The Early Settlement of North America

The Clovis Era

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> When a thing ceases to be a subject of controversy, it ceases to be a subject of interest. William Hazlitt (Flesch 1957:48)

1.1 Introduction

The beginning of prehistory in the Americas is a moving target. The bull's-eye on this target contains three missing pieces of crucial information: the date when the very first colonists arrived, the homeland where the first immigrants originated, and the subsistence behavior of the first people. But the target doesn't stand still – with each new discovery, the earliest dates keep changing, the possible homeland moves from Asia to Europe and back again, and the foraging behavior of the first migrants is lost in a fog of debate.

One hypothesis that has been around for a long time is that a single earliest founding population entered North America about 12,000 radiocarbon years ago (rcybp), which is approximately equal to 14,000 calendar years ago (see Appendix 1 for calibrated equivalencies of radiocarbon and calendar years). The hypothesis was inspired by the discovery that the earliest archeological materials in just about every geographic zone in North America were similar stone tools datable radiometrically or typologically to the same relatively brief time interval, 11,500 to 10,500 years ago. The trademark tool is a unique kind of stone spearpoint (Fig. 1.1) given the type name "Clovis," after a town in New Mexico near which early discoveries were made. These artifacts were manufactured by widely separated prehistoric people at almost the same time throughout North America, south of the great ice sheets that covered half of the continent. The fluted points from Nova Scotia are much the same as those from California. They are not identical, but the similarities outweigh the minor differences. Not only are the spearpoints similar across most of the continent, but there are other significant stone-tool classes and aspects of culture in general that seem to be equally widespread (Storck 1988). It legitimately can be said about almost every sampled locale in North America that Clovis is the "basal stratum from which other cultural groups descend[ed]" (quoted from Morrow 2000a:86, describing mid-continent finds).

But very occasionally an archeologist samples a locale where cultural materials underlie Clovis artifacts, or apparently pre-date the Clovis time interval. And increasingly, linguists and physical anthropologists find their interpretations of variability in Native American languages and biology hard to reconcile with models of basal Clovis migration. The dynamic interplay of data and opinions from different scientific disciplines is reshaping the debate about America's earliest people.

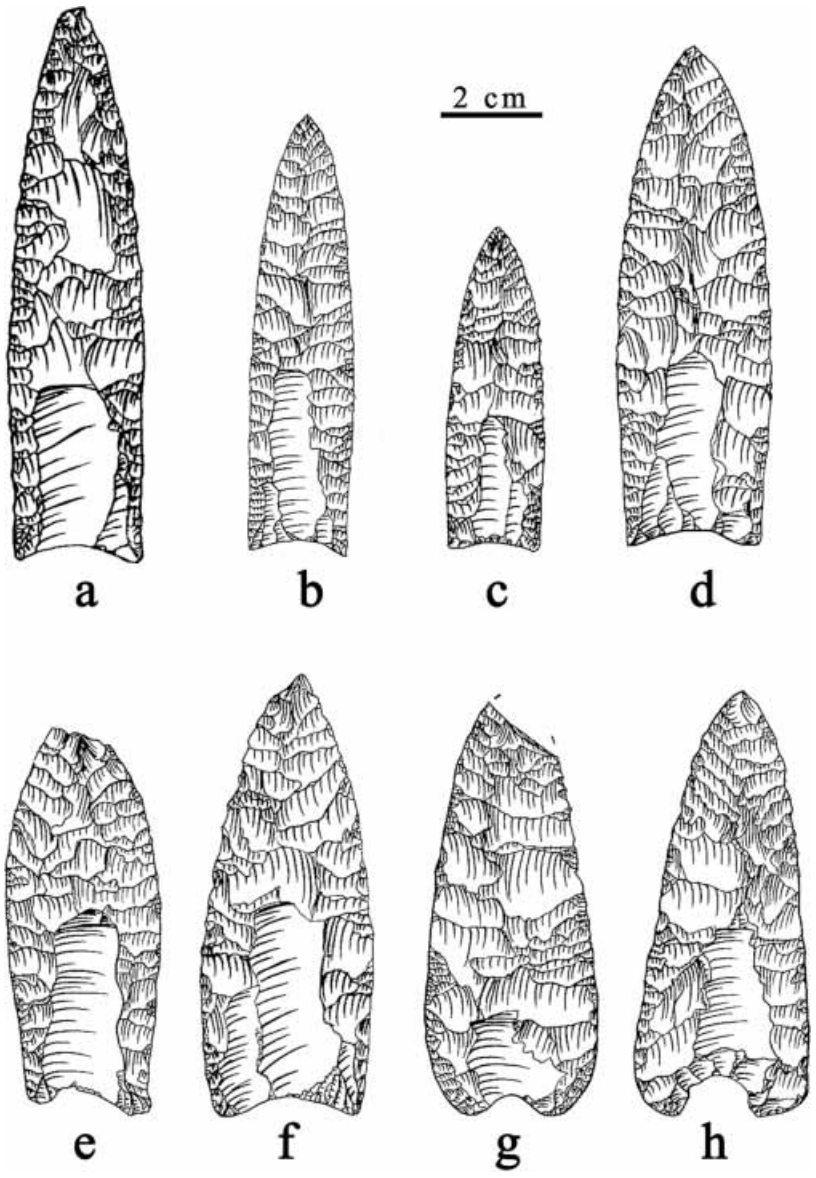

Fig. 1.1 Clovis points: (a) and (b) are from Blackwater Locality No. 1 (New Mexico); (c) is from Domebo (Oklahoma); (d) is from Lehner (Arizona); (e) is from Murray Springs (Arizona); (f) is from Dent (Colorado); (g) and (h) are from Colby (Wyoming) (drawings by Ted Goebel).

1.2 *Fin de siècle* paradigm-busting, or, what's at stake in the debate about the colonizing of North America?

> We have reached a point where further proof is superfluous, and where the weight of disproof lies upon those who deny . . . One feels that the stage of investigation is passed, and that of religious construction is overdue.
>
> Arthur Conan Doyle 1918:94, 95

When twentieth-century discoveries were made of fluted spearpoints, archeologists began to ask why people living in different habitats and ecozones throughout the continent would have manufactured stone tools that were so similar. One answer offered in earlier archeological literature was that the fluted

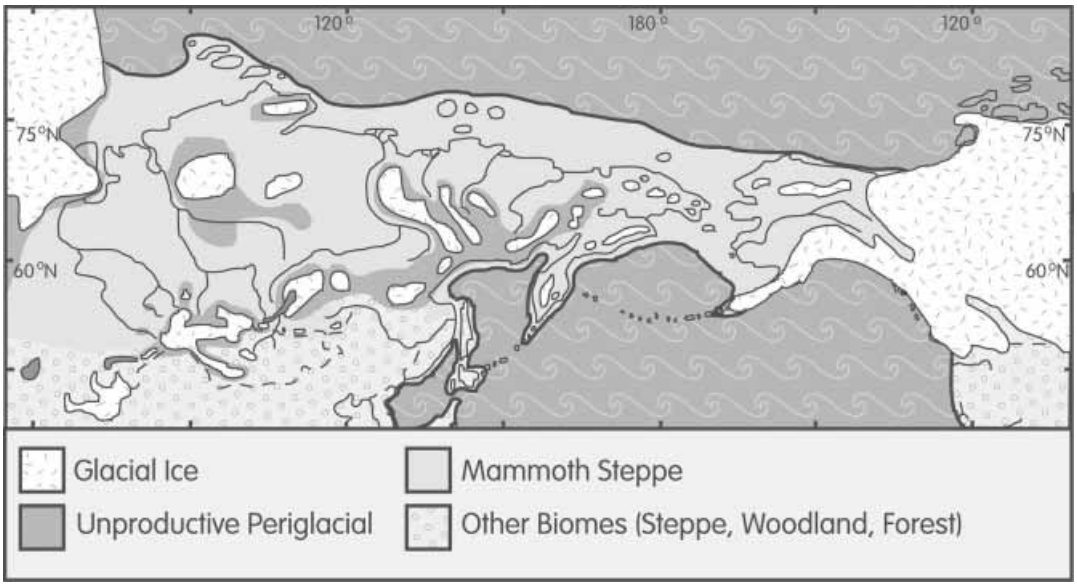

Fig. 1.2 Beringia and the land bridge connection between North America and northeast Asia, showing glacial coverage and shorelines during the Last Glacial Maximum about 18,000 rcybp (from a map drawn by Ted Goebel).

points were emblematic of a single culture carrying a coherent technology with them and spreading quickly into lands unoccupied by other people. A related answer was that the points were specialized for killing large animals, and they were such efficient killing tools that they contributed to the extinction of big-game species such as mammoths and mastodonts. When the large mammals died out during a period of rapid climate change, the Clovis way of life came to an end, the fluted spearpoints were replaced by other tools, and subregional cultural traditions replaced Clovis culture throughout the continent (Anderson and Faught 2000:512).

But not everyone agreed with this picture of the settlement of the New World. Among others, Alex Krieger declared that it was impossible to believe America was unoccupied before the appearance of fluted points (Krieger 1962, commenting on Mason 1962), because so many pre-Clovis sites were known to him. For decades, sites and artifacts had been discovered with artifacts (or *possible* artifacts) that may have been much older than the Clovis fluted points (for readable histories of over a century of controversy, see Meltzer 1991 or 1993b). Some of the sites contained simple-looking stone tools whose simplicity seemed to equate with "older," and some sites contained only very old bones thought to have been cutmarked and broken by humans. The sites that lacked stone tools altogether, it was suggested, had been created by people who could find no suitable stone at hand or who made tools out of only bone, antler, and wood because they had "lost the art of stone-flaking while [migrating] through the vast stretches of boreal forest and across the alluviated Bering land bridge" (Bryan 1969:345–6) (Fig. 1.2). Numerous summaries of putative very early materials appeared almost regularly, starting late in the nineteenth-century (for example, Wright 1892) and leading up to a late-twentieth-century flurry including (alphabetically) Bryan (1969), Krieger (1957, 1964), MacNeish (1976), Payen (1982), Stanford (1982, 1983), Waters (1985), and others.

As well, some linguists analyzed Native American languages and insisted that there had to be more than 12,000 years of language separation in the present-day continent. Up to (or over) 40,000 years of separation was proposed, based on

the language diversity. And some physical anthropologists also claimed that the few earliest pre-Columbian skeletons from North America showed too many morphological and genetic differences from Asian skeletons to be explained by a mere 12,000 years of separation.

Thus two opposing factions became clearly defined in the study of the peopling of the New World. The two factions unwittingly unfolded their debate in terms of "stereotypes in opposition" (Sherratt 1997), which seems fairly typical whenever discourse develops in major scientific issues (see Oreskes 1999, for example). In the case of the Clovis issue, the factions were those prehistorians who favored an early entry of the first American settlers (pre-dating the Clovis projectile points) versus those who favored a late arrival (in which Clovis-point-makers were the first settlers) (Bonnichsen and Schneider 1999).

The "early-entry" (pre-Clovis) faction tried to adopt into its cause each new discovery of a potential pre-Clovis-age site or artifact; but the "late-arrival" (Clovis-first) faction scrutinized the earlier sites with skeptical coldness. The early-entry faction was suffering cognitive dissonance; the Clovis-first faction could argue that the early-arrival advocates wanted so much to believe in pre-Clovis that they blocked out the arguments showing the weaknesses of their case. On the other hand, the pre-Clovis faction could accuse the late-arrival advocates of condescension every time they uttered pre-Clovis judgments, which were sometimes along the lines of Sherlock Holmes's lofty remark to Watson about the giant rat of Sumatra – it's a tale for which the world is not yet prepared (Conan Doyle 1924).

The serious ill-feelings resulting from the disagreement gave a permanent gravitas to the study of the peopling process. When an announcement was made about a pre-Clovis site that contained unusual artifacts, it was often soundly trashed by the group of archeologists favoring late arrival. The faction supporting an early entry argued that the standards for accepting archeological evidence from pre-Clovis finds were much more exacting or limiting than the standards for Clovis-age or later sites (Bryan 1991). This claim is identical to one made by a faction of paleontologists during the acrimonious 1980s debates about whether a meteorite impact caused dinosaur extinctions 65 million years ago; the standards demanded to prove the new theory were said to be "far higher than is normal in science" (Raup 1994:151). All uncertainty had to be removed from a huge range of topics if the new theory was to become acceptable. The Freudian psychiatrist Wilhelm Reich also echoed this sentiment in 1956 – just before going to jail – after the Federal Drug Administration refused to allow his "Orgone Energy Accumulator" box to be manufactured or distributed: technically, he said, he'd lost to "an incomprehensible procedure treadmill" although in a historical sense his good fight would be long remembered (Heard 2000:193–4).

It has been written that the stakes were much lower in the Clovis versus pre-Clovis debate (Meltzer 1995) than they were in other scientific arguments, such as the fierce forty-year fight over continental drift, in which one antagonist complained that if the new theory were true, geologists would have to "forget everything which has been learned in the last 70 years and start all over again" (R. Chamberlin in 1928, quoted in Oreskes 1999:313). The outcome of pre-Clovis debates will have much less effect on American prehistory, even after models of migration and subsistence are rethought. The existence of a pre-Clovis human presence may stretch the timeline yet it does not restructure mainstream

archeological methods, end the prevalence of inductive reasoning, or replace foundational ideas such as the law of superposition and uniformitarianism.

But the stakes really are sky-high in the scramble for the spotlight by individual participants, because career visibility goes up when the fighting starts. To be recognized as an iconoclast or revolutionary is to be assured a voice and a forum. To be quoted at length in a popular news magazine or to be featured in a major television program is exhilarating and attracts supporters (and ultimately funding). Thus combative and self-assertive archeologists have everything to gain from participating in the debate while the field itself will change very little.

As the pre-Clovis arguments developed in the 1960s through the late 1990s, the early-entry faction in the debate inevitably came to speak of themselves as an embattled minority treated unfairly by the highly respected and imperious specialists who shaped public opinion. The skeptical authorities were deemed small-minded people, like "humorless pedants [of the variety] who correct grammatical errors in love letters" (Sokal and Bricmont 1999:ix). In one case, a criticized early-entry advocate seemed to brandish his critic's skepticism "as though this were a criminal attitude in science" (Fiedel 2000b). Why did the early-entry minority feel so oppressed?

Scientific scrutiny – especially the dissection of claims that lie out of mainstream thinking in any discipline – is harsh and uncompromising. This is because archeologists know that every archeological find is destroyed when it is discovered. An excavated site cannot be pieced together again except on paper, which means that data can be submerged, hidden, or altered. In fact it is relatively simple to pull off a hoax with an archeological find; for example, in November 2000 "one of Japan's best-known archaeologists confessed . . . to having falsified important finds by secretly burying items and then 'discovering' them" at two sites claimed to be up to 700,000 years old " (Joyce 2000; see also Anon. [*Japan Times*] 2000; Anon. [*Mainichi News*] 2000; Bleed 2000; Keally 2000). The entire sequence of the Japanese Early and Middle Paleolithic "has sunk into the mire of scandal" (Keally 2000); as a consequence, it has been suggested that Japan may have *no* Middle or Early Paleolithic sites at all! Another possible example of a hoax is the unusual Sandia point type that some people believe was planted in Sandia Cave (NM) and other sites and did not belong in the early stratigraphic levels where it was reported (Preston 1995). The most famous example is the Piltdown hoax of the early twentieth century (Spencer 1990a, 1990b).

Even when hoaxing is not deliberate, archeological interpretations that are hasty, intuitive, or unsubstantiated can lead to blunders or oversights. During the 1970s the Old Crow collecting localities in Yukon yielded a broken caribou tibia that appeared modified by human hands to make a defleshing tool (although some archeologists disputed whether the toothed working end of the implement really had been deliberately created by human actions [Lee 1975:23; Payen 1982:362–3]). A fraction of the bone (in fact half of it was sacrificed to provide enough inorganic carbon for the date) was radiocarbon dated to about 27,000 rcybp (Irving and Harrington 1973). Surely this was universally indisputable proof of a pre-Clovis human presence in the Americas? Only a few professionals doubted the artifactual origin of this specimen, but the early date did surprise archeologists. Numerous broken bones also had been recovered from the stretch of the Old Crow river where the defleshing tool was found, and many bone fragments were claimed as artifactual

debris left by people who flaked tools out of large bones instead of stone. However, a few years after the initial discovery, the tibia deflesher was sampled again for dating – this time using improved methods that needed a small part of the remaining organic carbon – and and the tool's age was exposed as late Holocene (Nelson, Morlan, Vogel, Southern, and Harrington 1986). Later research also revealed that the associated broken bone "artifacts" recovered from the river's beaches and bars could have been fragmented by *noncultural* processes, which no one had adequately studied at the time of the original interpretations. The scientific method of scrutiny, skepticism, and testing was therefore a success in that it forestalled an automatic (but naïve) acceptance of dubious materials. The devoted advocates of pre-Clovis found themselves dragging more and more of this sort of burden behind them like Marley's chain.

Developments in the search for North America's first inhabitants were decidedly different from the same kind of archeological search going on in Australia in the late twentieth century. There, between 1960 and 1970, on a continent about the same size as unglaciated North America, a true revolution took place in the dating of the earliest human arrivals (Jones 1979). In 1961, the oldest "acceptable" human occupation of any site was early Holocene in age; but within a year a site with a terminal Pleistocene date was soon joined by more and more, at the rate of about two discoveries per year, until dozens of acceptable sites had joined the record. None of these older and older discoveries had been set ablaze as if they were Trojan horses, which is how American archeologists seemed to think of each pre-Clovis announcement. Why would Australia's prehistory be rewritten so swiftly and almost instantaneously, when North America's was proving to be so contested and unchanging?

Australia had remained an archeological blank well into the middle of the twentieth century, accounting for the missing information about deep prehistory. But once the archeologists began exploring and digging they succeeded in finding the earlier and earlier sites, and they succeeded in convincing colleagues. And once the reports and publications finally hit the streets, Australian archeologists sensibly reordered their research goals and strategies to take advantage of the emerging knowledge. But North America has been examined and surveyed for much longer, by a larger population of amateur and professional archeologists with an ever-expanding number of journals and periodicals for publications to appear in, and still the conventional wisdom placed the first human arrivals at not much earlier than 11,500 rcybp. Thus it would seem that maybe North America did not possess the same kind of reservoir of undiscovered ancient sites that Australia had, or if it did, the sites were somehow being passively censored.

The last two decades of the debate in North America may have been even more heated than the decades before, because frustrated Americanists could see the Australians burrowing deeper and deeper in time, slowly moving beyond 20,000 years, then quickly to 35,000 and 40,000 years, and beyond. Recent studies (Miller, Magee, Johnson, Fogel, Spooner, McCulloch, and Ayliffe 1999; Roberts, Flannery, Ayliffe, Yoshida, Olley, Prideaux, Laslett, Baynes, Smith, Jones, and Smith 2001) attribute the massive megafaunal extinctions in Australia to human colonists entering around 46,400 rcybp. Meanwhile, in the New World the arguments still raged about sites dated to a mere 12,500 rcybp.

By the early 1980s, the two most important "doyens of American archaeology" (Fiedel 2000a:43), Gordon R. Willey and Jesse Jennings, who wrote influential textbooks and commentary on North American prehistory (Jennings 1974, 1983; Willey 1966, 1974), were eagerly convinced that more and better archeological evidence would be found to prove humans had been in the New World for at least 25,000 years. Their support clearly shows – as Fiedel (2000a:43) notes in his review of the debate – that "the scientific 'establishment' has not been predisposed to crush such claims." Still, some posturing archeologists insisted that pre-Clovis colleagues feared for their reputations and funding because of their involvement in early-site research. These alarmed colleagues were rarely (if ever) named (see Morell 1990). Geographer George Carter, after one of his papers was rejected by the journal *Science* in 1960, wrote to the editor: "I have a correspondent whose name I cannot use, for though he thinks I am right [that humans lived in California 90,000–80,000 years ago], he could lose his job for saying so. I have another anonymous correspondent who as a graduate student found evidence that would tend to prove me right [but] he and his fellow student buried the evidence. They were certain that to bring it in would cost them their chance for their Ph.D.'s." Carter went on in that vein to refer to other (unnamed) professionals afraid of losing their jobs (Lee 1977:4).

In 1999, *Newsweek* magazine ran an article that mentioned mysterious (but also unnamed) "mandarins of American Anthropology" who had held archeology "in a stranglehold" before a critical moment of acceptance (see below – the Monte Verde pronunciamento), and who had been capable of banishing pre-Clovis finds. These mythical deities allowed "no deviation . . . from the party line" that Clovis was the very first culture in the New World (Begley and Murr 1999). Of course mandarins do not exist in archeology, and if they did they never would have agreed on anything. But the real lesson of this attitudinizing is that the force of the majority opinion clearly had been perceived as nothing short of oppressive and conspiratorial.

Ironically, even some non-archeological anti-evolutionists welcomed the archeologists' long history of resistance to pre-Clovis – particularly to the site in Chile named Monte Verde – because it supported their claims that "powerful forces in the academic world have suppressed research and publication" out of long habits of "prejudice and oppression" which these forces could perfect against creationism. The creationists now yearn for their own "Monte Verde milestone" (Jones 1999), referring to the appearance of a series of publications from prominent archeologists declaring one pre-Clovis site to be acceptable, after all. Cremo and Thompson (1998 [orig. 1993]) echo these sentiments, asking readers to question professional archeologists who steadfastly resist claims for very early people in the New World; Cremo and Thompson (1998:26) speak of a "shroud of silence" placed over unwanted findings, which soon fade into obscurity and disappear from all but the "moldering pages of old scientific journals." The anomalous evidence for very early people in the New World, doomed to the unseen depths, and supposedly suppressed by archeologists, includes a 505 million-year-old "shoeprint" from Utah (Cremo and Thompson 1998:810–13) and a 600 million-year-old metallic "vase" from Massachusetts (Cremo and Thompson 1998:798–9).

Over the last "two decades of acrimony" (a journalist's term – see Wilford 1998), in spite of what they publicly claimed was unsparing skepticism towards

them, bordering on persecution – the early-entry advocates apparently won what they think is the right to represent the consensus in New World studies (Adovasio and Pedler 1997; Meltzer 1997; Meltzer, Grayson, Ardila, Barker, Dincauze, Haynes, Mena, Nuñez, and Stanford 1997). At the beginning of the twenty-first century, archeologists prefer thinking that long before 12,000 years ago "multiple origins and numerous migrations" (the quote is from David Hurst Thomas's *Skull Wars* [Thomas 2000:171]) had created a continent full of regionally separated people, who had different technologies, ate different foods, spoke different languages, and looked very different from each other. The implication? America has always been an ethnic melting pot (Dillehay 2000), from the very earliest days of Pleistocene human colonization.

This theory of New World colonization has had several variants, and even the most popular versions have been decomposed "into elements that enjoy different levels of acceptance," a common enough occurrence in science (Clemens 1994:endnote 3.9). Yet the underlying linking idea of a pre-Clovis human presence is widely accepted by mainstream archeologists. Why are the claims for pre-Clovis migrations accepted by so many prehistorians who once rejected them?

Sometimes just the sheer weight of repetition in a science has a telling effect, as when one side repeats its claim often enough that people begin believing it. Shipman (2000:491–4) has suggested other reasons why scientists gradually replace their skepticism with belief – sometimes the evidence itself seems better and stronger over time, or famous experts sign on to the teams making the discoveries, thus enhancing credibility. And skepticism itself earns a bad name when rejected discoveries are later substantiated. "Skepticism is a cheap stance to adopt," Shipman (2000:494) writes, "for it is easier to cast doubt than to substantiate, especially if new techniques and new paradigms must be forged along the way." Skeptics are viewed as spoil-sports and wet blankets, while the new and unusual discoveries are welcomed with open arms.

Over the past century of archeological study of the peopling of the New World, a variety of larger social, political, or philosophical attitudes behind the scenes probably helped to shape the favored interpretations of the peopling process. The various interpretations can be seen as narratives that have storylines behind them, reflecting popular trends in the way we view human behavior. The Clovis-first (late-arrival) storyline, which imagines Clovis people to have been specialized big-game-hunters who spread rapidly into an unoccupied continent, is a great story to archeologists because it is so familiar – in fact, its appeal goes right back to the historically pervasive notion of the American frontier. Anthropologist John Alsoszatai-Petheo (1986:20) and journalist Roger Downey (2000:78–9) viewed the Clovis-first model as an updated variant of Frederick Turner's influential visions of the waves of pioneers entering the "wild" American frontier in the last century, "discovering" and then conquering it. The resident Native Americans occupying the frontier were treated as merely a difficulty to be overcome in the westward march of American settlement. Much of American life today, from political rhetoric to adventure movies, was influenced by this frontier obsession, according to Downey, providing a "richly provocative (if unconscious) template for thought" that still appeals to archeologists. The Clovis-first and Clovis-fast model was accepted and stuck around so long in the literature possibly because it is an exciting story white Americans like to tell over and over again about this continent. The story appealed to archeologists at the same time

as it once again validated the western world's ideals of dominion over nature (Alsoszatai-Petheo 1986:20), relentless expansion and exploitation, American exceptionalism, and the supremacy of technology. The possibility that Clovis hunters extinguished America's largest mammals – mammoths, mastodonts, horses, camels, and dozens of other species – was also employed as an object lesson in human insensitivity and power negatively to transform the natural world.

The alternate interpretation of the peopling process, the Clovis-NOT-first model, approaches the big lesson to be learned from another angle altogether. Archeologist Tom Dillehay (2000:293) wrote: "The Americas of the late Pleistocene [may have been] one of the world's first real ethnic melting pot [*sic*] and multicultural society" that had "no true categories of race and ethnicity." This sort of directly opposite model emphasizes the successes of local subsistence patterns rather than continent-wide cultural blueprints, seeks evidence for the existence of distinct and recognizable migrating groups derived from geographically separate homelands, regards technology as strictly shaped and limited by unique sets of regional resources, and does not allow us to view any archeological culture as superior or first or faster or more successful. This model's storyline is fitting for the political and social sensibilities of the twenty-first century.

Thus stories about the past continue to change to fit the current trends in thinking. Plus, some scientists can argue a new theory better than others, gaining the upper hand in print. The sharpest archeological rhetoricians may establish a theory that is the result of a "selective search through the literature for corroborative evidence, ignoring most of the facts that are opposed to the idea, and ending in a state of auto-intoxication in which the subjective idea comes to be considered as an objective fact" (Sullivan 1991:15). Ironically, this remark was made by a paleontologist attacking the early theory of continental drift, which has turned out to be correct after all. Ulysses S. Grant (1885–6) once observed that it saves a lot of trouble to declare a victory instead of fighting for it.

Obviously the scientific rules for "discovering" the true facts about the peopling of the Americas are in perpetual states of change. The appropriateness of certain kinds of evidence seems to change, as does the potential meaning of the evidence. One reason for the shifting ground is that scientists' opinions sometimes carry more weight than the evidence at hand. If an eminent expert announces that she or he disbelieves a particular interpretation of a discovery in the field, such as a new site or new DNA study, then the thousands of interested onlookers who make up the archeological community may also follow the trend towards disbelief. Similarly, a handful of experts approving a new discovery can be the catalyst for community acceptance or the stimulus for a testy backlash. The most vocal and enshrined experts reserve the right to shape thought and policy in the study of the earliest peopling process, and expect the archeological community to follow them.

This points up a very non-scientific feature of this subfield concerned with the peopling of the New World. The subfield has always been a no-holds-barred, wide-open and almost separate specialty in American archeology, a science "frontier" as it were – defined as a "boundary or limiting zone" (Ashcroft, Griffiths, and Tiffen 1998:107) that distinguishes the well known from the unknown. As with other frontiers, the prehistorians who want to be players in this subfield communicate in a unique priestly jargon whose lexicon is rewritten

all the time (for example, the appellation "Clovis culture" is frequently redefined). The players choose friends carefully, because certain individuals may be disliked or disbelieved, and must prove themselves capable of mastering the secrets and esoterica. The evidence needed or sufficient to support favored scenarios is discovered, discussed, and debated by the players. New classes of data are introduced and allowed to be analyzed. Official chronologies are drawn and redrawn or discarded. The qualifications of acceptability needed by an artifact or a site are set. Adding publicity is like giving oxygen to a fire. The specialists stand ready to fight it out on the streets and in the saloons (in conferences, publications, backrooms), trying to agree on something that can be called the truth, because the winners earn the power to define completely or invent the very objects of their studies. An aggressive disrespect is displayed towards the opposition, especially by those archeologists who want to be seen as original thinkers and critics.

Announcements about new discoveries are often treated with open contempt by adversaries who scoff at the evidence's insupportability. If the discoveries do not fit into the conventional categories – such as specific kinds of stone implements, or radiometric date ranges, or the sequence of deeply stratified tools – then the discoverers and opponents battle for the discovery's right to any kind of shelf-life. The well known and the unknown trade places daily in the canon of knowledge.

The experts thus demarcate the boundaries and conventions of their own study, and in so doing of course they establish themselves as distinct from the many other kinds of archeologists. The leaders in this subfield are responsible for imperially setting the boundaries around the original colonizers of the Americas. The experts define the ancestry of all Native Americans – they officially determine the time when the first people arrived in the New World, they decide upon the original homelands of the first peoples, and they make models to explain the behavior and adaptations of the founding settlers. For this reason, the stories told by experts about the peopling of the New World are of critical interest to Native Americans (for example, Deloria 1995) and post-modern archeologists (Kehoe 1998), as well as philosophers of science, sociologists, and historians concerned with changing patterns in scientific interpretations. Indeed, as suggested by Downey and Alsoszatai-Petheo, perhaps the archeological narratives of the peopling process and related interpretations really are examples of "post hoc objectification of Manifest Destiny" (the words are from Bruce Trigger's [2000] review of Kehoe [1998]). Some Native American activists flatly reject the archeological reconstructions (Deloria 1995, for example) because of conflicts with oral histories, legends, folklore, or religious beliefs. Deloria (1995) finds a scientific belief in trans-Beringian migrations to be absurd and unacceptable; but the preferred alternative stories about Native American origins are quite varied and unreconciled. The Hopi "speak of transoceanic migrations in boats," while other people "speak of the experience of a [local? *in situ*?] creation." "Some tribes," Deloria (1995:97) states, "even talk about migrations from other planets." Deloria's point is that either Native Americans originated nowhere else but in America, or they came to the continent far earlier than scientists know is possible. The politically significant implication of such a disagreement is easy to see – native people refuse to allow themselves to be defined as just another migrant influx.

The dispute has been transformed from science into politics. Not only are lines drawn in the sand to separate factions, but the styles of quarreling also differ within each faction. On the one side are the "urban" scientists (Segerstråle 2000:263), who are sophisticated, well connected, politically minded and unafraid of arguing loudly in public, and who tend to be cynical as well. The urban scientists are intellectuals who look for answers in controlled experiments, and who theorize and invent scenarios to predict the archeological data they expect to find. On the other side of the dispute are the "country" scientists, the simpler and more straightforward sorts steeped in natural history, who look for answers in nature, who observe events and processes and then make *ad hoc* explanations for them. Both kinds understand the political implications of the different scenarios of the peopling process (see Segerstråle 2000:262–3, for a discussion of another heated scientific dispute, this one about sociobiology).

The study of the peopling process is charged with emotion. It is impossible to write a book about the first settlers in America without offending someone, be it an anti-scientist, a Native American whose beliefs about ancestry are not open to question, or a scientific archeologist with a different point of view about the evidence at hand. Now that a "new paradigm" (Dillehay 2000) has been proclaimed for studying the peopling process, independent thought and skepticism are going to be run through flash furnaces to eliminate impurities, once again, which is what the Clovis-first proponents were accused of doing. The new generation of experts do not want to argue anymore, and the honorable tradition of skepticism is unwelcome. Even the potentially useful parts of the old Clovis-first models are to be rejected in the spirit of reactionary nihilism, which is skepticism carried to an extreme. The new generation of archeologists in the peopling discourse are absolutists whose beliefs are no longer subject to debate.

This book is my entry into the debate while it is still open. I offer certain clear opinions and a purpose that is not ultimately to uphold any existing model; rather I hope to assess the competing models and find productive directions for further research, and also for further questioning. I begin with a review of approaches that have been used to address the largest unanswered questions in the discourse.

1.3 How do archeologists address the big unanswered questions about fluted-point-makers?

> The superior man does not set his mind either for anything or against anything.
>
> Confucius (Flesch 1957:197)

"Clovis" is the name of a projectile point type widely found in North America. Similar points can be found in Mexico, Central America, and South America. The time period is rather narrow when the classic Clovis type is thought to have existed. Table 1.1 lists sites with radiometric dates customarily accepted as "Clovis" in North America.

Several Clovis finds have been dated radiometrically well older than the ages listed in Table 1.1, and some well younger. For example, the Lewisville (TX) site yielded a Clovis point, one flake scraper, a hammerstone, and a flaked

TABLE 1.1 Generally accepted radiometric dates on Clovis or Clovis-like point sites (from Dent 1999; C. V. Haynes 1993; Holliday 2000a; Tankersley, Ford, McDonald, Genheimer, and Hendricks 1997; Tankersley and Redmond 1999; Taylor, Haynes, and Stuiver 1996). Note that some of these commonly cited dates have been questioned (for example, by Roosevelt, O'Donnell, Quinn, Kemp, Machado, Imazio de Siveira, and Lima da Costa 1998), because of unclear sample selection procedures or disparities between wood dates and bone dates from the same strata.

SITE	DATE(S)	MATERIAL(S) DATED
Anzick, MT	Average of 3 = 10,820 ± 60	Bone
Aubrey, TX	Average of 2 = 11,570 ± 70 (but wide range of other dates)	Charcoal
Big Eddy, MO	8 dates from "Early/Middle Paleoindian levels" range from 10,260 ± 85 to 11,900 ± 80	Charcoal (mostly)
Cactus Hill, VA	10,920 ± 250	Charcoal
Clovis type-site (Blackwater Locality No. 1, NM)	Average of 2 = 11,130 ± 290 Average of 3 = 11,300 ± 240, (but wide range)	Plant remains Plant remains
Colby, WY	11,200 ± 220 (RL-392) 10,864 ± 141 (SMU-264)	Bone collagen Bone apatite
Debert, Nova Scotia	Average of 13 = 10,590 ± 50	Charcoal
Dent, CO	Average of 5 = 10,690 ± 50 plus 11,200 ± 500 (I-622)	Bone Bone organic acids
Domebo, OK	Average of 2 = 10,820 ± 230 Other averages 11,040 ± 250 and 10,940 ± 180	Carbonized plants Bone collagen and gelatin Bone collagen and gelatin
Johnson, TN	10,700 ± 980 (?)	
Lange/Ferguson, SD	11,140 ± 140 (AA-905) and 10,730 ± 530 (I-13104)	Charcoal flecks Bone organic acids
Lehner, AZ	Average of 12 = 10,930 ± 40	Charcoal fragments
Murray Springs, AZ	Average of 8 = 10,900 ± 50	Charcoal
Paleo Crossing, OH	10,980 ± 110 (AA-8250-E) 10,800 ± 185 (AA-8250-D) 11,060 ± 120 (AA-8250-C) (3 other dates average 12,150 ± 75)	Charcoal granules from postmold
Shawnee Minisink, PA	Average of 2 = 10,640 ± 290 10,940 ± 90 10,900 ± 40	Charcoal Carbonized hawthorne plum seeds

TABLE 1.1 (*cont.*)

SITE	DATE(S)	MATERIAL(S) DATED
Sheriden Pit, OH	Range of 13 AMS dates on culture-bearing stratum = 10,470 ± 70 to 10,970 ± 70	Wood charcoal (NB: burnt and calcined animal bones from elsewhere in cave yielded AMS dates about 1,000 years older)
Templeton, CT	10,190 ± 300 (W-3931)	Charcoal
Vail, ME	7 dates, ranging from 11,120 ± 180 to 10,040 ± 390	All but one on charcoal; youngest date on humates
Whipple, NH	Average of 2 = 11,050 ± 300 (2 other parts of the site were dated 9,400 ± 500 to 10,430 ± 300)	Charcoal (charcoal)

pebble associated with hearths and Pleistocene animal bones, radiocarbon dated > 37,000 rcybp (Crook and Harris 1962) (later work at the site indicated that lignite, which is mostly ancient carbon, was burned in the hearths, accounting for the very old dates [Stanford 1983]). Another example is a Kentucky mammoth (Vesper and Tanner 1984, cited by Lepper 1999:369) with a possible Clovis-point association, radiocarbon dated 8,360 ± 310 (Beta lab number not reported). More examples can be found in the literature; several Holocene dates on mastodonts are cited in Byers (1962, commenting on Mason 1962; also see Stafford 1994). The dating spread can be partly explained by the nature of radiocarbon dating – "dates" are only a statistical probability of an object's age and not a simple fact – or by the potential for sites, sediments, and samples to be contaminated, or by inappropriate choices of materials to be dated, or by "associations" that are speculative rather than clearly demonstrated, and so forth. Different materials dated from the same stratigraphic layers may give disparate dates – for example, bone dates often seem to be several hundred years younger than associated wood-charcoal dates, as suggested by C. V. Haynes (1992). At least one-half of all radiocarbon dates returned over the past half-century probably have been rejected or suppressed because of suspected errors. This might make readers nervous that the *true* dates of Clovis could be quite different from the 11,500–10,500 radiocarbon years generally accepted. The possibility that Clovis sites are not correctly dated was raised (Roosevelt, Douglas, Brown, Quinn, Kemp, and Weld 1998 and Roosevelt, O'Donnell, Quinn, Kemp, Machado, Imazio de Siveira, and Lima da Costa 1998). However, when dating samples have been carefully collected and the lab protocols followed so that contamination is controlled or eliminated, sample selection is supportable, and different components of bone (such as collagen or non-collagenous proteins) are tested, the dating much more often produces results within the expected time interval (see Stafford 1988, 1994, 1999a; Stafford, Brendel, and Duhamel 1988; Stafford, Jull, Brendel, Duhamel, and Donahue 1987; Taylor 1991).

Over the last quarter-century, archeologists normally kept these cautions and qualifications in mind when addressing the big unanswered questions about the peopling of North America. Perhaps the biggest question was – and still is – this one:

(1) When did the first colonizers arrive?

In 1989 David Meltzer asked, "Why don't we know when the first people came to North America?" His suggested answer was that pig-headedness and entrenched archeological arguing in general have not by themselves made a consensus answer impossible, as some archeologists insisted; instead, or so he proposed, a central and underlying assumption about the very nature of colonization may have been dead wrong. Meltzer suggested that the Pleistocene migrations of founding populations were not discrete waves of separate groups, but were continuous dribbles over long periods of time. He also suggested that cycles of pre-Clovis colonists may have established themselves in the continent temporarily, long before the terminal Pleistocene, but then disappeared (genetically, morphologically, technologically, etc.) (Meltzer 1989a).

More than a decade later we still do not know when the first people came to North America. In fact, now we seem to be even less able to produce a reasonably acceptable approximation of an answer. Not only has the archeological background changed since then, but now the supplemental information about possible human populations in the New World is coming in waves of contradictory or confusing suggestions. At the time of Meltzer's article, many archeologists believed in a three-pulse migration model that accommodated archeological, linguistic, and bioanthropological data (Greenberg, Turner, and Zegura 1986). The limited amount of analyzed genetic and immunoglobulin data did not contradict the implications of the three-pulse model – namely, current Native American populations could be fitted into three language families (Amerind, Na-Déné, Eskimo-Aleut), as well as three parallel dental and genetic groupings, and three accompanying technological and chronostratigraphic groupings of archeological finds, whose earliest time of arrival followed the Last Glacial Maximum.

Sites that contained artifacts which did not fit into the three technological groups were often considered to be dubiously dated or suspiciously interpreted. Yet the discovery and publication of early sites or assemblages proceeded throughout the next ten years, even though the earlier sites, if valid and correctly interpreted, would have fatally weakened the three-phase model. An even more serious weakening of the model occurred when competing genetic, linguistic, and bioanthropological studies began to pile up in the literature. The first widespread appearance of such studies may have been adversarially motivated, and specifically intended to attack and replace the three-pulse model of colonization.

LINGUISTS HAVE THEIR SAY

Linguists had early on joined the movement to revise the peopling timeline. Joseph Greenberg used a method of comparing large numbers of languages at one time and his results led him to propose that all indigenous American languages fall into only three genetic groups ("language families") of unequal size (Greenberg 1987a; Ruhlen 1991, 1994), each having an Asian origin (Greenberg

1987b, 1996). The spread of the largest language family, Amerind, "must have been . . . within one or two thousand years" (Greenberg 1996:531). Greenberg's thesis was criticized passionately by other linguists (for example, Goddard and Campbell 1994), some of whom may have been "extreme" empiricists who insisted on "getting all of the local facts first before saying anything general, that is, before saying anything that was at all interesting" (Sapir 1987:663; see also Darnell 1987:653, 656). As a result competing theories emerged about the sources of American native languages. Many of the unhappy linguists were "splitters" who scorned Greenberg's attempt at "deeper classification [of language] in the Americas" (Greenberg 1987b:666) because they saw no possibility of reconstructing direct ancestral connections among so many distinct languages. A "majority of linguists working on American Indian languages" believe in the existence of 100 to 200 pre-Columbian linguistic stocks lacking affinities (Greenberg 1996:532), as opposed to Greenberg's classification of all languages into either eleven stocks or six "branches" or aggregated stocks (Greenberg 1987a:60, 378) which were subsumed in three families.

The points of view of both Greenberg and his adversaries predisposed them to expect either a few or multiple migrations; Greenberg's adversaries expected to see many waves of different language-speakers entering the New World throughout longer spans of time than Greenberg had proposed based on his sweeping approach to language classification, his knowledge of the dated archeological finds, and the biological connections between Old and New World populations.

The linguist Johanna Nichols (1990), building on the earlier work of Austerlitz (1980), and based on what she believed to be a relatively high density of linguistic stocks in the New World, proposed that the separation of all indigenous American languages must date back at least 35,000 years to explain her linguistic model. Greenberg has suggested that part of the disagreement between him and Nichols (and other linguists) about the number of linguistic stocks in the New World is due to the use of the widely applied comparative method (which compares languages presumed to be related), rather than Greenberg's "mass" or "multilateral" comparisons (which examine broad arrays of languages in order to note "how they group genetically" [Greenberg 1996:535]).

In later publications, Nichols estimated that over the last 6,000 years, about 1.5 "language families" have separated out in each ancestral "language stock" in the northern hemisphere, and she used that figure and allowances for new migrations to predict that the New World's 140 language families separated out over at least 40,000 years (summarized in Gibbons 1998). However, if the number of language families is lower, as Greenberg's methods conclude, then the separation time also could have been much reduced.

Greenberg (1987a, 1987b:665) had pointed out that linguistic studies are very weak when trying to uncover absolute chronologies – such sequences of time cannot be reliably discovered. Thus, a linguistically inspired estimate of 35,000–40,000 years in the Americas is hardly a steadfast datum point. Nichols herself has cautioned that linguistic reconstructions of origins, movements, and language spread are not simple and easily proven; she noted (1997) that "the rate at which languages diverge . . . is not constant," and that "given present knowledge of language change and probability . . . descent and reconstruction will never be traceable beyond approximately 10,000 years." Languages change rapidly when in close contact with other languages. Linguistic density is highest

in areas where autonomous small societies live in small territories with year-round and reliable food resources (Nettle 1998), and "neither time settled nor number of colonizations has any appreciable effect on genetic density, which is determined entirely by geography, population density, and economy." Therefore, it would seem that the only supportable test of an estimate of the time elapsed since language divergence is an age estimate in harmony with archeological or geological data (Nichols 1997). Because solidly dated archeological sites in the 40,000-year-old range are not generally available in North America – unless one accepts such finds as the mid-Pleistocene flaked stones at the Calico site in southern California (Budinger 2000; Leakey, Simpson, and Clements 1968; Patterson 1999; Simpson, Patterson, and Singer 1986) – the linguistics-based estimates of deep age must be viewed as untested (perhaps untestable) hypotheses about New World colonizers.

PHYSICAL ANTHROPOLOGISTS DO SOME MEASURING UP

A relatively small sample of fossil human skeletal material over 8,000 years old has been available from the Americas (Table 1.2). These remains are not uniform, but all of them are anatomically aligned exclusively with modern *Homo sapiens sapiens* (Steele and Powell 1994). Thus, based on the fossils, no suggestion of an earlier hominin population such as archaic *H. sapiens* in the New World has been taken seriously. Unfortunately, even as more and more studies are made of the skeletal sample's morphologies and genetics, several different scenarios and models still compete to explain how these individuals came to be in the Americas so long ago.

Jantz and Owsley (2001) could find no morphometric affinity between early Native American crania and recent Native Americans; instead the greatest (but still imperfect) affinity seemed to be with Europeans, Polynesians, and East Asians. The earliest skeletal samples from North America are not neatly assignable to any extant geographic or ethnic populations. Neves and Pucciarelli (1991), on the basis of craniometric studies, proposed that the first colonizers of the New World were from a generalized "non-Mongoloid" population. Either another wave of people characterized by "Mongoloid" cranial traits later contributed to the population, or the Mongoloid traits appeared by local evolution (see also González-José, Dahinten, Luis, Hernandéz, and Pucciarelli 2001). Neves (2000; Neves and Pucciarelli 1991) concluded that the earliest South American founding populations had a "marked morphological affinity with present-day Africans and Australians," and show no resemblance to present Asian Mongoloid physical types or American Indians. Africans and Australians, it must be remembered, have been geographically separated for over 50,000 years. Chatters, Neves, and Blum (1999) interpreted the 7,900-radiocarbon-year-old Kennewick (WA) skeleton as similar to Polynesian or Ainu in size and shape, but not similar to Mongoloid, thus adding yet other possible source areas for Native American ancestry (although no Polynesians lived in the Pacific before about 3,500 years ago). Thus the oldest known crania are not "Mongoloid," while those from the more recent Native Americans are.

Native Americans are physically and genetically variable, as are other people in the rest of the world. Compared with Asian populations, they are more genetically differentiated, their anthropometrics are more variable, and their mtDNA also

TABLE 1.2 Human skeletal finds older than 8,000 radiocarbon years.

LOCALITY (REFERENCE)	N	REMAINS	AGE (RCYBP)
Anzick, MT (1)	2	cranial fragments	8,620–10,500; later redated 11,500
Arlington Springs, CA (2)	12	femora	10,000 ± 310; collagen redated 10,960–11,500
Browns Valley, MN (3)	1	skeleton	8,700 ± 110
Buhl, ID (4)	1	skeleton	10,700
Fishbone Cave, NV (5)	1	postcranial fragments	10,900–11,200
Gordon Creek, CO (6)	1	skeleton	9,700 ± 250
Horn Shelter, TX (7)	2	skeletons	9,000–10,000
Kennewick, WA (8)	1	skeleton	8,410 ± 60
La Brea, CA (9)	1	skeleton	9,000 ± 80
Marmes, WA (10)	3	cranial fragments	10,000–11,000
Mostin, CA (11)	1	bone fragments	10,000–11,000
Pelican Rapids, MN (12)	1	skeleton	–
Sauk Valley, MN (13)	1	skeleton	–
Shifting Sands, TX (14)	1	tooth fragments	–
Spirit Cave, NV (15)	1	skeleton	9,400
Vero Beach, FL (16)	1	cranial fragments	–
Warm Mineral Springs, FL (17)	1	postcranial fragments	10,620 ± 190
Whitewater Draw, AZ (18)	2	skeletons	8,000–10,000
Wilson-Leonard, TX (19)	1	skeleton	9,000–11,000
Wizards Beach, NV (20)	1	skeleton	9,200

References

1 Stafford 1999a; Stafford, Jull, Brendel, Duhamel, and Donahue 1987, D. C. Taylor 1969
2 Orr 1962; Stafford 1999a 3 Jenks 1937
4 Green, Cochran, Fenton, Woods, Titmus, Tieszen, Davis, and Miller 1998
5 Orr 1956, 1974 6 Breternitz, Swedlund, and Anderson 1971
7 Young 1988 8 Chatters 1999 9 Berger 1975; Kroeber 1962
10 Fryxell, Bielicki, Daugherty, Gustafson, Irwin, and Keel 1968
11 Kaufman 1980; Taylor, Payen, Prior, Slota, Gillespie, Gowlett, Hedges, Jull, Zabel, Donahue, and Berger 1985
12 Jenks 1936 13 Jenks and Wilford 1938
14 D. G. Steele and Powell 1994, citing Owsley pers. comm.
15 Edgar 1997; Jantz and Owsley 1997; Tuohy and Dansie 1997
16 Stewart 1946 17 Clausen, Brooks, and Wesolowsky 1975
18 Waters 1986 19 Steele 1989; Weir 1985
20 Dansie 1997; Edgar 1997; Tuohy and Dansie 1997

shows more variability (but less variability than Europeans). Hence the question arises: how variable were the founding populations, to have allowed this much unusual variability now? Or did the variability derive from multiple migrations, or even from *in situ* evolution? The human fossils dated older than 8,000 years are at least as variable as modern samples from the same geographic area.

In 1991, Cavalli-Sforza described work that brought together genetic, archeological, and linguistic data (Cavalli-Sforza 1991, 1997) to reinterpret human evolution, and decided that the first human entry into the Americas most likely occurred between 15,000 and 35,000 years ago, somewhat encouraging a stretching of the earlier three-pulse model farther back in time. Wallace and Torroni (1992) proposed a view of "American Indian prehistory as written by mtDNA," in effect finding that one of the three linguistic groups proposed by Greenberg, namely Amerind, originated from two Asian migrations dated four times older than the Na-Dené migration. Thus, in this interpretation only two different migration waves populated the continent – the ancestors of Amerind and of Na-Dené.

A rush of exciting new DNA studies began to appear in the mid-1990s. By 1996 some genetic studies, particularly of mtDNA, seemed to indicate that either all New World native populations shared a single ancestry in Asia – one migration introduced all the mtDNA (Merriwether and Ferrell 1996; Merriwether, Hall, Vahlne, and Ferrell 1996; Merriwether, Rothhammer, and Ferrell 1994, 1995) – or they derived from only two pulses of migration (Torroni and Wallace 1995; the views are summarized in Gibbons 1993, 1996). Yet, while suggesting the three-pulse model was not correct, these studies could not find common agreement about the timing of this one-wave or the two-wave scenario, owing to the fact that the human bone samples analyzed did not possess significant antiquity and may have reflected relatively recent losses of genetic diversity or intermixing of older genetic lineages (O'Rourke, Hayes, and Carlyle 2000). Still, one group of researchers claimed that the genetic data indicated the first wave appeared 20,000 to 25,000 years ago (Gibbons 1996). Unfortunately, as geneticists were realising, "There are just too many different histories compatible with present-day patterns of genetic variation" (Goldstein 2000:62).

THE MONTE VERDE MOMENT

While these sorts of interesting but unreconciled studies were being read and debated, several archeologists were making a concerted push to persuade colleagues to accept the validity of pre-Clovis dating on materials from a site named Monte Verde in Chile (Adovasio and Pedler 1997; Fagan 1997; Grayson 1998; Meltzer 1997; Meltzer *et al.* 1997). By 1998 some of the so-called "revisionists" who wanted to rewrite the peopling scenarios were presenting their ideas at a meeting of the American Association for the Advancement of Science, a heavyweight organization indeed. These different views were termed "the culmination of a series of spectacular recent discoveries that have contradicted the accepted model and thrown the field of Paleo-Indian anthropology into turmoil" (McDonald 1998:A22).

According to McDonald, the "widespread acceptance" of the pre-Clovis dates from Monte Verde "lent credence to the authenticity of sites older than Monte Verde that had been bitterly contested by adherents of the traditional view of

migration" (McDonald 1998:A22). The controversial sites in question had been dated to 12,000 to 20,000 years old, and in some cases even older. Up until the moment that leading archeologists announced that Monte Verde must be accepted as older than Clovis, the long lists of pre-Clovis localities were viewed as Elvis sightings, hardly taken seriously by many in the archeological community.

Monte Verde gained its widespread acceptance because of pronouncements made by a few prominent archeologists who had visited the site (Adovasio and Pedler 1997; Meltzer *et al.* 1997). However, at least one of the Monte Verde visitors later rejected his role as a *miraculé* cured of his Clovis-first affliction when he later reconfirmed his feelings of doubt (C. V. Haynes 1999a).

A more convincing source of authentication for all the other archeologists unable to visit the site should have been the huge published second volume (Dillehay 1997) detailing the Monte Verde site's findings and interpretations. This 1,100-page book was praised by reviewers (for example, Fagan 1997, who called the book brilliant; also Grayson 1998; Meltzer 1997) and won the Society for American Archaeology's 1998 book award. But Stuart Fiedel (1999b), like the ancient mariner, wrecked the three reviewers' celebration when he zeroed in on the book's numerous problems such as inadequately documented key finds and maps or tables that contradicted each other (see Rose 1999a). Dillehay and eighteen colleagues (Dillehay *et al.* 1999a) and other interested parties (Adovasio 1999; Collins 1999b; Meltzer 1999) responded to Fiedel's "hostile" remarks (Dillehay and six others 1999b), hoping to clarify or dismiss the criticisms as unimportant or understandable in view of the Monte Verde project's complexity. Dillehay and six colleagues also published a much longer and irritable website reply to Fiedel (Dillehay *et al.* 1999b), calling his comments "biased and error-ridden," and his questioning an "unscrupulous" challenge to the site, upon which Fiedel (2000b) returned to the as yet unresolved problems, and declared that the extremely important report about Monte Verde did "not rise to the modest standard of an average Phase II cultural resources report" (Fiedel 2000b). The fortissimo exchange of views had been pumped up to furioso very quickly, but it was allowed to diminuendo publicly (although not in the private backrooms) (Thomas 1999) through the years 2000 and 2001.

DEVELOPMENTS AFTER THE MONTE VERDE CONVERSION EVENT

While the Monte Verde quarrels raged, certain other prehistorians were elaborating the emerging hypothesis that the morphology of the earliest known American skeletons (mostly dated to the early Holocene and possibly late Pleistocene) reflected an early pulse of migration whose human populations died out completely and had little or no relation at all to later Amerind populations. One possible implication was that pre-Clovis migrations were not only early, but also unrelated to later migrations, and derived from very different regions of the Old World. Another alternative possibility was that small groups of people from different homeland areas entered the New World and lived separately from each other, sometimes scattering quite widely over the continent. Over time, genetic drift and natural selection occurred, changing gene frequencies and sometimes eliminating some DNA altogether. Later, Holocene gene

flow from northeast Asia added a large input to the New World genetic reservoir to create the modern Amerind morphology and genome.

According to one popular writer, 1998 was the year that "the fall of the Clovis barrier" provoked excitement at the annual meeting of American archeologists in Seattle (Hall 1998). However, the apparently better-educated physical anthropologists, human biologists, and geneticists who had already "long inferred pre-Clovis entry of people into the Americas" (Hall 1998:13) showed far less excitability at their own annual meeting in Salt Lake City. What did emerge from the physical anthropology meetings were more seemingly incongruous suggestions about the timing, origins, and spread of the earliest American populations.

By early 1999, anthropological geneticists realized that DNA research could be interpreted in at least two different ways, depending on one's fundamental model of the human fossil record, and the two ways led to contradictory conclusions. In one model, analysts assume that DNA differences between populations arose almost exclusively after geographic isolation of those populations. The other model would have genetic variations arising from interbreeding of populations at very low levels in combination with random mutational changes or founder-effect. The two models have led to two views about human evolution: either all modern humans are descended from "founding" populations that originated in Africa after 200,000 years ago and began to migrate out about 60,000 years ago; or modern humans are descended from interbreeding regional populations around the world that were in place 100,000 to 200,000 years ago.

The annual meeting of American physical anthropologists in 1999 saw more conflicts in interpretations of the first peopling of the Americas. Some scholars favored two migration waves; but some did not. Some favored the idea of "continuity" of populations, meaning that microevolutionary changes within regions accounted for morphological differences over time; some favored "replacement" of populations, meaning that successive waves of immigrants added new genes swamping the old ones over time. Some favored a relatively "late entry" of populations, and some did not (see the summary in Hall 1999).

By the year 2000, craniometric studies by physical anthropologists continued to emphasize that the earliest known human remains from the Americas did not closely resemble modern Native Americans, although there were some similarities. The usual conclusion reached about the variability in American skulls was that, of the several geographically separate founding populations which migrated to the New World, the earliest had been the least similar to the modern native people. The first founders must have pre-dated the development of Mongoloid populations, to whom Native Americans are most closely similar. Gene flow was uninterrupted over time, however, and after Mongoloid microevolutionary development in Asia the New World populations continually received an input of Mongoloid genes and traits.

Of course, it must be remembered that shapes and measurements of prehistoric human skulls are not always clear reflections of ancestry. If fossil skulls do not match modern ones from the same locales, but do show similarities to skulls from more distant world areas, the differences need not result from ancestry alone. For example, 9,000-year-old skulls from the Elmenteita site in Kenya are more similar to Peruvians, Ainu, and Europeans than they are to modern Africans, a similarity that is not at all due to any temporally close relationship to those other ethnic groups (Howells 1995). The similarities are due