Ranch below the Mesa—went out to check his cattle and fences, and he came across a newly incised portion of Wild Horse Arroyo, a tributary of the Dry Cimarron. Looking down, he noticed bones jutting out near the base of the deep arroyo.

By all accounts McJunkin was no ordinary cowboy; born a slave in pre–Civil War Texas, he was befriended at an early age by the plantation owner (Jack McJunkin), who taught him to read and kept him supplied with books. In his teens, George McJunkin moved to Midland, taking a ranching job and using the McJunkin name (Folsom 1992; Preston 1997). By the time he was in his twenties he was working in New Mexico 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, as he told others about them. During one of his trips to nearby Raton, New Mexico, he described the find to blacksmith Carl Schwachheim, a kindred amateur naturalist and fossil collector.

Schwachheim visited the site in December 1922 (after McJunkin died) with Raton banker Fred Howarth. They collected a few of the bones, which they subsequently took to the then-Colorado Museum of Natural History in Denver. Jesse Figgins, the museum's director, turned the bones over to paleontologist Harold Cook, who identified them as being from an extinct species of bison. Figgins and Cook joined Howarth and Schwachheim for an on-site visit in March of 1926 and decided to excavate, with the aim of acquiring a bison skeleton to put on display at the museum. 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 from Snake Creek, near his ranch in western Nebraska, which had been identified as a Lower Pliocene anthropoid primate supposedly resembling *Homo erectus* (Osborn 1922; also Gregory and Hellman 1923:14; see Skinner et al. 1977:277–78). Sadly for Cook's hopes of taxonomic immortality, *Hesperopithecus* proved to be a fossil pig that had gone extinct millions of years before our ancestors appeared in Africa (Gregory 1927). Cook paid that little mind, for he was convinced there was other evidence people had been here in America a very long time.

Part of that evidence came from Lone Wolf Creek, in Colorado City, Texas. Figgins had hired a couple of workers there in 1925 to extract the bones of an extinct bison for display at the museum (Figgins to Vaughan, May 9, 1925, JDF/DMNS), only to learn afterward that several projectile points had been found with the skeleton (Figgins to Hay, March 11, 1925, JDF/DMNS; Figgins to Vaughan, March 16, 1925, JDF/DMNS). Figgins found himself in the awkward position of building a case for Lone Wolf Creek's antiquity, long after the evidence was out of the ground. Cook visited the site, assessed the geology, and then boldly announced in *Science* 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 (Cook 1925:459).

That was a bold claim at a time when most were unwilling to push human antiquity on this continent back even to the end of the Pleistocene. But because of the sloppiness of the discovery, 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 the only reaction that elicited was an inquiry from Holmes to John C. Merriam, president of the Carnegie Institution of Washington, about whether a site visit could be arranged to evaluate Cook's "risky announcement" (Holmes to Merriam, November 23, 1925, WHH/SIA). A site visit was not possible, but inquiries were made of Cook's old mentor, who reported that "Harold has, as you know, a somewhat optimistic temperament, and I find it necessary to discount his geological conclusions more or less" (Matthew to Merriam, November 27, 1925, JCM/LC). And so most did.

Still, Cook persisted. In a *Scientific American* broadside attack on Hrdlička published the next year (Cook 1926), Cook invoked Lone Wolf Creek and *Hesperopithecus* to support his claim for a Pleistocene or even earlier human presence in the New World. In Hrdlička's eyes, Cook's latest paper was just "Another head of the hydra" (a remark Hrdlička angrily scrawled across the top of the reprint Cook had sent him) and he moved swiftly to cut it off (Meltzer 2006).

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" (Cook 1927b:117). Cook assessed the geology and concluded the site was about 365,000 years old (Cook 1927b:117). Paleontologist Oliver Hay, on the basis of the fauna, put its age in the "early Pleistocene, the first interglacial stage, in round numbers, 500,000 years ago" (Hay 1927). But as at Lone Wolf Creek, crucial details on what was found, and where, relied on the testimony of an inexperienced collector (Cook to Hay, March 16, 1927, OPH/SIA; also Figgins to Cook, October 12, 1927, HJC/AGFO). Worse, an independent assessment of the geology of the site concluded the Frederick strata were "not necessarily more than 10,000 years old, and might be somewhat younger" (Evans 1930), and that was followed by a searing critique of the archaeology, which called into question claims about artifact context, and the incongruity of grinding stones in Pleistocene beds (Spier 1928).

Yet, Cook and Figgins paid the skeptics little mind, writing that *Hesperopithecus*, Lone Wolf Creek, and Frederick pushed human antiquity back "by hundreds of thousands of years" (Cook 1927b:116). By then, however, few were taking them or their sites very seriously. As Hrdlička put it, here was a trio of questionable finds which plunged the human presence in the Americas back hundreds of thousands of years, but none had "been examined except superficially by any anthropologist or archaeologist outside of those directly concerned" (Hrdlička to Hodge, June 7, 1928, AH/NAA). Worse yet, their advocates always seemed to "assume the highly unscientific attitude of endeavoring to prove the case without considering the evidence to the contrary" (Hodge to Hrdlička, June 1, 1928, AH/NAA). It was in this climate of skepticism that Cook and Figgins's Folsom work emerged.

Schwachheim was hired by Figgins to excavate at Folsom, and he began in May of 1926. By early July he was down to the bonebed level, and in mid-July the first Folsom point was uncovered (Meltzer 2006). Unfortunately, the point was out of the ground before he spotted it. Schwachheim notified Figgins, who sent instructions that if any more artifacts were found they should be left in place, so Figgins could examine and collect the find himself (Figgins to Howarth, July 22, 1926, DIR/DMNS). Figgins waited all summer for word of another artifact found in place at Folsom. None were.

But Figgins and Cook were convinced this was yet another Pleistocene archaeological site. That fall they wrote a pair of papers for *Natural History* (Cook 1927a; Figgins 1927) which, 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 he explained:

> Everyone seems to think Hrdlička will attack . . . and if you haven't realized . . . I will fight back in a two-handed manner, then watch the dust the instant Hrdlička appears in print. . . . You see, I am a free lance and without responsibility in the matter of "scientific courtesy," so if a party 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 (Figgins to Cook, December 28, 1926, JDF/DMNS).

"I am daring the whole miserable caboodle of them," he proclaimed.

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" (Figgins to Cook, November 26, and December 21, 1926, HJC/AGFO; Figgins to Hay, November 8, 26, December 10,1926, and Hay to Figgins, November 17, December 6, 1926, DIR/DMNS and OPH/SIA)

In the end, Cook was unwilling to be sacrificed, so Figgins himself traveled east in early 1927. By the time he arrived at the Smithsonian he was in a fearful lather. Yet, to his great relief, Hrdlička seemed pleased to see the Folsom points he'd brought, and only expressed the regret none were found in place. Hrdlička even offered some advice: if additional artifacts appeared, they should be left in place, and telegrams should be sent around the country inviting "outside scientists" to come and examine them. Figgins thought that perfectly reasonable, and left with newfound respect for Hrdlička (Figgins to Brown, June 8, 1927, VP/AMNH; Figgins to Hay, July 1, September 29, 1927, OPH/SIA; Meltzer 1991:32).

What Figgins didn't appreciate was Hrdlička's motive for offering that advice. Hrdlička had long advocated the establishment of a blue ribbon panel, to be funded by an agency such as the National Research Council, that would critically examine each new claim of great antiquity in the Americas (e.g., Hrdlička to Chamberlin, October 20, 1919, AH/NAA; Meltzer 1994). While that particular idea never got off the ground, Hrdlička always held to the principle (longstanding in both Europe and America) that all such claims needed to be examined and evaluated on site by recognized experts. Hrdlička didn't trust Figgins or Cook for a minute, and he wasn't about to be convinced by anything they said about the site, its age, or any possible association of artifacts with extinct animals. He wanted the professionals called in when the time came.

The summer of 1927, Schwachheim resumed excavations at Folsom, and on August 29, 1927, a Folsom point was found, this time firmly between the ribs of a *Bison antiquus*. Schwachheim wrote Figgins, who immediately broadcast telegrams around the country announcing "Another arrowhead found in position with bison remains at Folsom, New Mexico. Can you personally examine find" (e.g., Figgins to Brown, August 30, 1927, BB/AMNH). Schwachheim was commanded to keep his eyes on the point "every minute" (Figgins to Schwachheim, August 31, 1927, DIR/DMNS), and he did so, awaiting the parade of "Scientists, Anthropologists, Archaeologists, Zoologists, or other bugs" (Schwachheim to Figgins, September 4, 1927, DIR/DMNS).

It began September 4, 1927, with the arrival of paleontologist Barnum Brown of the American Museum of Natural History (already out West doing fieldwork) and Frank Roberts of the Smithsonian Institution, who had been sent in Hrdlička's stead (Wetmore to Figgins, September 2, 1927, DIR/DMNS). Roberts had been attending the first Pecos Conference, and he was so taken by what he saw he returned twice more, on the last occasion with A.V. Kidder in tow (Kidder to Figgins, October 13, 1927, JDF/DMNS; Roberts 1935:5). All agreed that the projectile point and the bison were contemporaneous, and, in those pre-radiocarbon days, that was evidence enough that the first Americans had arrived in the Pleistocene (Brown 1928a; Kidder to Figgins, October 13, 1927, DIR/DMNS; Roberts to Fewkes, September 13, 1927, BAE/NAA; Meltzer 1983:35–37).

Within the month, Kidder announced publicly what he'd always hoped for privately (Meltzer 1993:129–30): the first Americans had arrived some 15,000–20,000 years ago (Kidder 1927). The announcement, elaborated on by Brown, Nelson, and Roberts at the meeting of the American Anthropological Association that December, electrified the scientific community. For his part, Brown returned in 1928 to open a larger excavation at Folsom. That July, when points were found in situ with bison remains, telegrams were once more broadcast to institutions across the country, and again in response the find was seen by "several of the best men in the country" (Cook to Loomis, November 12, 1928, HJC/AGFO). Frank Roberts visited in 1928 as well, joined by his Smithsonian colleague Neil Judd, and by USGS/Harvard University geologist Kirk Bryan. Bryan spent time there assessing the geology, and he put the "age of the material containing *B. taylori* and the implements [as] late Pleistocene or perhaps early Recent" (Bryan 1929:129). Brown reached a similar opinion based on the bison bones (Brown 1928a, 1928b, 1929). The case for Folsom was sealed.

Unlike the previous contenders, going back to Abbott's Trenton gravels, Folsom was Late Pleistocene in age, there were artifacts, and their geological context was unimpeachable. Even from a distance, *Hesperopithecus* and toad-bearing mud balls smelled bad. More to the point, because Folsom was a *kill site* with dozens of extinct bison, it was possible for successive waves of visitors to witness newly discovered points in place. It was not inevitable that resolution of the human antiquity controversy would occur at Folsom—only that a site *like* Folsom was needed, one where the association of points and extinct animals was indisputable, and could be repeatedly witnessed.

The latter is critical, for the evidence at Folsom was seen—on Hrdlička's advice—by the scientific elite. The history of science shows that in times of controversy, resolution is achieved when an elite core of the scientific community makes up "its collective mind on the issue" (Oldroyd 1990:345; also Grayson 1983). These elite scientists regard themselves, and are regarded by others, "as competent arbiters of the most fundamental matters of both theory and method within the sciences" (Rudwick 1985:420). Hrdlička was one, Kidder another. Kidder 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 (Kidder to Figgins, October 13, 1927, DIR/DMNS).

We know the opinions of these individuals 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, Hrdlička and Holmes (e.g., Figgins 1927:229). They recognized, however much they disliked the idea, and they disliked it intensely, 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" (Cook to Hay, December 23, 1926, OPH/SIA). Hrdlička had to be convinced—or at least dissuaded from criticizing—any claim for great antiquity. That Kidder examined the site, then publicly announced his acceptance, carried enormous weight; in the 1920s he was a god within the archaeological community.

Cook and Figgins were, at best, false prophets. At a time when virtually all archaeologists had become deeply skeptical of a Pleistocene presence in the Americas, these two were campaigning for several spectacularly weak cases. Even worse, it wasn't obvious to them, 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 (Cook 1927b:117, 1928b:39; Cook to Hay, January 25, 1928, OPH/SIA; Cook to Ingalls, January 6, 1929, HJC/AHC; Cook to Wissler, March 25, 1929, HJC/AGFO). Figgins wasn't as enamored of Frederick (Figgins to Cook, October 12, 1927, HJC/AGFO), but he nonetheless considered Folsom "merely confirmatory" (Figgins to Cook, September 25, 1926, HJC/AGFO). That Cook and Figgins couldn't even properly evaluate their own evidence destroyed any remaining shred of their credibility.

ESTABLISHING A PLEISTOCENE HUMAN ANTIQUITY: EUROPE, 1858–1859

In many ways Folsom was history repeating itself. European scholars in the early nineteenth century were grappling with the realization the earth was proving to be far older than a literal reading of Genesis allowed. And in those biblically-unrecorded times, as Georges Cuvier demonstrated, a whole zoo of exotic animals had roamed the earth, animals that he showed were distinct from any living animals, and must represent now-extinct genera and species (Cuvier 1796 [in Rudwick 1997]). Yet Cuvier did more than just demonstrate extinction had occurred: he used the fossils of mastodon and mammoth to set up an argument about earth and human history in such a way as to make it all empirically accessible (for a fuller discussion, see Grayson 1983, 1990). He observed that neither of these extinct elephants was ever found with human remains, but only with other species that also lacked any living analogue (a point amplified by Parkinson 1833:463–64). From that, it was but an easy step to the conclusion that this suite of now-extinct animals "prove the existence of a world previous to ours, destroyed by some kind of catastrophe" (Cuvier 1796 [Rudwick 1997:24]).

More importantly, the "unconsolidated . . . layers of the earth" in which these particular fossils were found lay close to the surface, and often had what appeared to be an alluvial origin (Cuvier 1806a [Rudwick 1997:92]).[4] It would be several decades before those deposits were recognized by Swiss naturalist Louis Agassiz as the remains of vast continental ice sheets and, in turn, linked with Lyell's Pleistocene epoch, independently defined on the basis of fossil shells as the most recent, pre-modern period. Nonetheless, even by the early 1800s Cuvier realized these were not truly ancient deposits, at least in relative terms, for there were other fossil elephants deeper still, below "regular stony [consolidated] beds, and covered by regular marine strata" (Cuvier 1806b [Rudwick 1997:97]). He concluded the earth had been inhabited at different times by different suites of animals, the most recent—which included mammoth and mastodon, but not humans—representing "the last or one of the last catastrophes of the globe" (Cuvier 1806b [Rudwick

1997:96]; Grayson 1990). This was the pre-modern world, the world before the biblical Creation. No one was looking for human remains in these Pleistocene deposits; no one expected to find any, and for good reason: whatever its age in absolute years, the Pleistocene was old relative to humans (Grayson 1983, 1990). As Lyell put it:

> The comparatively modern introduction of the human race is proved by the absence of the remains of man and his works not only from all strata containing a certain portion of fossil shells of extinct species, but even from a large part of the newest strata, in which all fossil individuals are referable to species still living (Lyell 1853:182).

The Pleistocene preceded people, was beyond range of the Mosaic chronicles, and a boundary against which human antiquity could be measured (Grayson 1983, 1990).

Hence, though it was proving to have little bearing on earth history (Parkinson 1833; Playfair 1802), the Old Testament still apparently spoke to human history. It provided a timeline of humanity's past that stretched back some 6,000 years, as calculated by biblical chronologists from Theophilus of Antioch to Archbishop Ussher (Haber 1959), the latter's calculations becoming Anglican orthodoxy and printed in the margins of the Authorized (King James) Bible (Toulmin and Goodfield 1965:76). But the Bible was more than that: it was also seen as a written record of that span, compiled by people who had either been present or had access to a supernatural informant.

On an earth that had by the early 1800s become almost inconceivably old, humanity's last refuge in the search for ultimate design and its own uniqueness and divinity lie in the affirmation of the validity of the Old Testament account (Gruber 1965:383); here, fortunately, the Bible and geology seemed to agree (Bowler 1976:31). Humans were indeed the last creation or the last in a progressive series of creations, so it seemed on good authority (Parkinson 1833:467).

Still, through the first half of the nineteenth century an increasing number of sites (reviewed in Lyell 1863; also Grayson 1983, 1990) were found with human remains alongside Cuvier's extinct animals. None of this evidence was accepted, for a number of reasons. For one, many of the claims came from continental Europe and especially France (Grayson 1990), 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 (Grayson 1983, 1990; Haber 1959; Trigger 1989). Further, much of the evidence came from the excavations of provincial amateurs (Grayson 1983:207, 1990:5–6) who were looked upon, in charitable Victorian parlance, as mere "enthusiasts" (Lubbock 1865:269). No scientist is going to reject long-held beliefs on that dubious source.

Compounding resistance, the bulk of finds were made in caves, settings ill-suited to the strict geological requirements of establishing contemporaneity (Grayson 1983):

> Must we infer that man and these extinct quadrapeds were contemporaneous inhabitants of the south of France at some former epoch? We should unquestionably have arrived at this conclusion if the bones had been found in an undisturbed and *stratified* deposit of subaqueous origin. . . . But we must hesitate before we draw analogous inferences from evidences so equivocal as that afforded by the mud, stalagmites, and breccias of caves, where the signs of *successive* deposition are wanting (Lyell 1832[II]:232, emphasis in original).

Thus, when an ochre-covered human skeleton—the so-called "Red Lady" of Paviland—was found in a cave alongside the remains of a mammoth, it was all too easy for geologist William Buckland to explain with a wink that a nearby Roman campsite threw "much light on the character and date of the woman under consideration" (Buckland 1823:90). Ochre apparently wasn't the only reason the Lady of Paviland was red (and as it happens, the "Red Lady" of Paviland was no lady, either, but an Upper Paleolithic male, now radiocarbon dated to slightly over 26,000 BP [Pettitt 2000]).

Finally, none of the evidence fit the prevailing theoretical paradigm or model of expectation—based on the Bible's account of human history and the evidence from geology—and thus there was no compelling reason to accept it (Grayson 1990:8). One does not reject a long-established and workable model without abundant and compelling reason to do so. This is clear in Lyell's own *mea culpa*, published in 1863, when he was forced by the weight of evidence to renounce his former opposition to a deep human antiquity. As he explained, "I can only plead that a discovery which seems to contradict the general tenor of previous investigations is naturally received with much hesitation" (Lyell 1863:68).

Importantly, these purportedly ancient localities were not being ignored (cf. Geike 1881:3). Lyell as a young man had visited Schmerling in the caves near Liege (Belgium) and was well aware of, and somewhat shaken by, the magnitude of the evidence; however, he and others were unwilling to take that evidence at face value (C. Lyell 1863:67–68; K. Lyell 1881[1]:402; Grayson 1983:109ff.). It is only when such anomalies become too numerous or weighty to ignore that models of the world are reassessed.

That process began in 1858, as Gruber has discussed in detail (Gruber 1965; see also Grayson 1983; Van Riper 1993), with the discovery by William Pengelly, a part-time geologist of the Torquay Natural History Society, that Brixham Cave in southwestern England contained fossil bones. The work was conducted by Pengelly and paleontologist Hugh Falconer, who saw in Brixham Cave the potential to resolve some of the details of the sequence of Pleistocene faunal change in England. Importantly, as at Folsom, the Brixham Cave research was initiated for reasons having nothing to do with archaeology, but by geologists looking to solve geological problems (Gruber 1965:385; Van Riper 1993:80–82).

To insure precise stratigraphic control and the reliability of the results from this cave, the Geological Society of London, which funded the work, established a committee of Britain's top geologists as overseers, which included Lyell, Richard

Owen, and Joseph Prestwich (Evans 1872:466; Van Riper 1993:83–85). The excavations were meticulous, done layer by layer (an unusual method for its time), and the provenience of each object was carefully measured in (Grayson 1990:9–10; Gruber 1965; Van Riper 1993:87–88). Owing to such care, work was slow, but by mid-August seven stone artifacts had been found beneath an impermeable limestone stalagmite layer, in a loamy stratum that also contained fossil bones of a variety of extinct mammals, including rhinoceros, cave bear, and hyena (Grayson 1990; Gruber 1965; Van Riper 1993).

As at Folsom, the appearance of stone tools was a surprise. By the time excavations were complete the following year, several dozen objects had been recovered, of which 15 were undeniably human artifacts (Grayson 1990). Because the site was dug with great care, there was little traction to be gained criticizing the excavation methods. Still, Brixham Cave was a cave, and that was grounds for caution (Grayson 1990:10; Van Riper 1993:94–95). In response to a paper by Pengelly on Brixham Cave delivered that fall of 1858 to the Geology Section of the British Association for the Advancement of Science (Van Riper 1993:96), Owen—then president of the section—allowed that:

> he was glad that means had been taken for the careful exploration of this cave, but it would be premature to raise any hypothesis until the whole of the facts were before them (Anonymous 1858:461).

Falconer therefore turned his attention to insuring the authenticity of the Brixham Cave specimens, and to examining other localities to see if its evidence could be duplicated elsewhere. En route to Sicily in late 1858, he visited a number of the French cave sites that had yielded stone tools and stopped in at the home of Jacques Boucher de Perthes (Van Riper 1993:100–101). Boucher de Perthes was a customs official in Abbeville in northwestern France who had, since the 1830s, been collecting in the Somme Valley the remains of extinct fauna and, apparently, associated stone tools. Many knew of his work, amply published in his two-volume *Antiquités Celtiques et Antédiluviennes* (1847 and 1857).

Yet, while many knew of his work, few believed him (Grayson 1990:9), for a substantial number of the "artifacts" he illustrated were clearly not humanly manufactured, and what genuine evidence he had was embedded in arcane catastrophist flood theories that had long since been rejected (Grayson 1983, 1990; Gruber 1965; Rudwick 1976). As John Evans gently put it:

> The announcement by M. Boucher de Perthes, of his having discovered these flint implements . . . was, however, accompanied by an account of the finding of many other forms of flint of a much more questionable character, and by the enunciation of theories which by many have been considered as founded upon too small a basis of ascertained facts (Evans 1860:281).

Boucher de Perthes was perceived as little more than a crank provincial amateur using outdated theories and collecting bogus data; some considered him "almost a madman" (Lubbock 1865:269). Charles Darwin was hardly alone in looking at Boucher de Perthes' work and thinking many of the "artifacts" were naturally flaked, and that "the whole was rubbish" (Darwin to Hooker, June 22, 1859 [in Burkhardt and Smith 1991:308]; Darwin to Lyell, March 17, 1863 [in Burkhardt et al. 1999]).

But then Boucher de Perthes' specimens had the virtue of having been recovered from deep and well-stratified alluvial deposits and thus were far less susceptible to the problems of stratigraphic mixing that plagued cave sediments. And he well understood the importance of working in "flood deposits":

> diluvial deposits do not present . . . like the bone caves, an inconcealable cavern, open to all who come, and which from century to century served as a sanctuary and then as a tomb to so many diverse beings. . . . In the diluvial formations, on the contrary, each period is clearly divided. The horizontally superimposed layers, these strata of different shades and materials, show us in capital letter the history of the past: the great convulsions of nature seem to be delineated there by the finger of God (Boucher de Perthes 1860:96–97).

All of this explains why Boucher de Perthes' evidence became compelling, once the illegitimate artifacts were discarded, and the genuine ones were divorced from their arcane theoretical context (Grayson 1983, 1990; also Evans 1860; Lyell 1863; Prestwich 1861a).

In his visit to Boucher de Perthes that fall of 1858 Falconer recognized many artifacts similar to those of Brixham Cave, and he duly reported such back to Prestwich and others in England, suggesting they go see for themselves (Falconer to Prestwich, November 1, 1858, in Prestwich 1899:119). Unlike Falconer, who by interest and long experience was more comfortable in cave sites, Prestwich was well versed in the Quaternary alluvial stratigraphy of northwestern France. Heeding the call, Prestwich and Evans visited Boucher de Perthes in late April of 1859, where they witnessed and photographed the removal of a handaxe *in situ* from a locality in Amiens, and collected a few more handaxes from the workmen (Evans 1943:101–2; Prestwich 1899:123–24; Van Riper 1993:104–6).

At a meeting of the Royal Society scarcely a month after their return to England, Prestwich read a paper on the stratigraphy of the Somme Valley glacial gravels (Prestwich 1860, 1861a), and Evans gave an extemporaneous talk on the artifacts. They were well received, as Evans recalled:

> There were a good many geological nobs there, Sir C. Lyell, Murchison, Huxley, Morris, Dr. Perry, Faraday, Wheatstone, Babbage, etc. so [we] had a distinguished audience. Our assertions as to the findings of the weapons seemed to be believed (in Evans 1943:103).

Evans underrates his audience. It included not only members of the geological elite (Lyell, Huxley, Murchison), but also one of the greatest experimental scientists of all time, who developed the electromagnetic field theory (Faraday); the inventor of the stereoscope, and a pioneer researcher in acoustics, electricity and telegraphy (Wheatstone); and one of the trio who revolutionized English mathematics in the nineteenth century with the introduction of Leibniz's differential calculus, and a dabbler in cryptanalysis, probability theory, geophysics, astronomy, and computing machines (Babbage). Geological nobs, indeed. It might not have been possible to gather a more influential group of scientists in all of England. Clearly, the question of human antiquity was out of the closet and on center stage before the major scientists of the day (Grayson 1983). A favorable reception in this rarefied atmosphere could only help the cause.

Prestwich and Evans's papers (Prestwich 1860; Evans 1860), coupled with the evidence from Brixham Cave and the testimony of others (e.g., Falconer 1860; Flower 1860), was enough to prompt a stream of cross-channel and in-country visitors to examine Boucher de Perthes' sites in the Somme Valley (Grayson 1990:10; Van Riper 1993:111–13). Lyell's pilgrimage to Abbeville was by far the most symbolic, since he had long cherished a belief in the recency of human antiquity and, as Hrdlička later would, criticized any claims to the contrary. Yet, he too returned from France a convert:

> I am fully prepared to corroborate the conclusions which have been recently laid before the Royal Society by Mr. Prestwich. . . . I believe the antiquity of the Abbeville and Amiens flint instruments to be great indeed if compared to the times of history and tradition (Lyell 1860:94).

Lyell announced that conversion in September 1859 to a meeting of the British Association for the Advancement of Science, of which he was then president of the Geological Section. As the premier geologist of the nineteenth century, his announcement, like Kidder's 70 years afterward, carried extraordinary weight (Grayson 1983; Van Riper 1993:115).

Where the idea of a deep human antiquity had just months before been the dubious claim of provincial amateurs, it was now almost universally accepted fact (Grayson 1983; cf. Van Riper 1993:117ff). As one observer put it, people were no longer insisting "it was not true" or that "it was contrary to religion," but that "it was all known before" (Dawkins 1863). Smelling blood, Leonard Horner took the opportunity to publicly denounce as "untrue, and therefore . . . mischievous" (Horner 1861:lxviii) the inclusion of Ussher's biblical chronology in the margins of the King James Bible, giving that human chronology a misleading air of divine authority.[5] But perhaps the best barometer that a deep human antiquity had suddenly become conventional wisdom was its serving as fodder for satire by "Gorilla" (Philip Egerton) in the pages of *Punch* (Gorilla 1861:206):

> LEONARD HORNER relates,
> That Biblical dates
> The age of the world cannot trace;
> That Bible tradition,

By Nile's deposition,
Is put to the right about face.

Then there's PENGELLY,
Who next will tell ye
That he and his colleagues of late
Find celts and shaped stones
Mixed up with cave bones
Of contemporaneous date.

Then PRESTWICH, he pelts
With hammers and celts
All who do not believe his relation,
That the tools he exhumes
From gravelly tombs
Date before the Mosaic creation.

To be sure, there were matters to be clarified (Van Riper 1993), and a brief debate played out in the pages of popular periodicals such as the *Athenaeum* and the *Gentlemen's Magazine*. But these were primarily over details of geological context and the precise age of the associated fauna, rather than over the authenticity of the claims (e.g., Babbage 1860; Ramsay 1859; Worsaae 1859; Wright 1859). There were a small number who raised larger questions (e.g., Anderson 1859; Trevelyan 1859), but as Gruber observes, "the very shrillness of their tone betrayed the weakness of their position" (Gruber 1965:374). The scientific journals of the day contained reports of new sites that expanded the empirical record and provided further support of a deep human antiquity (e.g., Christy 1865; Cochet 1857–1860, 1861; Dawkins 1862, 1863; Evans 1863, 1864; Lartet 1860; Prestwich 1861b). With the publication in 1863 of Lyell's masterful and wide-ranging synthesis, *Geological Evidences of the Antiquity of Man*, the case was sealed.

RESOLUTION AND REVOLUTION

The establishment of human antiquity in Europe and, seven decades later, in America, profoundly altered the course of archaeology on each continent (Grayson 1983; Meltzer 1983; Van Riper 1993).

In Europe, the Bible and written history, as well as the nationalistic approaches of the antiquarians (Trigger 1989), had served as the foundation on which the understanding of humanity's past had rested. In the late 1850s, that understanding was suddenly rendered completely irrelevant to that 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 (Lubbock 1865:2). The exotic pre-modern world that Cuvier had revealed half a century earlier had indeed been populated by humans (Grayson 1983).

As that ancient past would be knowable only through artifacts and bones, a new discipline of *pre*history, with its own body of theory and methods, amply infused by geology, had to be invented. There were newfound concerns for questions of chronology, for there was no longer a tidy 6,000-year frame around humanity's shared past. The question of whether humanity was old was soon replaced by *just how old?* and *how far back in geological antiquity did humans first appear?* (Van Riper 1993).

Having learned what to look for in the archaeological record (Prestwich 1861b), answers came rapidly, as dozens of Paleolithic sites were found across Europe throughout the 1860s. Humanity's roots were soon pushed back deep in time: hundreds of thousands of years, some suggested, perhaps millions of years, others supposed (Lubbock 1865; also Grayson 1983). What had transpired over that intervening span was for a long time poorly known, testament to the vast "chasm which separates the flint folks from ourselves" (Dawkins 1863:219; also Christy 1865; Dawkins 1874, 1880; Evans 1863; Geike 1881; Lartet and Christy 1875). Prehistorians had to grapple with questions of how best to identify and chronologically sort—whether through geological, faunal, or artifact criteria—the cultural periods of the human past that filled that void (e.g., Evans 1872; Lartet and Christy 1875; Stevens 1870; also Sackett 1981).

Julia Wedgwood, Darwin's niece, saw "something dreary in the indefinite lengthening of a savage and blood-stained past" (Grayson 1983:217). But however dreary or theologically unnerving a deep human antiquity might be, those rude artifacts were also vivid testimony of the savage depths from which humanity had climbed (Stocking 1987), and they became for many a triumphant demonstration of social evolutionary progress and, conversely, the death-knell of the view that humanity had degenerated from an ancient Golden Age (Grayson 1983:218).

Not surprisingly, the notion of a deep human antiquity was soon entangled in the theory of evolution by natural selection, as had been laid out independently and almost simultaneously in Darwin's *On the Origin of Species* (1859). Lyell made his first public mention of Darwin's views in his 1859 *mea culpa* address to the British Association (Lyell 1860), and even Gorilla (1861) linked the two in *Punch*:

Then DARWIN set forth
In a book of much worth,
The importance of "Nature's Selection,"
How the struggle for life
Is a laudable strife,
And results in "specific distinction."

Let pigeons and doves
Select their own loves,
And grant them a million of ages,
Then doubtless you'll find,
They've altered their kind,
And changed into prophets and sages.

The *Origin*, of course, had no more to say about the evolution of the human species from our animal forebears than that breathtakingly understated one-liner "Light will be thrown on the origin of man and his history" (Darwin 1859:488). But everyone knew what *that* meant: a shared ancestry with other primates, and pre-*sapiens* ancestors deep in time. One could scarcely accept Darwin's views of human evolution without the vast chronology that the new-found prehistory provided. In turn, Darwin "provided a theoretical framework within which a tremendous human antiquity could be understood [and] questions of human antiquity quickly became caught up in discussions of the larger issues of human evolution" (Grayson 1980:372). This is not to say the acceptance of a deep human antiquity was in any way linked with the acceptance of Darwin's ideas, or vice versa (Gruber (1965); many who accepted a deep human antiquity were still creationists, or at least not evolutionists (Grayson 1983; cf. Trigger 1989:87). Darwin had actively followed the emerging human antiquity story (e.g., Darwin to Hooker, October 6, 20, 1858, June 22 and July 2, 1859; Darwin to Lyell, September 2, 1859 [in Burkhardt and Smith 1991]), and a decade later when he finally tackled the topic of human evolution publicly, 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 . . . (Darwin 1871:3).

That's where the search began on the other side of the Atlantic, but 70 years later it was obvious to most—save Cook and Figgins—that American prehistory did not have a Pleistocene antiquity comparable to that of Europe (Meltzer 1983). Still, Folsom expanded America's prehistoric time-scale, and likewise challenged prehistorians here to fill in the chasm between the known (the Late Prehistoric), and the remote unknown—the Late Pleistocene. Meeting that challenge involved a variety of new methods and approaches (e.g., McKern 1939) and triggered the rise of Culture History, which dominated American archaeology through the middle of the twentieth century (Meltzer 1983:40).

It also changed the traditional relationship of archaeology to anthropology. Previously, it had seemed the past survived up to the present, as the people who made and used stone tools had not entirely disappeared (Haven 1864:37). For many archaeologists, the American Indians "were here and must be recognized by every theory, must be a factor in every general conclusion" (Thomas 1898:22). Past and present were not perceived as being qualitatively distinct; the archaeological record seemed to vary more on a spatial (ethnographic) dimension than a temporal (archaeological) one (e.g., Holmes 1919).

The chasm that opened between the Late Pleistocene and the Late Prehistoric thus raised knotty questions about the relationship between ancient archaeological cultures and historically known Native Americans. Was ethnographic data, long used to explain the archaeological record of the Americas (Meltzer 1991), relevant to anything but the latest part of the prehistoric record? The answer was unclear, though one fact seems certain: the archaeological use of ethnohistory and ethnographic analogy was never quite the same afterward.

Folsom also provided much-needed "chronological elbow-room," and that solved a host of problems, as a relieved Kidder explained (Kidder 1936). Foremost for him, the depth provided by Folsom de-clawed the diffusionist claim that there wasn't enough time for the New World civilizations to have developed independently. For those like Kidder who believed in the indigenous development of complex society in the Americas, this was welcome news (Kidder 1936).

Finally, and as in Europe, additional ancient sites came rapidly. Folsom had taught archaeologists how to find more sites like it: look for bones of extinct animals, bison or mammoth, and then examine the spot for associated stone artifacts. Within the decade, nearly a dozen more Paleoindian sites were found, in which fluted points were associated with the remains of extinct animals. Most of these, of course, were kill sites, setting the interpretive precedent (Binford 1981) that fluted points (Folsom and otherwise) were weapons for killing large game, which strongly influenced emerging views of Paleoindian subsistence strategies (Meltzer 1989).

THE ESTABLISHMENT OF PRE-CLOVIS IN THE AMERICAS, 1997

One of the kill sites discovered in the wake of Folsom was at a spring-fed pond near the High Plains town of Clovis, New Mexico, where larger, less-finely-made fluted points were found associated with mammoth remains. Clovis artifacts proved to be more widely distributed than Folsom and were typologically broader and less refined. To a generation raised on the age-area hypothesis, these were clues that Clovis must be older. Within a decade, that was shown to be true on stratigraphic grounds, and a few years later was confirmed using the newly invented technique of radiocarbon dating (Cotter 1938; Libby 1955; Sellards 1952).

If Clovis was earlier than Folsom, what of the first Americans? Were they Clovis Paleoindians, or had they arrived in pre-Clovis times? In the aftermath of the Clovis discovery, archaeologists began to search for traces of still older Americans. But by 1953, Alex Krieger was already feeling pinched. He worried that archaeologists, having overthrown the Holmes-Hrdlička "dogma," were in danger of replacing it with another. The first Americans were apparently being permitted a late Pleistocene entry, but perhaps no earlier: Krieger worried that 10,000 to 15,000 years before present was fast becoming the new "allowed antiquity" (Krieger 1953:238–39).

Nevertheless, in 1953, Krieger identified half a dozen sites he thought "may and probably do" surpass that barrier. A decade later, he upped the total to fifty sites in North and South America that he believed pointed to a human presence predating Clovis (Krieger 1964). Not all sites are created equal, and Krieger appreciated that fact. Some had radiocarbon ages ranging upwards of 38,000 BP (Lewisville, Texas [Crook and Harris 1957, 1958]); others had bones of Pleistocene fauna that appeared split, burned, or broken by human hands (e.g., Santa Rosa Island, California [Orr 1962]); and some of the sites of the American

Paleolithic, Trenton included, in Krieger's view demanded another look: they "cannot all be set aside as insignificant" (Krieger 1964:44; see Meltzer 1994:19).

In fact, many of the sites on Krieger's list recalled those of the American Paleolithic: they contained simple stone or bone artifacts and lacked more "advanced" projectile points. Krieger insisted he was not 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 onto his speculative limb; others were busy sawing it off behind him.

Why not? The idea that Clovis Paleoindians were the first Americans neatly conformed to emerging geological and radiocarbon evidence. The same year Krieger published his pre-Clovis compendium, Haynes (1964) reported the first secure radiocarbon ages for the Clovis occupation. By then, Clovis artifacts were recorded throughout the coterminous United States, yet virtually all dated sites fell in a very narrow slice of time, between 11,500 and 11,000 years BP. None were more than 12,000 years old. As it happens, geologists had at about that same moment declared that 12,000 years ago, for the first time in 15,000 years, warming climates had melted the great glaciers and an ice-free corridor had opened between them, allowing passage between Alaska and the lower 48 states (Broecker and Farrand 1963; Haynes 1964).

It all made perfect sense: the land bridge connecting Siberia and Alaska only emerged during glacial cycles, but once migrants reached Alaska, the glaciers blocked their path south. Either the first Americans came before the last major ice advance, in which case they had to contend with crossing open seas, or they walked across the land bridge, then waited in Alaska for the ice sheets to retreat. The splendid correlation then existing between the disappearance of the glaciers and the appearance of Clovis seemingly favored the latter hypothesis (Haynes 1964).

Still, over the next several decades sites were discovered that potentially provided evidence of a pre-Clovis human presence on this continent (Meltzer 2004). Some were heralded with great fanfare: Louis Leakey, fresh from his triumphant discovery of two-million-year-old hominids at Olduvai Gorge in Tanzania, proclaimed the Calico site in California's Mojave Desert site to be Middle Pleistocene in age (Leakey et al. 1968). Yet, Calico was set in the middle of a vast alluvial fan, through which the excavation had plunged. The site's artifacts were plucked from amidst hundreds of thousands of naturally broken stones.

At the suggestion of an outside panel of reviewers (which included C. V. Haynes), the Calico excavators saved all specimens, whether identified as artifacts or not (Morrell 1995). As the excavations continued, the number of Calico artifacts increased, but then so did the pile of rocks tossed aside as non-artifacts—and that was surely no coincidence. Given the site's geological context, Vance Haynes (1969b, 1973) supposed that none of the site's specimens were humanly made, but instead all were *geofacts,* produced naturally as rocks tumbled downstream from sources in the Calico Mountains. Among their vast numbers, it was inevitable that a small proportion resembled primitive stone tools.

Haynes's criticisms sparked a lively discussion over the minutiae of flaked stone artifacts: did the Calico pieces possess uniquely human attributes (e.g., Patterson 1983; Payen 1982)? Unfortunately, as Toth (1991) observed, Calico was “a worst case” setting for documenting a human presence, given its backdrop of millions of naturally fractured stones (also Grayson 1986). An international conference and site visit was held to evaluate the Calico material in 1970; much to Leakey’s dismay no one believed him (Morrell 1995).

Other claims were made by lesser mortals than Louis Leakey, and over the next several decades dozens of ostensibly pre-Clovis age sites across North and South America were reported (see Bryan 1978, 1986; Dincauze 1984; Ericson et al. 1982; Humphrey and Stanford 1979; Irving 1985; MacNeish 1976; Morlan 1983, 1988; Stanford 1983). Each of these contenders was evaluated to determine whether it had (1) genuine artifacts or human skeletal remains in (2) unmixed geological deposits, accompanied by (3) reliable pre-Clovis age radiometric ages (Dincauze 1984; Haynes 1969a). These are criteria that, in one form or another, had been used for over a century to evaluate ostensibly ancient sites (Chamberlin 1903; Hrdlička 1907) and are in use in other parts of the world as well (Toth 1991). By these criteria, virtually all the sites proved flawed, and fatally so (Dincauze 1984; Meltzer 2004). Once again, archaeologists grew profoundly skeptical of claims for a deeper human antiquity than conventional wisdom allowed.

But then the site of Monte Verde appeared on the scene. Discovered in the mid-1970s near Puerto Montt, in southern Chile, it was excavated over an eight-year period by Tom Dillehay and a large, interdisciplinary research team (Dillehay 1989a, 1997). The MVII component at the site yielded an extraordinary array of inorganic and organic artifacts and features. These included wooden foundation timbers and pegs used to constructed a series of huts; wooden mortars containing charred and uncharred skins and seeds of edible plants; finely woven string; a wide range of plants, some exotic, some chewed, some in presumed human coprolites; hearths with burned and unburned plant and animal remains; and the burned and/or broken and split bones of mastodon, along with pieces of its meat and hide (some of the hide adhering to wooden timbers, the apparent remnants of coverings that once draped over the huts). Also found were hundreds of stone, bone, tusk, and wooden artifacts—some with plant residues and tar obtained from the distant coast still adhering to surfaces (Dillehay 1997). Indeed, owing to its spectacular preservation, the majority of the MVII artifacts and other materials are organic remains, not stone tools, surely indicative of the proportion of the non-stone tool component missing from other, more poorly preserved sites. The MVII component dated to approximately 12,500 BP (Dillehay 1989a, 1997).[6]

Through the 1980s Monte Verde attracted increasing attention from the archaeological community, and it was highlighted in a lengthy presentation and detailed discussion at the 1989 World Summit Conference on the Peopling of the Americas (held at the University of Maine, in Orono). Dillehay publicly extended an invitation to visit the site (Dillehay 1989b), the value of which Haynes quickly seconded (Haynes 1989). Dillehay, Haynes, and Meltzer began organizing a visit in the early 1990s; it finally occurred in January of 1997, coinciding with the appearance of Dillehay’s second volume on the site (Dillehay 1997).

Like all such events (Meltzer 1994), the Monte Verde site visit had its share of awkward and disagreeable moments (recounted without varnish in Adovasio and Page 2002; see also Gore 1997). In the end, however, participants agreed that the MVII component at the site was indeed archaeological and 12,500 years old. A statement to that effect was drafted some months after the visit, to allow for a re-reading and re-examination of Dillehay's volumes, and to let any bruised egos or twisted arms heal. The draft was circulated for comment and signature by the participants, *if* they agreed and were comfortable with its conclusions. All the participants signed, and the paper was published (Meltzer et al. 1997). To be sure, many had lingering questions about the character of the artifact assemblages and their interpretation, but that did not change the central conclusion about the antiquity of the site (Meltzer et al. 1997; also see Adovasio and Pedler 1997). Given Monte Verde's antiquity and distance from the Beringian gateway, that conclusion had profound implications for the antiquity of the Americas (Adovasio and Page 2002; Jablonski 2002; Madsen 2004), just as Folsom had before it (and as Brixham Cave and the Somme Valley sites had on European prehistory the century before).

CONVERGENCE AND DIVERGENCE IN CONTROVERSY AND ITS RESOLUTION

These three episodes are similar and different in important ways. In all three, excavations were conducted for reasons other than trying to push back human antiquity. Indeed, at Brixham Cave and Folsom, it was neither known nor anticipated that these sites were archaeological (Gruber 1965; Grayson 1983:182; Meltzer 1991). As noted, these excavations were initiated for paleontological purposes. Monte Verde was initially thought to be just another Paleoindian locality (Dillehay 1997).

In all three cases, the crucial evidence emerged in a climate of profound skepticism on the part of the scientific community, owing to the repeated failure of many prior claims to pass critical muster (Grayson 1983; Meltzer 1983). Skepticism may have been deepest in Europe in the 1860s, as there was no a priori presumption that human remains would or should be found in Pleistocene deposits (Grayson 1990). In retrospect, of course, Paleolithic handaxes might seem obviously of another age, perhaps made by a people "who had not the use of metals," at a "very remote period indeed, even beyond that of the present world" (Frere 1800), but such evidence could have been and readily was accommodated within the existing worldview (Grayson 1983). In contrast, even steadfast critics of human antiquity in the Americas were open to the possibility of an earlier human presence than conventional wisdom allowed (e.g., Holmes 1897; Haynes 1969a). That this was so was partly based on *non*-archaeological evidence from linguistics (e.g., Goddard 1927; Nichols 1990) and, beginning in the late 1980s and early 1990s, human genetics (e.g., Torroni et al. 1992, 1993). Importantly, in all three cases, the sites that finally provided resolution did not just meet but far exceeded the evidentiary criteria by which such sites are judged (Haynes 1969a; Hrdlička 1907).

There are obvious differences too. The Brixham Cave excavations shook loose the Bible's stranglehold on human history, and it was an event that had a farreaching impact on Western thought (Grayson 1990; Toulmin and Goodfield 1965). Folsom shattered the shallow past most scholars accorded American prehistory, but the reach of its implications only extended to the disciplinary borders of anthropology. Monte Verde only tells us that the first Americans arrived a thousand or so years earlier than we used to think, a fact which, candidly, hardly matters in the grand scheme of human prehistory (Meltzer 1995). Nowadays we're playing the game for remarkably low stakes, though it sometimes seems we're playing for keeps (Adovasio and Page 2002; Fiedel 1999).

The two earlier episodes were based on artifacts in close association with the remains of extinct fauna, the only secure means then available for telling time. Brixham Cave produced Paleolithic artifacts sealed beneath a thick and virtually impervious layer alongside the bones of a Pleistocene fauna, a discovery which prompted a closer look at Boucher de Perthes' artifacts with extinct mammals in deep alluvial deposits of the Somme Valley. Folsom yielded artifacts between the ribs of an extinct bison, which could only have become embedded when that animal was alive. In neither case was it possible, as it had been so many times before (Meltzer 1991), to argue that the association of the artifacts with the remains of the extinct fauna was merely fortuitous. Mastodon remains were present at Monte Verde, and these prompted the initial attention given to the site (Dillehay 1989a), but ironically, this was not the Paleoindian kill site it was first suspected to be. The association of artifacts (in this case, of both stone and wood) with the remains of an extinct mammal was important in demonstrating the stratigraphic integrity of the site, but that association was not critical to establishing its antiquity, as the site's artifacts and features could be and were directly radiocarbon dated.

More differences: in the earlier two episodes there was a sharp division of labor and scientific status between those who made the discoveries (e.g., Boucher de Perthes and Pengelly; Cook and Figgins), and those who were called upon to judge the significance of the discovery (e.g., Evans, Lyell, and Prestwich; Brown, Kidder, and Roberts). As Rudwick (1985) and others (e.g., Grayson 1983, 1990; Oldroyd 1990) have amply demonstrated, controversy in science—or at least non-trivial controversy—is not resolved simply by gaining consensus across the community. Rather, resolution is brought about by a core elite within the community who are recognized as expert in the field, even if they are not particularly involved in the research within that particular area (Oldroyd 1990:348). Not all scientists are created equal; some are more equal than others. That inequality is perhaps most visible during episodes of controversy, when the stakes are highest (Oldroyd 1990:345).

Thus, in both Europe and early twentieth century America the discoverers were relegated to second-class status because of their propensity to make absurd arguments about their findings, or cloak them in arcane theoretical contexts. Boucher de Perthes, a Noachian flood-monger when such was no longer intellectually fashionable, was lionized for *what he found*, but not *what he said* about what he found (Grayson 1983:128). Prestwich may have "agreed essentially" with Boucher de Perthes on some matters, but he admitted that "on the theoretical questions I differ materially" (Prestwich 1861a:308). In America, Nels

Nelson sternly lectured Figgins that if everything he and Cook said was true, "we shall have to revise our entire world view regarding the origin, the development, and the spread of human culture" (Nelson to Figgins, August 16, 1927, JDF/DMNS). Nelson was not ready to do that. Few were.

The testimony of a towering figure like a Lyell or a Kidder, who had seen and evaluated the material first hand, and could speak to the meaning of those discoveries, was critical and widely influential. In contrast, the role of a Boucher de Perthes, Cook, or Figgins was to make the discoveries, then get out of the way. Thus, from 1927 to 1937 there were seven major symposia devoted to human antiquity in the Americas. In no case was Cook or Figgins a participant; it was only on two occasions that they were even suggested as possible speakers, but in both cases the suggestions were ignored (Meltzer 2006). Their absence shows that *discovery* and *resolution of controversy* were in these instances separate events, involving very different participants. It was not true, as one wag suggested, that Hrdlička would not be satisfied with Folsom unless he had "fired the arrow himself" (Lucas to Figgins, November 18, 1927, DIR/DMNS), but Hrdlička did want to know who was there when the arrow was unearthed. At some level, Boucher de Perthes, Cook, and Figgins understood their place. As Figgins privately admitted:

> I feel this museum [of which Figgins was director and de facto voice] should avoid all expression of opinion concerning its finds and that silence is golden. The evidence must go before the final jury for ultimate opinion and for that reason our opinions are valueless (Figgins to Gregory, December 30, 1927, DIR/DMNS).

This is not to suggest any of them were happy with that knowledge; indeed, all carried considerable chips on their shoulders. Boucher de Perthes' own view of the reception of his work is not without irony:

> Those who did not attack my religious beliefs accused me of temerity. What? An unknown archaeologist, a geologist without a diploma—a strange pretension indeed to attempt subverting a system confirmed by long experience, and adopted by the most eminent men of science (quoted in Anonymous 1863:80).

Still, as Hull observes, "neither humility nor egalitarianism has ever characterized scientists, and no one has ever given any good reasons why they should" (Hull 1988:31).

Monte Verde is a different case altogether. Tom Dillehay was no provincial amateur, but a degreed professional, who, although relatively young at the time he began work at the site, already had broad experience and numerous publications. He involved in the Monte Verde research a large and well-qualified team of interdisciplinary specialists. And the presentation of the emerging research results did not rely—as it did in the earlier two instances—on the efforts of others who were not involved in the work, or who had influence that went beyond those who were. Rather, the results were presented by Dillehay himself, in a steady stream of publications in top-tier scientific journals as well as books (e.g., Dillehay 1984, 1987, 1989a, 1997; Dillehay and Collins 1988, 1991).

That being the case, what of the role of our site visit, as opposed to those that occurred on earlier occasions? Parallels have been drawn between the Monte Verde visit of 1997 and the ones at Folsom in 1927, and Abbeville seventy years earlier, but the cases are hardly identical (also see Haynes 1999). In those earlier times, archaeological methods and techniques were far more uneven, training was spottier, more amateurs were in the mix at critical sites, the criteria for evaluating evidence were less explicit, and evidence had to be evaluated in the field (Meltzer 1994). Indeed, determining the age of a site required an examination of the site's stratigraphy and geology, artifact context, and the nature of the associated remains: just as at Folsom, Brixham Cave, and in the sites of the Somme Valley.

To be sure, examination of such matters is equally critical today, but there is now an asymmetry to the process: site visits can be sufficient to *reject* a claim of great antiquity, but they cannot *demonstrate* one (Meltzer 1994:18). Indeed, in 1997 a site visit could have been seen as something of an anachronism, even almost irrelevant, as so much of the evidence emerges in post-excavation analysis of radiocarbon samples, sediment chemistry, artifacts, the isotopic composition of organic remains, and so on. The Monte Verde site visit mostly provided an opportunity to examine the site's setting and surroundings, sediments and stratigraphy, the potential for sample contamination or mixing, and the like (Meltzer et al. 1997). It could not be definitive in regard to the site's antiquity, as such visits had been on those earlier occasions.

That being the case, and given that the discipline has advanced to the point where professional competence can usually be assumed, and that field and laboratory work follows well-defined and accepted protocols that can be widely evaluated without having to look over the shoulders of those doing the work, it begs the awkward question: what was our role as participants in the Monte Verde site visit? I want to tread carefully here, not out of some sense of false modesty, or to avoid the inevitable and invidious comparisons between participants then and now, but rather because the nature of the scientific community has changed (Meltzer 1994).

As a co-organizer of the Monte Verde visit, I cannot deny it was important to have the participation of Vance Haynes, who has long been accused of being a latter-day Hrdlička (which, frankly, I consider a compliment). That said, because we now have agreed-upon standards of evidence, we need not wait for judgment from on high. I and other participants on the Monte Verde visit saw our role as individuals knowledgeable about the archaeology of this period, not as a "supreme court" of the first Americans. We reported our observations, and we urged others to judge for themselves based on their own evaluation of the evidence compiled in Dillehay's two volumes on the site (Meltzer et al. 1997).

Many did just that, and the fact that some reached very different conclusions than we did (e.g., Fiedel 1999) is clear testimony that our say-so following our site visit did not carry the weight that Lyell's or Kidder's had 140 and 70 years earlier. So too is the fact that Haynes has since had second thoughts (Haynes 1999), yet this has not reversed the tide of opinion on Monte Verde. In this respect, the sea change in thinking that followed Monte Verde is reminiscent of that which took place following those earlier episodes, showing that today, as on those earlier occasions, once the antiquity genie is out of the bottle, it won't go back in. In any case, the message is clear: the structure of the discipline is far less hierarchical than it was decades and centuries ago.

What may in the end prove most interesting in comparing the episodes of the 1850s, 1920s, and 1990s will be what happens over the long term. Although just 1,000 years older than Clovis, Monte Verde's distance from the Beringian entryway, as well as its decidedly non-Clovis look, raises a flurry of questions about who the first Americans were, where they came from, when they crossed Beringia, how they made it south from Alaska, and why (at the moment at least) the oldest site in the New World is about as far from Beringia as one can reach, with no sites in between proven to be as old or older (Meltzer 2002).

Complicating matters, the rare geological circumstances and unusual archaeological record of Monte Verde does not lend itself, as Brixham Cave or Folsom did, to ready generalizations about how or where more sites like it will be found. Moreover, the archaeology of the Americas is far less simple than it was in 1927, for now we have to understand what these distant South American assemblages may mean relative to Clovis, whether they represent the same or different colonizing pulses (the current crop of pre-Clovis claims looks nothing like what we see at Monte Verde), and then take on the task of explaining why, if people were in South America well before Clovis times, we've yet to find their traces in North America. And in the course of doing that, we'll have to continue to make headway in understanding the timing and position of the entry routes into and through the Americas, the speeds by which hunter-gatherer groups may have traveled across an utterly unknown landscape, the barriers that might have slowed them along the way, and tease out other variables we've yet to define, let alone measure (Meltzer 2002, 2004).

Moreover, if Monte Verde is pushing the limits of antiquity on this continent, as our geneticist colleagues suggest (e.g., Zegura at al. 2004), then the likelihood of finding scores of sites like it diminishes (Meltzer 2004). Even so, I can only assume that if history is any guide, there will emerge a new understanding of Pleistocene peoples in the coming decades—just as Brixham Cave and Folsom brought about in their time—and that the race will soon be on to find still older sites in the Americas, if it's not already. Perhaps more than anything else it is that continuing cycle that will make the events at Brixham Cave, Folsom, and Monte Verde in the end look much the same.

I'll try to report back in 70 years on how it turns out. . . .

NOTES

I would like to thank Lawrence Straus for inviting me to the University of New Mexico in the spring of 2005, to present the XXth JAR Distinguished Lecture, from which this paper is derived (and which, for lack of self-discipline on my part, retains something of the informal style in which it was delivered). Lawrence had asked for a talk that would encompass my archaeological fieldwork at the Folsom site, and also incorporate the larger issue of the peopling of the Americas. I'm not sure I hit that high mark, but Lawrence and my friends and colleagues at UNM were wonderfully hospitable all the same, and I thank them for a most enjoyable visit.

My research into the history of the human antiquity controversy has been supported by the National Science Foundation and the Smithsonian Institution; my archaeological research at Folsom and, more broadly, on North American Paleoindians and the peopling of the Americas is supported by the Potts and Sibley Foundation and the Quest Archaeological Research Fund. This paper has benefited from the wise counsel and constructive comments of Lawrence Straus and Donald Grayson.

1. Absolute ages in this paper are presented as radiocarbon years before present.

2. The discussion in this section is derived in part from the historical chapter in my forthcoming volume on archaeological investigations at the Folsom site (Meltzer 2006).

3. Abbreviations used for unpublished archival sources are listed at the beginning of the References Cited.

4. Cuvier was at pains to show this was not an alluvial origin from some great flood, which might reopen the possibility these animals hadn't lived here but were swept in from tropical regions. As he showed, those deposits occurred on low-lying plains, and not the tops of mountains, and the bones were not waterworn. For that matter, there were simply too many fossils for people to have brought them in (Cuvier 1806, in Rudwick 1997:92–93).

5. A surprised Charles Darwin, after reading that the dates were actually the work of the merely-human Archbishop Ussher, wrote Horner to say, "How curious about the Bible! I declare I had fancied that the date was somehow in the Bible" (Darwin to Leonard Horner, March 20, 1861 [in Burkhardt et al. 1994]).

6. The much older MVI component in a nearby part of the site is neither as well preserved nor as rich in artifacts as MVII, and Dillehay is understandably circumspect about its status (Dillehay 1997).

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BAE/NAA: Bureau of American Ethnology Papers, National Anthropological Archives, Washington, D.C.

BB/AMNH: Barnum Brown Papers, American Museum of Natural History, New York

DIR/DMNH: Papers of the Director, Denver Museum of Natural History, Denver

HJC/AGFO: Harold J. Cook Papers, Agate Fossil Beds National Monument, Scottsbluff, Nebraska

HJC/AHC: Harold J. Cook Papers, American Heritage Center, University of Wyoming, Laramie

JCM/LC: John C. Merriam Papers, Library of Congress, Washington, D.C.

JDF/DMNH: Jesse D. Figgins Papers, Denver Museum of Natural History, Denver

OPH/SIA: Oliver P. Hay Papers, Smithsonian Institution Archives, Washington, D.C.

VP/AMNH: Department of Vertebrate Paleontology Papers, American Museum of Natural History, New York

WHH/SIA: William Henry Holmes Papers, Smithsonian Institution Archives, Washington, D.C.

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Journal of anthropological research. Author(s): University of New Mexico. NLM Title Abbreviation: J Anthropol Res. ISO Abbreviation: J Anthropol Res. Title(s): Journal of anthropological research. Continues: Southwestern journal of anthropology ISSN 0038-4801.Â Continues Southwestern journal of anthropology. Other ID: (DNLM)J12620000(s) (OCoLC)01787802. NLM ID: 0403466 [Serial]. Supplemental Content.