CLOCKING THE FIRST AMERICANS

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ABSTRACT
The antiquity of the first Americans is one of the most controversial issues in American archaeology, and it must be resolved to understand fully the adaptive radiation of Homo sapiens into the New World. Humans were in the Americas at least by Clovis times 11,200 years ago. Accepting that these were the first Americans, however, is complicated by claims of an even earlier presence, by the absence of a Clovis source (i.e. an Alaskan predecessor is lacking), and by the theoretical demands of explaining how or why Clovis groups apparently migrated rapidly through the hemisphere. New models from evolutionary ecology, along with possible changes in the Clovis chronology (resulting from improved radiocarbon calibration), may address some of these anomalies. But there still remains the possibility of an earlier (pre-Clovis) entry, supported by some mtDNA and archaeological evidence. The mtDNA evidence, however, is complicated by questions about the viability of the presumed founder effect (on which the mtDNA clock is based). Also, the several possible pre-Clovis archaeological sites have not yet been accepted. Resolution of the timing of the peopling of the Americas, on which several theoretical and methodological issues hinge, remains a question of developing archaeological evidence, resolving ambiguity in analytical techniques and dating methods (and rejecting ones that now appear critically flawed), and expanding search strategies. The Monte Verde site in Chile is the most viable pre-Clovis candidate, although for now neither it nor any other site resolves when and by which route humans first came to the Americas.
Introduction

Ten years ago, Irving (67:538) warned that evidence from sites such as Calico, Old Crow, San Diego, and Valsequillo, ostensibly showing that people reached the Americas as early as 150,000 years ago, could not be disregarded “without undermining the empirical basis of our discipline.” A decade later, American archaeology is still standing strong. The same cannot be said for the claims from those sites.

But failed claims for great antiquity are nothing new. Since the Clovis benchmark of human antiquity in the Americas was established in the 1930s, each decade has seen new claims of even older sites (98). Eventually, however, nearly all these contenders fade under the harsh glare of critical scrutiny. Of the 50 sites identified in 1964 as older than Clovis, only 4 made a 1976 version of that list, and none made a 1988 list (74, 84, 102). The shelf-life of pre-Clovis claims seems little more than a decade (90). To date, there is universal agreement only that the first Americans were Homo sapiens sapiens, who were in North and South America by Clovis times, 11,200 years ago.

The inability to prove otherwise has bred considerable controversy and occasional ill will within the archaeological community. Pre-Clovis proponents accuse their critics of creating a “Kafkaesque” situation, in which there are good and bad archaeologists, the latter being those who make pre-Clovis claims (53). Critics wonder aloud whether the proponents’ haste to find pre-Clovis sites marks a “basic flaw in the motivational structure of academia” (83:270). The controversy has broadened and intensified lately (75) because of growing anomalies in the Clovis-first model; because genetic and linguistic evidence seemingly supports a pre-Clovis entry; because several pre-Clovis sites have successfully cheated archaeology’s actuarial tables and remain viable after a decade of scrutiny; and because, cynics would add, the stakes are so low.

The cynics are partly right. In the grand scheme of the last several million years of human evolutionary history, it matters little if humans arrived in the Americas earlier than we now think (unless they arrived as pre-sapiens, which is a doubtful prospect). Nonetheless, there is more here than an academic turf war. Hanging in the balance is our understanding of when, how, how fast, and under what conditions hunter-gatherers colonized a rich but empty continent, which will (a) force resolution of core theoretical and methodological issues (e.g. If there were pre-Clovis populations, why have they eluded detection?); (b) enhance insight into why certain biological traits evolved in New World peoples (e.g. 128a, 142, 143); (c) reveal whether the “speeded-up” New World evolutionary trajectory from hunter-gatherers to state societies (12) is as rapid as it appears; and (d) calibrate more precisely rates of genetic, linguistic, and dental evolution.
The controversy over the peopling of the Americas, nominally about sites and dates, cuts across several levels of inquiry. Hence, rather than focus on the particulars of potential pre-Clovis claims, I offer a more general overview of issues raised in the debate.

**Looking Back**

It was not until 1927, at Folsom, New Mexico, that the discovery of fluted projectile points embedded in the rib cage of an extinct bison finally demonstrated humans had reached America in the Pleistocene, albeit the latest Pleistocene. Soon thereafter, the discovery of larger, less finely crafted fluted points with mammoth remains at Clovis, New Mexico, pushed back human antiquity in the Americas even earlier.

How much earlier did not become clear until the late 1950s, when radiocarbon (\(^{14}\)C) dates at the Lehner site in Arizona put the Clovis occupation at ca 11,250 B.P. (56). By then, Clovis fluted points and assemblages were reported throughout North America, yet no pre-Clovis predecessor was found; it was beginning to seem there was not one. Krieger sounded the alarm that archaeologists, having overcome the pre-1927 resistance to any human presence in the Pleistocene, had replaced it with a new barrier at Clovis times (73:239). Throughout the 1950s, he championed prospects of a pre-projectile (i.e. pre-Clovis) stage—conceiving it partly on evolutionary grounds that such a stage ought to exist, but also to explain why its traces were so elusive (74, 75). Yet in 1964, even as he published his compendium of possible pre-projectile sites, a powerful Clovis-first model was emerging.

Six Clovis sites had by then been dated between 11,500 and 11,000 B.P. Their seemingly sudden appearance coincided neatly with new geological evidence indicating that at 12,000 B.P. the Cordilleran and Laurentide glaciers, which for the previous 15,000 years had apparently coalesced to cover much of Canada, had melted sufficiently to open an ice-free corridor through Canada, connecting Alaska with mid-latitude North America (57). Haynes argued the opening of the corridor, and the appearance of Clovis soon thereafter, were related; in the absence of an “indisputable [pre-Clovis] progenitor” in the mid-latitudes (which ought to predate the closing of the corridor at 27,000 B.P.), he concluded Clovis were the first Americans (57, 58).

**Clovis Archaeology**

In the decades since the emergence of this Clovis-first model, much has been learned of Clovis and near-contemporary Paleoindian occupations of North and South America. Yet key questions persist, beginning with whether Clovis records an initial entry or the evolution of a distinctive technology within an in situ pre-Clovis population [in which case the origins of Clovis and the origins of the first Americans are separate matters (17)]. The answer might be had
easily if Clovis origins were better known, but they are not. Clovis and Clovis-like points are recorded from the late Pleistocene in Alaska to Panama and in California to Nova Scotia (papers in 15). But none of the fluted points from Alaska or the corridor region of British Columbia are demonstrably as old (or older) as those in the mid-latitudes (21, 25), as they ought to be had they been invented by a founding population in the north and carried south. All, in fact, may be younger.¹ The apparent exception, fluted points from Alaska’s Putu site, ¹⁴C dated to 11,470 B.P., were recently shown to be from a near-surface context unassociated with the dated charcoal (111).

Clovis is argued to have roots in interior Alaska’s Nenana Complex, which has ages ranging from 11,820 to 11,010 B.P. (though most postdate 11,300 B.P.). Nenana is reportedly distinct from other Alaskan complexes, notably in its lack of a microcore-microblade technology, but it is said to resemble Clovis in tool type and technology, and “probably represent[s] the same population” (65:52; see also 45, 110). Nenana assemblages, though, lack fluted points. This absence is downplayed because Nenana assemblages have small, nonfluted points (Chindadn points), purportedly like those found in Clovis assemblages (65:51). But that similarity is more apparent than real. Small points occur in Clovis assemblages, but they are extraordinarily rare, and although they may be morphologically like Chindadn, the small Clovis forms are usually fluted, and in this and other aspects are unlike the thin, fine, marginally retouched Chindadn points. Complicating the picture even more, microblades and microcores are reported from Nenana-age contexts at Healy Lake and, recently, Swan Point (27, 66). That Nenana and Clovis assemblages appear alike may simply reflect the limited Clovis sample and the rather coarse-grained statistical analyses used in analytical comparisons of the two. For now, claims for similarities and a historical connection between Nenana and Clovis are unconvincing (13, 103). The far north still lacks a clear-cut Clovis predecessor, at least among the several groups now known to have been there in late Pleistocene times (76).

The oldest Clovis assemblages are those on the Great Plains and in the Southwest, dating between 11,200 and 10,900 B.P. (62:364). Fluted-point occupations in eastern North America appear to be slightly younger [ca 10,600 to 10,200 B.P. (63)], but the few dated sites occur in northern deglaciated areas, which apparently were occupied relatively late. There is a suspicion, though no confirmation, that fluted-point groups were in the Midwest and southeastern United States even earlier (17, 35, 92).

¹ This follows Krieger’s (73:239) hunch that northern fluted points are a “backwash,” having moved north from the Great Plains after Clovis times, and Anthony’s observation (5:904, and below) that most migratory streams develop a counterstream back to the place of origin.
Clovis points per se do not occur in South America,\textsuperscript{2} although certain attributes of a Clovis technology are present—notably fluting, which appears on the widely distributed fish-tailed projectile points. The existence of this attribute is said to indicate a direct historical relationship in the late Pleistocene prehistory of the two continents (80). South American assemblages occur as early as the earliest Clovis sites, although most appear to cluster in the millennium after 11,000 B.P. (31:153; 82:257).

North and South American Paleoindians occupied complex environments, many without modern analogues, but which included settings akin to modern high-alpine habitats, grasslands, forests, near-deserts, and swamps, all in the midst of the geologically rapid climatic and ecological upheaval that accompanied the end of the Pleistocene (31, 35, 82, 145).

Despite the diversity of their ecological stage, there is an apparent uniformity in the Clovis archaeological record across the North American range: Its assemblages typically are represented by lanceolate fluted points, large bifaces, end- and side-scrapers, gravers, flake tools, and (where preservation permits) a few tools of bone and ivory; blades and burins are present but rare (60, 72, 121). Clovis artifacts routinely were fashioned of high-quality cryptocrystalline stone, usually from bedrock rather than cobble sources, which in many instances were over 300 km from the sites in which the stone was recovered (60:392). Although difficult to demonstrate, it appears that these groups acquired most of their stone directly at the source rather than indirectly via exchange (94). The distance from the source to the place where it was discarded thus is a blunt measure of the scale of settlement mobility and leaves little doubt that most of these groups were wide ranging and highly mobile (72, 92, 129).

Contemporaneous South American artifact assemblages are much more diverse, comprising various bifacial and unifacial complexes (31:184). Their diversity is partly attributed to regional archaeological traditions and classificatory schemes, but also to the highly varied environments these groups were exploiting (16, 82).

The apparent uniformity of Clovis in North America and its links into South America are at times explained as the archaeological wake of rapidly moving, specialized hunters and their immediate descendants, whose pursuit of migratory game enabled them to cross ecological boundaries (72). The extreme version of this strategy, Martin’s overkill hypothesis, argues that human predation caused the extinction of nearly 80 genera of megafauna in

\textsuperscript{2} This absence makes applying the term pre-Clovis to occupations in South America inappropriate (93), but ease and convention warrant its usage, and I do so with the express understanding the term is not meant literally, but as pre-Clovis in age (i.e. pre-11,200 B.P.).
North and South America in the final millennium of the Pleistocene (89, see also 82), but that is unlikely.

The overkill hypothesis contains deep theoretical flaws and empirical weaknesses (48), not the least of which is that the apparent chronological correlation on which it rests—the appearance of Clovis and the disappearance of the megafauna—may not exist. Importantly, rejecting the overkill hypothesis is not tantamount to rejecting the Clovis-first model, as some mistakenly imply (116:284). As defined, overkill required the Clovis-first chronology and no pre-Clovis presence (89:972), but the Clovis-first model does not require overkill. Haynes made that point clear early on when he dismissed the possible role of Clovis groups in megafaunal extinction as a “moot question” (57:1412). One can create viable models of fast-moving Paleoindians without invoking overkill (71).

Such models are badly needed. Even if, as now appears, the affinities between Nenana and Clovis are unsubstantiated, and even if the apparent technological similarities between Clovis and contemporary South American assemblages are fortuitous or result from convergence, not divergence (31:184–186; 109:297), it is true, nevertheless, that the oldest generally accepted sites in North and South America are virtually contemporaneous (17, 144). If there were no pre-Clovis, then the first Americans must have traversed the hemisphere in a matter of centuries (82).

How Fast, How Far?

Traversing the hemisphere that quickly is far faster than the current American record for prehistoric migration in an empty niche by hunter-gatherers, which is held by the ancestral Thule Eskimo, who about 900 years ago radiated some 5000 km from Alaska across the northern maritime to Greenland in the course of a few centuries. But these marine hunters were moving across a homogeneous habitat they had been exploiting for millennia and that was expanding eastward because of warmer-than-average temperatures (91).

The first Americans had no such advantage, pioneering as they were a changing, diverse, and (moving south) increasingly exotic continent. They had to enter unfamiliar habitats and identify the edible, useful, or harmful flora and fauna in each; cope with novel pathogens and diseases (29); and traverse topographic and ecological barriers, including barren deglaciated landscapes, rivers swollen by glacial meltwater, and low-productivity forests. They had to

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3 The relatively limited and, in the case of Native South Americans, novel alleles of the major histo-compatibility complex (MHC), is generally attributed to disease-driven selection, likely a response on the part of founding populations to newly encountered pathogens such as acute malarial infection, Trypanosoma cruzi, and Leishmania braziliensis (9, 141).
accomplish all of this while maintaining their population sizes and vital kinship links on a vast and otherwise empty continent.

On the face of it, these unpropitious circumstances ought to have slowed migration (97, 144). Yet, the contemporaneity of sites across the hemisphere indicates otherwise, and the seeming impossibility of reconciling so rapid a spread with "existing demographic models of human migration," becomes, for Whitley & Dorn, supporting evidence for a pre-Clovis presence (144:642). Difficult though it may be, one may ask if it is impossible to model a fast radiation of people through the Americas.

Such a task may seem impossible if that radiation is thought of as a demic expansion (as it usually is) (e.g. 89, 116, 127, 144), in which daughter groups fission from a growing population but relocate only a short distance away. Their proximity permits mate exchange, raises effective population, and lowers inbreeding and risk of extinction (7). As the process is repeated, descendants expand to cover the continent. Under such density-dependent circumstances, "faster colonization speeds require population growth rates grossly beyond anything remotely possible for hunter-gatherers" (144:633); hence, the apparent impossibility of explaining the sudden appearance of Clovis.

But that conceptual hurdle may be cleared by disengaging migration and migration speed from population growth (5, 7, 71). Consider Beaton's (7) Transient Explorer Model (TEM), in which fissioning is not population dependent but occurs instead at minimum numbers and involves long-distance migration. Such a strategy favors wide and rapid spread, and it might entail leapfrogging of areas (patch-jumping) and even return migrations (5), which, depending on their scale, might have the ancillary benefit of increasing the effective population. A TEM strategy has great costs: low social connectivity, high inbreeding, low fecundity, and a higher probability of extinction of new buds owing to stochastic events (7, 93); but it renders population growth unnecessary for rapid, long-distance movement.

Kelly (71) argues the TEM strategy could explain a Clovis-first migration under certain parameters derived from evolutionary ecology and based on several archaeological assumptions. The key is that large game animals (and not plants) were highly ranked resources to Clovis foragers (said to be preadapted to hunting by virtue of having traversed an arctic landscape). Prey encounter rates, and thus hunter return rates, were initially high as these groups entered virgin, apparently game-rich habitats but declined rapidly as prey learned the dangers of human predators and because of overall late Pleistocene environmental instability. In response, Clovis foragers could either stay put and expand their diet by experimenting with new, lower-ranked resources or move to new habitats where encounter and return rates of the higher-ranked resources would be expected to be greater. Given the overall
richness of the late Pleistocene landscape and assuming hunting skills were more easily transferred to new and exotic habitats than was knowledge of plants, Kelly argues that Clovis groups were more likely to move, and move often, in response to locally diminishing returns; higher encounter rates elsewhere would readily offset the costs of moving. By concentrating on hunting, Clovis groups would be “pulled south” through the Americas, and although their habitats would change, their niche would not; hence, they could conceivably move as quickly as later Thule groups [if not more so, in submarginal areas like the ice-free corridor (86)].

The viability of the TEM obviously depends on whether these groups were specialized hunters, whether the late Pleistocene landscape was as rich (relative to humans) as appears, whether large-scale environmental instability directly impacted forager encounter rates, whether territory shifting and not dietary switching was the primary response to resource stress, and whether movement under these circumstances inevitably led south and why (96). It also requires a more complete accounting of the demographic costs of the TEM. Inbreeding may work in the short term to sustain such a fast-moving population, but it is not viable demographically over the long term. As range size increases and population density decreases (as would have happened in the circumstances envisioned), the costs of social interaction—keeping contact with distant kin and allies for mate exchange—rise considerably (86). How were such groups able to maximize (or even maintain) their populations while moving away from their points of origin? Part of the answer may be the size of the founding group, but groups sufficiently large to maintain a mating network might be too large to be flexible enough to move that far that fast that often. Much more needs to be known about the social ecology and rules of range extension across an empty land (86). If such issues are resolved, and if it is shown that Clovis and related Paleoindians had the adaptive strategy envisioned by Kelly (71), then the TEM provides a reasonable account of the rapid spread of Clovis.

Recent developments in radiocarbon dating may bear on this issue as well. The advent of high-precision Uranium-Thorium (U-Th) dating (6, 40) has at last pushed reliable calibration of the radiocarbon time-scale into the Pleistocene [the tree-ring calibrated portion still only reaches as far back as 9494 B.C. (8)]. Doing so extends a trend evident in much of the Holocene—$^{14}$C ages are consistently younger than true calendar ages (see 125). The magnitude of the discrepancy is substantial: Corals from Barbados had $^{14}$C ages $\approx 3500$ years younger than the U-Th calendar ages of $\approx 20,000$ B.P. (6). In effect, late Pleistocene radiocarbon years and the $^{14}$C time scale are compressed.

Although the relevant calibrations are still imprecise (126), partly because of strong late Pleistocene $^{14}$C oscillations triggered by the Younger Dryas
event (40, 125), they have tantalizing archaeological implications. Clovis sites occur across much of the hemisphere in just a few hundred radiocarbon years. Yet, in actual calendar years, the radiation may have taken longer [Haynes anticipated this point (59)]. How much longer depends on extending and more precisely resolving the calibration and may require tree-ring controls to remove the complications of marine-to-atmosphere reservoir age corrections (now at a 400-year constant) necessary in using U-Th ages (126). Ultimately, if this calibration decompresses late Pleistocene radiocarbon years, it could make it easier to explain the overall rate of spread of Clovis groups, even if it remains difficult to explain how the earliest of their sites in North and South America are essentially contemporaneous.

Of course, all this would be irrelevant if there were a pre-Clovis presence in the Americas, in which case Clovis might not record initial migration, but instead the diffusion of a new and highly visible technology across an extant population (assuming traits spread more rapidly among people than people spread into unknown territory) (97, 121). Admittedly, it is difficult to conceive, in theoretical or practical terms, of diffusion on such a vast scale (123), particularly without more evidence (none now exists) of the continent-wide adoption of what must have been a novel and difficult technology. The suggestion that Clovis points diffused widely because they made effective weapons for big-game hunters or to “offset deteriorating subsistence opportunities” (13:323) may fail if, as it appears, Clovis groups were not primarily big-game hunters (17, 70, 96, 121). Still, there are strong claims for a pre-Clovis population—not just from archaeology but from other disciplines as well.

**A Genetics Answer to the Archaeological Question?**

Genetics, Torroni announced in *Science*, “confirms [that the first Americans] were pre-Clovis” (43). Similar claims, nearly as bold, are echoed in linguistics (106). Questions about Native American origins have long attracted nonarchaeological answers (95). Occasionally, answers converge, as a decade ago when it was hypothesized, based on linguistic, dental, and (admittedly tenuous) evidence from so-called classic genetic markers, that Native Americans comprise three distinct groups—Amerind, Na-Dene, and Eskimo-Aleut—each descendants of separate migratory pulses, the earliest of which was Amerind (51). Since then, however, the underpinnings of that hypothesis have come under fire, especially Greenberg’s lumping of the vast majority of North and South American languages into the Amerind family (20, 44, 50, 97, 98, 106, 127, 128). At the same time, analysis of single-locus genetic markers, notably

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4 The Younger Dryas event was a temporary, 1300-year return to cold, glacial conditions, which occurred beginning around 12,900 B.P. in the midst of climatic warming and glacial melting.
mitochondrial DNA (mtDNA), has emerged as a potentially powerful tool for revealing the number, timing, and age of migration(s) to the Americas.

As is now well known, mtDNA is passed almost solely down the maternal line, varies by mutation rather than recombination, and evolves rapidly, making it possible to define molecular phylogenies and estimate the time elapsed since molecular divergence occurred. Molecular divergence, however, need not be in phase with population divergence (119:550; 127), although if it is, mtDNA becomes a powerful clock indeed.

Wallace and others (117, 132–134, 137) argue for this view, based on the high frequencies of rare Asian variants and the low number (four) of mtDNA lineages or clusters of specific mtDNA haplotypes among contemporary Amerind speakers [Greenberg’s (49) language families, acknowledged as controversial, are nevertheless used routinely to group populations analyzed in genetic studies]. From that evidence, they infer that ancestral Amerind populations experienced a severe bottleneck as they moved out of northeastern Asia and that they arrived on this continent with only limited molecular diversity (a founder effect). This inference, in turn, provides the basis for arguing that the molecular differences between Asians and Native Americans postdate their separation into distinct populations and, thus, measure the time elapsed since they separated (134). Based on sequence divergence within three of the Amerind clusters thought to represent the founding lineages and assuming a mtDNA substitution rate of 2–4% per million years, the ancestral Amerind radiation is estimated at 21,000–42,000 B.P. (133). A more recent estimate, using Siberian-American divergence times, puts that radiation slightly later (18,750–37,500 B.P.), but both are consistent with a pre-Clovis antiquity (134).

The weight of such figures obviously rests heavily on the inferred founder effect. But could that founder effect be more recent than the Pleistocene and perhaps mark the demographic collapse of Native American populations, triggered by the introduction of infectious Old World disease beginning in the sixteenth century (38, 97, 98)? Stone & Stoneking’s (122) analysis of a pre-Contact skeletal population from Illinois found that with a single exception, all individuals (n = 50) could be assigned to one of the four mtDNA lineages Wallace’s group identified previously (the exception may reflect sample contamination, mutation of a known lineage, or a now-extinct lineage). This finding clearly implies that historic demographic collapse did not significantly alter mtDNA variation (122). The same conclusion might also be reached by observing that if hemisphere-wide demographic collapse was random with respect to mtDNA lineages (i.e., no lineages were more or less susceptible to loss), then if there had been significantly more than four mtDNA lineages before European Contact, contemporary Native American populations ought to reveal more lineages than are now apparent. But perhaps they do.
Using different analytical techniques on different mtDNA sites than the Wallace group, Ward et al (138) found far more haplotype diversity in a single tribe (the Nuu-Chah-Nulth) than was previously reported among many other tribes. Sequence divergence between lineages indicated radiation times from 41,000 to 78,000 years ago, which, in the absence of archaeological evidence of pre-Clovis populations that old, led them to conclude that those molecular clusters predated population movements to the New World (138). When those populations did move, they were genetically heterogeneous; there was no founder effect.

That study was expanded subsequently to include more Amerind groups, as well as Na-Dene and Eskimo-Aleut speakers (the latter two were lumped as Circumarctic), and confirmed the high level of molecular diversity in Amerinds and showed that their within- and between-group divergence was much greater than in Circumarctic peoples (119, 139). This result indicated ancestral Amerinds diverged earlier than the Circumarctic groups (see also 132). Just how early depends on one’s analytical assumptions: Using a substitution rate derived from a model based on the coalescent (rather than the common but slower rate based on human-chimpanzee divergence), Shields and others put the Amerind molecular lineage origin as late as 12,100 to 13,200 B.P., which, assuming that molecular divergence precedes population divergence, would put an upper age on Amerind populations potentially within Clovis times (119, 139).

Yet, even in that time frame, the rapid rate of change in mtDNA may have long since obliterated Pleistocene founder effects (22), as witness the evidence for mtDNA genome equilibrium among contemporary Amerinds (22, 139). The now extant lineages “may as likely be the product of new mutations as of ancient founder effects” (128:796).

Torroni et al (132) disagree but provide no disproof of this conclusion. They attribute the greater mtDNA diversity detected by Ward and others to a misperception of the underlying population structure in Amerinds—an assertion in turn rejected by Szathmáry (128). The incompatibility of mtDNA results from the various laboratories may be driven by several factors, including differences in analytical techniques, choice of mtDNA substitution rates, and the nature of the samples. Sample size effects may also be a factor: Large samples are needed to detect the majority of lineages in groups with extensive genetic diversity (139) and may not have been obtained in all instances.

At the moment, there is no consensus from genetics on the antiquity and evolutionary history of the first Americans (see 127, 128 for a thorough appraisal). Yet Torroni and others’ (132, 134) age estimates coincide with Nichols’ (106) linguistic reckoning of a 35,000 B.P. entry. Although Nichols expressly disavows the language families by which the genetic results are derived, could that coincidence affirm a pre-Clovis antiquity for the first
Americans? Probably no more so than the genetic and linguistic evidence affirms a Clovis arrival. Genetics and linguistics can yield insight into Native American population history, which in turn can support inferences about the age of those populations, but not until the methodological conflicts in each are resolved. For now, these issues are as disputed outside archaeology as they are within it.

Ultimately, the question of the antiquity of the first Americans is a strictly archaeological one: Although their ages can be inferred, neither genes nor languages can be dated directly—only sites can. The archaeological demonstration of antiquity may yield no “insights into biological history” (127:217), but it surely puts that history on firm chronological ground, thereby helping settle differences over rates and modes of genetic and linguistic change. The discussion returns, then, to archaeology.

What Are We Looking For?

It is generally agreed the Americas were colonized by modern humans. Despite considerable and largely fruitless debate over what traces of these humans ought to look like (97), one fact seems inescapable: They will be the relatively complex and patterned material remains one expects of *Homo sapiens sapiens* hunter-gatherers (90). Those remains should be undeniably of human origin, either artifacts or bones; they should be in undisturbed geological contexts; and their age should be determined by indisputable radiometric dates.

These criteria were established long ago (58) and have been frequently criticized since. Pre-Clovis proponents commonly complain the criteria are too strict and insist that by them Clovis would not pass muster (17, 67, 116). They see proof in the greater number of 14C-dated pre-Clovis than Clovis sites, which “must be a troubling observation for those who consider a Clovis presence…to be well established but think a pre-Clovis occupation to be at best…highly controversial” (116:286–287).

This observation is really not troubling at all. There are hundreds of Clovis sites and thousands of isolated Clovis fluted points throughout North America. Although not all of them have been securely radiocarbon dated (though more have been dated than are given credit), it cannot change the indisputable fact that people were in the Americas then. The point, of course, is that Clovis sites long ago met the criteria, and the process need not be repeated with each new Clovis discovery. Once certain patterns emerge, basic facts can be assumed [within bounds—overreaching inferences from those facts rarely are allowed to pass unchallenged (see 47, 64, 107)]. The same will be true for pre-Clovis, once a site is found that meets the criteria and becomes recognizable. Tallying a list of unacceptable pre-Clovis claims, no matter how long, means nothing.
The history of this dispute complicates matters, insofar as the repeated failure of pre-Clovis claims creates a climate of skepticism, as happened a century ago when dozens of sites failed to substantiate an American Paleolithic (95). Yet in 1927 Folsom still proved that humans had arrived in America by the late Pleistocene. Likewise, each pre-Clovis claim is independent: No matter how many fail, that does not reduce the odds of finding one that will pass, as some suggest (e.g. 65:51). Still, it does reduce the odds of finding those willing to accept such claims at face value.

The criteria in use represent a set of evidential principles that ought to be met by any purportedly ancient site and are no different than the ones used by those who seek the earliest traces of human culture in the African lower Paleolithic (135). Whittley & Dorn claimed that “we no longer require evidence so obvious that amateurs could recognize it, as in the Folsom case” (144:641), a strange assertion given how many professionals have wrecked on these shores. It is true that we are no longer looking for speared bison; we have ways of understanding sites that supersede those of 1927. But if the details have changed, the principles of good archaeology remain the same.

The process of evaluating sites against those criteria can, however, become tangled, as is apparent in the ongoing debate over the radiocarbon chronology of the pre-Clovis levels at the Meadowcroft site in Pennsylvania (2, 3, 61, 130). Although it is difficult to predict the outcome in that particular instance, it does highlight a larger certainty: The first site to break the Clovis barrier will have to be utterly unimpeachable in all respects.

Resolving Archaeological Ambiguity

That such a site has yet to be found or agreed upon reflects, in part, an interpretive ambiguity that often plagues the debate (28, 32). Some early archaeological records, whether artifacts or features, appear ambiguous because of unfamiliarity or poor preservation, others because they are natural features. There are ways of telling the difference, but the key is not just amassing evidence about why specimens could be artifacts or why circular occurrences of rocks could be hearths. Rather, the key is in showing why those specimens could not be natural.

At the rockshelter of Pedra Furada in Brazil, for example, the Pleistocene layers at the site (dated from 14,300 to 48,000 B.P.) are characterized by alleged simple cobble tools made of locally occurring quartzite. These specimens were found in a coarse matrix formed largely of debris that fell into the shelter from a thick quartzite cobble conglomerate layer 100 m above the shelter floor. They display attributes believed to be diagnostic of human origin (108), including the presence of more than three flakes, edge angles less than 90°, and a logic or pattern to the flaking.
Yet, at one end of the shelter there is an active talus cone of cobbles lately fallen from the same conglomerate that formed the Pleistocene deposits. Cobbles in this modern talus range from unbroken ones, to ones slightly flaked, to ones of similar dimension and form to the purported Pleistocene artifacts, and appear to fit all of Parenti’s criteria (100). Their context proves they are not artifacts but geofacts (naturally flaked specimens). Because the purported Pleistocene artifacts occur in deposits formed by the same geological mechanism, one must begin by assuming the Pleistocene specimens are also geofacts and then prove otherwise.

Actualistic approaches should be useful in this regard and have long been part of this debate (46), particularly in the last decade or so when a minor cottage industry sprang up to show how readily nature fractures bones in ways thought to have been unique to humans (papers in 14, 36). The best of those approaches are where nature is examined directly for what it produces, as at Monte Verde in Chile, where Dillehay (28) did preemptive excavations of natural deposits two kilometers off the site, to anticipate questions about whether the extraordinarily preserved organic deposits at the site proper could be a natural accumulation (they do not appear to have been so).

Ambiguity and its attendant problems can also arise from using unproven dating techniques, as happened over a decade ago when amino acid racemization was applied to human bone and yielded erroneously old ages (131). The latest unproven technique to be applied is cation-ratio dating, on which pre-Clovis claims are made for petroglyphs and artifacts from surfaces in western North America (144). Its flaws are already apparent—and considerable (10, 11, 112, 114, 115).

Cation-ratio dating assumes that more soluble elements (e.g. potassium and calcium, as opposed to titanium) within the rock varnish that coats artifacts will leach differentially over time; hence, their ratio will vary with age. As Bierman & Gillespie (11) demonstrate, however, the basic assumption is unsupported. Cation ratios do not vary consistently with age, but instead appear driven by many age-independent factors, including rock type, the presence of titanium-rich volcanic dust, and substrate contamination (114). This observation, along with vagaries in statistical and chemical analyses, explains why cation-ratio dating has defied true independent verification (10, 11).

Even less secure is the claim that the Pleistocene-Holocene boundary is preserved in varnish stratigraphy as a change from higher to lower amounts of manganese, the higher amounts ostensibly derived during wetter Pleistocene times (144). Varnish stratigraphy and chemistry are highly variable and not directly a consequence of age or climate, exclusive of other factors (112, 115).

The radiocarbon dating of varnish organics is promising, but this method has so far produced less than a dozen pre-Clovis ages (144). Leaving aside the unanswered question of how those organics are extracted from the varnish, it is
reasonable to ask, as critics have (113), whether old carbon is being incorporated into varnish or whether the varnish itself predates the artifact. Dorn (37) admits the possibility in theory but denies it in practice. Confidence in the denial is proportional to the number of independent checks on the results, and so far those have not been forthcoming. Until they are, varnish dating, and the pre-Clovis claims based upon it (144), cannot be accepted at face value.

Coincidentally, a similar situation is emerging in Australia, where luminescence ages 15 to 20 thousand years older than the oldest accepted \(^{14}\text{C}\)-dated sites have sparked a lively (and to American ears, familiar) debate over the antiquity of the first Australians (summarized in 4).

*Coveting Thy Neighbor’s Archaeological Record*

Analogy between the American and Australian situations may not be entirely apt, because luminescence dating rests on a much firmer geochemical basis than cation-ratio dating. Still, aspects of the peopling of the Americas and Australia are often compared (7, 69, 83), and usefully so, insofar as comparison enlarges discussion of the processes of migration and colonization, or addresses common methodological concerns (e.g. the accuracy of dating techniques).

It has also been observed that once serious work began in Australia, its deep antiquity was “established quickly, definitively, and largely without rancor,” even though that continent has far fewer archaeologists who have worked over a shorter period of time than in the Americas (83:273; see also 7, 69). The implication is that if there were pre-Clovis populations in the Americas, then we ought to have seen them by now [assuming a similarity in the geological histories of the two continents and a comparability in site-preservation processes, which may not be strictly true (144:634–635)]. After all, an “enormous amount of earth” in the United States has been moved by construction, providing “a random sampling of the topography and environments of the continent” (69:346).

That sample is probably not truly random, but it is true that it has not yielded any pre-Clovis sites. Of course, it has turned up very few Clovis sites. The issue is not the amount of earth moved, but the amount moved that exposes surfaces of the proper age. We need an age-representative sample, not just a random one. Then the absence of evidence becomes evidence of absence. Until then, the controversy over luminescence ages in Australia suggests that comparisons may be apt in an unintended way: The issue of antiquity may be equally unsettled on both continents.

Pre-Clovis critics also point to the impoverishment of early American archaeological records compared to those of Europe, Africa, and Asia. Look around before 12,000 B.P., Martin (90) argues, and everywhere but America
artifact quantities are "awesome;" if America was occupied at the same time, "why didn't they leave us some similar trophies?"

The answer is antiquity and demographics. The Old World was occupied earlier than the New, no matter how old the New, and by late Pleistocene times had relatively large and archaeologically visible populations. In contrast, as Hassan (55:202) shows, a small number of hunter-gatherers could have entered the New World 20–25 thousand years ago, yet hovered below archaeological radar screens long thereafter by virtue of low (1%) annual growth and geological processes that make recoverable sites dating to 11,000 B.P. 10–15 times more common than those dating to 30,000 B.P. (19). Clovis, in that situation, may reflect the visible portion of a population curve that began much earlier (97), although again such a scenario must explain its evolutionary origins.

**Searching for Early Sites**

The potential rarity of pre-Clovis sites highlights the importance of expanding search strategies beyond opportunistic surveys of exposed or accessible Pleistocene-aged deposits to include a systematic search of settings that might yield pre-Clovis age remains. These settings would include former valley-margin sites buried under alluvium or colluvium, spring sites, uplifted coastlines, wetlands and bogs, tropical forests, and caves and rockshelters (19, 26). Methodologically, the pre-Clovis search is a geological problem first and an archaeological one second. There are also obvious regions in which to look for early remains, such as the Isthmus of Panama, through which the first South Americans surely passed (32).

Pre-Clovis traces may not be found where Clovis occurs. What was attractive to groups 11,200 years ago might not have been so to others 5000 or 10,000 years earlier. Land use and adaptive strategies change, not to mention localities themselves. This is true even where there was adaptive continuity: With some obvious exceptions, most High Plains Clovis sites do not likewise have Folsom, Plainview, and later Paleoindian components stacked above them. That these sites also lack a pre-Clovis record is interesting, but not significant, except perhaps under certain conditions. Establishing pre-Clovis antiquity may not be as easy as looking below Clovis.

Currently, there is no compelling evidence to eliminate a pre-Clovis occupation. Whether the evidence exists to demonstrate one—and demonstration is an empirical matter, not a theoretical one (cf 144)—remains open. Of the pre-Clovis contenders that have attracted the most attention, an international conference and site visit in Brazil in December 1993 revealed that opinions are still sharply divided on Pedra Furada's Pleistocene antiquity (for one assessment, see 100). Debate over Meadowcroft has virtually stalled and may stay that way until the final report is issued, if then. So much controversy has
dogged this site that it may become the exception proving the rule (99) that if a site is not convincingly older than 11,200 B.P. before a pre-Clovis presence is generally accepted, then it will not become more convincing after such acceptance.

Monte Verde remains the most viable pre-Clovis candidate, because of its exceptional preservation and comprehensive analysis, the last volume on which will soon appear (30). Its richest deposit dates to 12,500 B.P., 1300 years earlier than Clovis. Depending on how the $^{14}$C calibration is resolved, the gap between the two may prove to be greater, making it even more difficult to draw Monte Verde (which is quite unlike Clovis) into Clovis times, as some would propose (34, 81). Recent studies confirm that $^{14}$C ages in the northern and southern hemisphere do not differ significantly (136); Monte Verde’s $^{14}$C ages are therefore not simply an artifact of the site’s location.

One If By Land, Two If By Sea

Monte Verde’s antiquity might imply an arrival in Beringia prior to 20,000 B.P. There is no archaeological evidence to preclude this conclusion, but admittedly the age when humans first appeared in western, let alone eastern, Beringia is unclear (23, 65, 101). The oldest accepted western Beringian sites are no older than 20,000 B.P. (65, 101). Then, or for that matter anytime between ca 30,000 B.P. (118) and as late as 11,000 B.P. (41), hunter-gatherers could walk from Siberia to Alaska across dry land. Even before and after, when low-lying parts of Beringia were underwater, crossings on foot probably were possible over winter sea ice. Under some circumstances, they still are today (54).

There were no significant geographic barriers to cross-Beringian traffic: The area was wide, relatively flat, and ice free except on its edges. There may, however, have been ecological barriers: In full-glacial times Beringia was cold, dry, and nearly treeless [timing varied, but treeless conditions lasted until at least 13,500 B.P. (78)]. The absence of wood for fuel, Guthrie (54) argues, would have made it difficult, if not impossible, for human settlement. That point, though well taken, is partly based on the current scarcity (or disputed status) of full-glacial archaeological sites in Beringia, which may change with more archaeological work in that region (65, 103). Moreover, groups could have used animal products as fuel, as did humans on the treeless central Russian plain in glacial times (120).

Hopes have largely faded for an early (pre-25,000 B.P.) human presence in the Old Crow region and elsewhere in eastern Beringia, at least one based on flaked bone artifacts. The reasonable doubts that those specimens were flaked by human hands (noted above) were compounded by the redating of the sole unequivocal artifact—a caribou tibia flesher—from 27,000 to 1450 B.P. (105). Only Bluefish Caves (23) remains a viable candidate of an early eastern
Beringia human presence, but because of depositional complexities, there is little consensus that its microblades and microcores are as old as they appear (65:50). This much is certain: People were in interior Alaska soon after 12,000 B.P. (110). On its face, that is not old enough to represent Monte Verde’s ancestral population, but even if the earliest Alaskans were present before then, could they have left Alaska via the ice-free corridor in postglacial times and arrived in southern Chile by 12,500 B.P.? That depends on when the corridor was open.

The corridor—really two routes, one through central British Columbia between the Coast Range and Rocky Mountains, the other via the foothills and plains east of the Rocky Mountains—was closed by glacial ice minimally from ca 20,000 to 13,000 B.P. [30,000–13,000 B.P. in the more interior route (68, 145)]. But even after glaciers withdrew, meltwater drainage, harsh near-glacial climates, and low primary productivity would have rendered the routes impassable (77, 85). Most fossils of vertebrates in the corridor—on which migrating human foragers would have depended at least in part—do not predate 11,300 B.P. (18).

Of course, an interior passage need not have been the route south: The first Americans may have come via the coast before and even during full-glacial times (39, 42, 52, 116). Although one cannot preclude the latter, a coastal migration then is considerably complicated. Cordilleran ice covered large segments of the coast, advancing early (ca 23,000 B.P.) in southern Alaska, and later (ca 20,000–14,000 B.P.) further south (24, 87, 88). Ice retreat was asynchronous as well, occurring earliest (16,000 B.P.) in southwestern Alaska and along the outer coastal islands (87, 88, 140), and later (ca 12,000 B.P., although the timing varied) along the interior islands and remaining portions of the coast (24, 79).

Even if, during full-glacial times, non-glaciated refugia along the coast were sufficiently numerous and rich to support migrating groups (a point yet to be demonstrated), colonists on foot faced significant obstacles: Individual glaciers were as much as 20–50 km across, 600 m high, heavily crevassed, and they flowed onto the continental shelf and were separated by steep unglaciated ridges (87, 145). One can raise the theoretical ante by assuming colonists had boats enabling them to bypass these obstacles and refugia-hop down the coast (39), but there is not even circumstantial evidence for boats, save that demanded by the coastal model itself.

The most likely location of sites marking a coastal migration would be along the continental shelf and glacial age shorelines, which are mostly thought to be submerged beneath as much as 100 m of ocean by postglacial sea-level rise (39, 116). This seemingly renders the hypothesis safely untestable (145), at least until considerable logistical obstacles are overcome (39). Yet along parts of the coast between southeastern Alaska and Puget Sound,
postglacial isostatic rebound (the uplift of the land following the removal of the ice mass) outran sea-level rise (24, 124). As a result, there are on the mainland relict uplifted shorelines some 200 m above modern sea level (24:44). These shorelines postdate glaciation (earlier ones were obliterated by ice), but some may date early in the sequence of ice retreat, and hence may open a window, albeit a geographically and chronologically narrow one, in which to test the coastal model.

In the absence of direct evidence of a coastal migration, it is asserted that linguistic data "strongly supports the coastal route," based on a belief that the apparent high diversity of languages historically recorded along the coast resulted from "populations being present on the Pacific coast for a long time" (116:289; see also 39, 52). Critics have responded rightly, however, that a high diversity of languages need not result from time alone, but from geographic (e.g. climate, latitude), economic, and other factors that may have little or no bearing on glacial (or earlier) age events, but much to do with the rich nature of coastal environments (44, 104, 106). Rogers et al consider the possible role of population, but dismiss it, saying "China has a vastly larger population than Siberia but is not any more diverse linguistically" (116:289). Even if this undocumented assertion is true, the point backfires, because China was unquestionably occupied far earlier and longer than Siberia: By their own reasoning, China ought to have much greater linguistic diversity. In any case, presupposing that modern languages mark migration routes taken tens of thousands of years earlier across a radically different landscape is probably no more realistic than inferring de Soto’s route across the Southeast by the distribution of Spanish dialects there today—and at least we know de Soto spoke Spanish (93).

Looking Ahead

"There was a time when, to all appearances, American archaeology would have to be squeezed into the cramped quarters of ten thousand years; but we are pretty sure of twenty or even thirty thousand now." Or so Abbott thought a century ago (1). He was wrong then (the American Paleolithic wasn’t even ten, let alone thirty thousand years old). It’s too soon to tell whether those words are right now.

Bringing closure to the debate over the antiquity of the first Americans will require a strong pre-Clovis contender and can be facilitated by "working parties, meetings of specialists on site, and formal debates, [that] take place regularly" (53:177). Such meetings are no less awkward than they were a century ago, when Abbott complained privately that a field conference assessing his Paleolithic site at Trenton, New Jersey was “just a lot of childish twaddle” (99:17). But they offer the opportunity to jointly examine and debate
evidence, clarify points of ambiguity, and possibly resolve the status of a purportedly early site.

Of course, the search for the first Americans should not be just an effort to find a thing—the oldest site in America. It should also be about finding things out—about this virtually unprecedented expansion by *Homo sapiens sapiens* into a rich, empty, dynamic, late Pleistocene landscape; about the processes, rates, and evolutionary rules of migration and colonization; about how long Native American populations were genetically isolated from their Asian ancestors, and the effects of that isolation; and about what this search reveals of our methodological and theoretical shortcomings. Solving the antiquity issue is a necessary step toward understanding these larger issues related to the peopling of the Americas, but it is only the first step. Unfortunately, it has also proven to be one of the most difficult ones.

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