WHY DON'T WE KNOW WHEN THE FIRST PEOPLE CAME TO NORTH AMERICA?

David J. Meltzer

The question of when the first people came to North America defies consensus. Data from an array of fields would seem to narrow the number and timing of migrations, but that evidence is at best circumstantial and cannot be used to constrain what is strictly an archaeological matter. Advocates of a pre-12,000 B.P. human population assert that their evidence is valid and is rejected by skeptics only because of deep-set historical biases. That assertion is not well-founded. If a bias exists, it is in the assumption that there were only three discrete migrations, the earliest of which was Clovis. The possibility that these migrations were not discrete episodes involving small founding populations, but instead may have been migratory dribbles spread over thousands of years, has implications for understanding the variation evident among modern descendant populations and the archaeological variability of Clovis. The possibility that there were early, pre-12,000 B.P. migrations that may have been wholly unrelated to Clovis and failed, may have equally important implications for why we don't know when the first people came to North America.

Hasta la fecha no ha sido posible llegar a un acuerdo en cuanto al problema de cuándo vinieron los primeros humanos a la América del Norte. Los datos de una variedad de campos científicos parecen limitar el número y la fecha de las migraciones, pero esta evidencia es circunstancial a más y mejor y por esto no se puede utilizarla para restringir lo que en realidad es estrictamente un asunto arqueológico. Los defensores de una población humana anterior a los 12,000 años antes del presente sostienen que su evidencia es válida, y que ha sido rechazado por los escépticos sólo a causa de sus profundos prejuicios históricos. Esta aseveración no es de veras verídica. Si existen prejuicios éstos se enfocan en la suposición de que sólo hubo tres migraciones distintas, la más temprana de las cuales fue Clovis. La posibilidad de que estas migraciones no fueran episodios distintos que consistían en pequeñas poblaciones fundadoras, sino que fueron goteras migratorias que continuaban por miles de años, tiene implicaciones para entender la evidente variación entre las poblaciones modernas descendientes y también la variabilidad arqueológica de Clovis. La posibilidad de que hubieran migraciones tempranas anterior a los 12,000 años antes del presente que no se relacionaron a Clovis y que además fallaron, quizá tenga implicaciones importantes para nuestro desconocimiento sobre la fecha de la primera llegada de los humanos a la América del Norte.

The Folsom and Clovis discoveries, which earlier in this century firmly established that early North Americans were here in the late Pleistocene, were no sooner out of the ground when archaeologists began the next cycle of pursuit of still older human occupations of the Americas. Since then, scores of purportedly ancient sites have appeared, some with estimated ages upwards of 200,000 years B.P. (e.g., Simpson et al. 1986).

Each new candidate for great antiquity brings with it fresh claims, but the outcome remains the same. Skeptics raise questions. Debate ensues. The claim is accepted by some, rejected by others, the remainder wait and see. The net result, so far, is that the Clovis "barrier" remains intact. A pre-12,000 B.P. human occupation in North America does not now exist publicly.

That early North American material should be so disputable is puzzling. After all, the implications of a pre-12,000 B.P. occupation simply are not all that profound. Knowing that American stone tools may date back to 14,000 B.P. (at, for example, Meadowcroft) only tells us that North American prehistory is a couple of thousand years older than we used to think.

Equally puzzling, the dispute occurs despite impressive converging evidence from an array of ancillary fields (notably linguistics, genetics, physical anthropology, and Pleistocene geology) that seems to clarify the number and timing of migrations to North America (e.g., Greenberg et al. 1986; Kirk and Szathmary 1985). It remains unresolved in the face of a regular crop of purportedly ancient

David J. Meltzer, Department of Anthropology, Southern Methodist University, Dallas, TX 75275

American Antiquity, 54(3), 1989, pp. 471–490.

assemblages and archaeological sites, and an almost-annual harvest of books and papers on the topic (e.g., Blackburn 1985; Bryan, ed. 1986; Carlisle 1988; Dincauze 1984; Ericson et al. 1982; Irving 1985; Morlan 1987; the "First Americans" series in *Natural History* 1986–1987; Owen 1984; Shutler 1983; Waters 1985; West 1983). There are likely many reasons why this issue defies consensus, from theoretical and epistemological inadequacies that exacerbate ambiguities in the data (Dillehay 1985:197–198; Dincauze 1984:299), to differences "as much psychological as archaeological" (Bray 1988:107).

Much already has been written on these matters, and I add my voice to the chorus only to discuss three issues that warrant further inquiry. The first issue is the strategy of using nonarchaeological and certain archaeological evidence to set limits on when people could have come to America. This popular and long-standing approach seemingly provides firm evidence for determining the relative and absolute antiquity of Native American groups, but this evidence may be more apparent than real. The second issue to be examined is the belief, strongly held in some quarters (Adovasio et al. 1987; Alsoszatai-Petheo 1986; Bryan 1977, 1986; Irving 1985), that for any pre-12,000 B.P. claims to be accepted they must convince modern critics, shatter 50 years of archaeological myth, and overcome a critical tradition that reaches back to the turn of the century. The perception that history is not merely passive, but an active obstacle to modern advance, is a troubling one and warrants careful scrutiny. The last issue that deserves attention is the manner in which migration to North America is viewed. The assumptions that the earliest migration involved a single, homogenous population, and that pre-12,000 B.P. occupations must be ancestral to Clovis, are not well-founded. They potentially limit the understanding of Clovis and subsequent populations, and divert attention from the possibility that there were multiple earlier migrations or ones wholly unrelated to Clovis that failed. All this, in turn, has implications for why we may not know when the first people came to North America.

This discussion focuses on North rather than South America, despite the very provocative evidence emerging in the latter area (e.g., Dillehay and Collins 1988; Guidon and Delibrias 1986), primarily because the port of entry for the initial peopling of the New World almost certainly was northwestern North America. Even if people were in South America before 12,000 B.P., one must account for why their traces have not yet been detected in North America. Beyond that, much of the discussion of the peopling of the New World uses North American terms (the label "pre-Clovis," generally applied, is irrelevant in South America where Clovis has never been found) and, some suggest, biases, that should be evaluated by the evidence from which they were derived.

SETTING BOUNDARIES ON THE NUMBER AND TIMING OF MIGRATIONS TO NORTH AMERICA

Who, When, and Wherefrom?

In 1935, Edgar B. Howard, in the midst of his Clovis work, wrote to a number of scholars in an effort to find out "what today may be the consensus of opinion among scientists regarding the length of time since Man's arrival in the New World" (Howard, E. B. Letter to F. Boas. May 9, 1935. Boas Papers. American Philosophical Society, Philadelphia). Franz Boas, among others, responded:

> In my opinion the time since the end of the Ice Age is not sufficient to account for the diversification of types and languages on the American continent, and I am inclined to set the time for the immigration from Asia towards the end of the Glacial period when Bering Sea was still land and there was an opportunity for land intercourse between the two continents [Boas, F. Letter to E. B. Howard. May 13, 1935. Boas Papers. American Philosophical Society, Philadelphia].

Polling one's colleagues for a consensus on the question of human antiquity in America was something new, but Boas's response was not. The idea that language diversity bespoke a great antiquity for the native Americans came into common use 150 years earlier (e.g., Jefferson 1787:Query XI), long before a late Pleistocene human occupation of the Americas was ever demonstrated and even before the demonstration in Europe that there was such a thing as *pre*history.

It was suspected early on, however, that the diversity of the Native Americans could not result solely from the in situ evolution of a single ancestral population. The problem, as Edward Sapir realized, was that "If the apparently large number of linguistic stocks recognized in America be assumed to be due merely to such extreme divergence on the soil of America as to make the proof of an original unity of speech impossible, then we must allow a tremendous lapse of time for the development of such divergences" (Sapir 1949:454). That tremendous lapse of time was far greater than "more conservative archaeologists and palaeontologists are willing to allow as necessary for the interpretation of the earliest remains of man in America" (Sapir 1949:454). The archaeological record then set boundaries on linguistic interpretation; decades later, others would reverse that relation. Sapir reasoned it was better to assume that the peopling of America was not a single historical process but a "series of movements of linguistically unrelated peoples, possibly from different directions and certainly at very different times" (Sapir 1949:454–455; also Holmes 1919: 55–56; Hrdlička 1925:493). Recent studies of native language (Greenberg 1987; Greenberg et al. 1986); physical data (Harper and Laughlin 1982; Szathmary 1985; Turner 1983, 1985a, 1986, 1987; Williams et al. 1985; Zegura 1985, 1987); and, to a lesser degree, cultural diversity (Suarez et al. 1985) reinforce Sapir's conclusions.

Greenberg's prominent (although not universally accepted) linguistic work, for example, suggests that North American native languages can be divided into three groups: Amerind, Na-Dene, and Eskimo-Aleut (Greenberg 1987:38; Greenberg et al. 1986; Ruhlen 1987). These groups are thought to be the product of three distinct migratory pulses (Greenberg 1987:333), though Greenberg and his colleagues urge caution here. The tripartite classification of modern languages "does not in itself prove that there were three distinct migrations from Asia" (Ruhlen 1987:10). They believe there could have been *fewer* than three migrations, although they assert that *not more* than three migrations occurred (Greenberg et al. 1986:494; Ruhlen 1987:10; cf. Greenberg 1987:333).

The relative order of the three migrations is determined partly by internal differentiation within each language group, partly by respective similarities to Old World languages, partly by the modern distribution of the language groups (which are generally exclusive of one another), and partly by their presumed archaeological affinities (Dumond 1987; Greenberg 1987:333–334; Greenberg et al. 1986:478–479). Sapir (1949:455) had inferred that the "latest linguistic arrivals in North America would probably have to be considered the Eskimo-Aleut and the Na-Dene." That conclusion is seen as supported by other recent evidence (Dumond 1987; Greenberg 1987; Greenberg et al. 1986; Ruhlen 1987) which puts the Na-Dene migration at 10,000 B.P., with ancestral Eskimo-Aleut groups appearing ca. 4500 B.P. (Dumond 1987:50–51). These later migrations, which are younger than Clovis, are seen as irrelevant to when the first people came to North America.

In this formulation the first migration is manifested linguistically by Amerind, a "vast assemblage" of languages that in ethnohistoric times was spoken by peoples living from Hudson's Bay to Tierra del Fuego (Greenberg 1987:38, 387). That assemblage includes 11 stocks, of which two branches (Northern and Central Amerind) comprise those languages spoken in North America (including Algonquian, Iroquoian, Tanoan, and Uto-Aztecan, among many others). It is assumed that Clovis is the archaeological trace of the ancestors of these groups. Linguistic evidence suggests that the ancestral Clovis–Amerind migration occurred sometime before 11,000 B.P. and before the limits of glottochronology (Greenberg 1987:335; Greenberg et al. 1986:480).

Dental evidence, according to Turner (1983, 1985a, 1986, 1987), supports Greenberg's three-part linguistic grouping of North American populations. That evidence comes from teeth of pre-Contact native groups (Turner 1986:37), and includes the exceedingly rare samples of teeth from latest Pleistocene and earliest Holocene archaeological sites in America and Asia. Whether the tripartite dental clusters represent three separate migrations from Asia "cannot be determined by the dental evidence alone" (Turner 1987:8). But, as was true of native languages, it is difficult to explain these dental clusters as evolutionary divergence from a single American source (Turner 1985a:41–46).

Patterns of shared traits among the teeth from different groups, the age of certain teeth samples and, most important, an assumed rate of dental microevolution imply a common ancestor of all Native American populations in North China about 20,000 B.P., and a divergence of Amerind groups from that population around 14,000 B.P. (Greenberg et al. 1986:484–485; Turner 1987:8). This geologically late event is supported, as Hrdlička earlier noted (e.g., Hrdlička 1907, 1918), by the absence of fossil evidence for any variety of hominid in North America other than *Homo sapiens sapiens*. Of course, there are hardly any Pleistocene human fossils in North America (Szathmary 1985:79; Zegura 1985:6; for a recent review of earliest North American human skeletal evidence, see Young [1986:122–143]).

Genetic and immunoglobulin data (Suarez et al. 1985:222–223; Szathmary 1985; Williams et al. 1985) corroborate only weakly the tripartite division of North American populations and the same number of migrations (Greenberg et al. 1986:487; Szathmary 1986:491; Zegura 1987:11). The "guess" dates provided for the earliest time of genetic divergence from the ancestral Asian populations range from 40,000 to 16,000 B.P., a range based not on independently derived genetic data but on archaeological and dental evidence (Turner 1985b:8; Williams et al. 1985:1, 13). Genetic evidence alone cannot provide independent estimates of divergence time (Zegura 1987:10).

Evidence from Pleistocene geology has long been sought to provide chronological control for these migration models (e.g., Antevs 1935; Johnston 1933; Wendorf 1966). Beringia, the continuous landmass exposed when sea level in the Bering Sea drops 46 m below its current level, likely was present for the final time sometime between 35,000 and 14,400 B.P. (less is known of sea levels prior to this time). It remained traversable on foot in winter at least until 10,000 B.P. (Bloom 1983; Hopkins 1982; McManus and Creager 1984).

Getting across, however, and then moving into North America were two separate events (Wendorf 1966). Time of lowest sea levels between 19,000 and 16,000 B.P. roughly corresponded with the time of maximum northern hemisphere ice extent. Coalescence of the Cordilleran and Laurentide ice sheets east of the Rocky Mountains in British Columbia and Alberta effectively could have blocked movement to the south (Haynes 1969:713). An ice-free corridor presumably was open prior to maximum Wisconsin times and following the meltdown of the ice masses after, say, 14,000 to 12,000 B.P. (MacDonald 1987; White et al. 1985).

Taken together with paleoecological evidence (Schweger et al. 1982; Yi and Clark 1985:12), this information would seem to suggest that if groups came across Beringia into North America in one relatively straight course of travel, it would have been possible sometime after 35,000 B.P. but before 19,000 B.P., and again after 14,000 B.P. but before 12,000 B.P. (the last age being the archaeological appearance of Clovis). Since the linguistic, genetic, and dental evidence "supports a relatively late initial peopling of the New World" (Greenberg et al. 1986:485; Harper and Laughlin 1982:298), in the view of these authors the post-14,000 B.P. period would seem the most likely for the first migration to North America.

Attractive as this line of reasoning may appear—and to some it is quite attractive—it unravels under close scrutiny. There is simply no particular reason to believe this scenario, or any other such scenario (one could devise quite a few others). As a host of authors have argued (e.g., Bordes 1978; Bryan 1986:3; Chamberlin 1903:64; Fladmark 1983:40; Grayson 1988:113–114; Nelson 1933:95; Sapir 1949:398), because these kinds of "alleged facts are merely circumstantial" (Nelson 1933:95) they cannot set limits on the number of migrations and the timing of the colonization of North America.

There may have been only three migratory pulses, and the earliest group may have come across Beringia when it was dry land, and they then may have gone through an interior continental route to arrive south of the ice sheets. But, then, they may not have.

Coming to North America was not an event that was physically impossible except along circumscribed routes within narrow time windows. There was not one, but many possible routes (Fladmark 1983) open at many different times. Beringia was a passageway through which there could have been hundreds, perhaps thousands of separate arrivals of small populations from Asia, and many movements back to Asia over tens of thousands of years. Even if we did know the precise timing of the Land Bridge, which we do not (Hopkins 1982:14), or the timing of the ice-free corridor, which we do not (Porter 1988:2–4), that would all be irrelevant if the earliest migrants had boats and traveled down the Pacific coast (e.g., Fladmark 1979, 1986).

Even if Greenberg's notions about the historical affinities of North American language groups are correct, they cannot be used as evidence for the number of migrations since different immigrants over thousands of years may well have spoken related or even unrelated languages (Greenberg 1987:

42). Moreover, the process whereby modern language groups or genetic systems are correlated with archaeological materials nearly 12,000 years old is, to say the least, "highly speculative" (Zegura 1987:10) (although discoveries like the early Holocene brain tissue from skulls at the Windover, Florida, site [Doran et al. 1986] hold some promise for tightening genetic links between modern and ancient populations). There are, as well, discrepancies in the conclusions reached by linguistic, dental, and genetic evidence (e.g., Fox 1986:489). Finally, one cannot preclude the possibility that divergence took place in America and not Siberia (e.g., Laughlin 1986:490; Szathmary 1986:491).

It is therefore wrong to dismiss archaeological evidence because it fails to match the chronological expectations derived from nonarchaeological sources (e.g., Owen 1984:524–526; cf. Turner 1985b: 8). Recall that time estimates derived from these sources are based on assumptions of rates of linguistic (or genetic, dental, or even demographic [Hassan 1981:202]) change, or circuitously on archaeological evidence (Harper and Laughlin 1982:298). The fact that many see, from those special vantage points, a "suggested departure date of about 20,000 years ago" (e.g., Greenberg et al. 1986: 494; Hassan 1981:202) may only mean that all are reading the same archaeological publications. If a site is found that is older than it "should" be based on evidence from language, teeth, and genetics, then that nonarchaeological evidence will have to be adjusted accordingly (Turner 1985b:8).

Similarly, neontological and ethnohistoric evidence cannot reveal *which* possible route was taken and which routes were not (Zegura 1987:10), though some archaeologists using linguistic evidence assert as much (e.g., Gruhn 1988; Rogers 1985a:133–136, 1985b:107). That assertion, however, is not based on the usual criterion of linguistic differentiation, but more simply on the observation that there are more native American languages along the Pacific Northwest and California coasts than in any other area of North America, which is said to imply "great time depth for human occupation" and thereby the corridor of entry (Gruhn 1988:84). The number of languages in any given region of North America, however, is hardly a function of time alone. There are a greater number of languages known from the Pacific Northwest and California primarily because it is one of the areas on the continent where indigenous populations weathered the deadly effects of European contact and disease and survived (though in an altered form) at least until the end of the nineteenth century when intensive linguistic fieldwork began in North America—by Boas, Kroeber, Sapir, and their students.

To claim that areas with fewer languages necessarily were inhabited relatively late (Rogers 1985a: 132), or could not have served as migration routes (Gruhn 1988:90), is equally misleading, since the native populations in most of those areas—such as the Missouri and Mississippi valleys—had collapsed (Ramenofsky 1987:173–174). They became extinct, as Jefferson lamented, "without our having previously collected and deposited in the records of literature, the general rudiments at least of the languages they spoke" (Jefferson 1787:Query XI). It is, of course, doubtful that the historic record of Native American settlement and language distributions bears any necessary relation to transitory migration routes, particularly those taken tens of thousands of years earlier across a radically different, partly ice-shrouded landscape, perhaps by peoples who may have been historically and linguistically unrelated (except in the general sense that all are ultimately Asian in origin [Greenberg 1987:61]). It is probably no more realistic to infer Pleistocene migration routes to North America by the number and distribution of modern language groups than it would be to infer Hernando de Soto's route by looking at the number and distribution of Spanish dialects in the Southeast today—and at least we know that de Soto spoke Spanish.

Setting Boundaries with Stone

Efforts to use archaeological evidence to set boundaries on when the first migration might have occurred and to predict what the accompanying assemblages might have looked like do not fare much better. For example, there has been much discussion of the earliest traces of human occupation in Siberia (e.g., Bryan 1978:309; Haynes 1982:395–396; West 1983:372–374; Yi and Clark 1985), and certainly knowing when people arrived in Siberia may help in setting the age for the earliest migration to America (Grayson 1988:113). For now this is a moot issue. Siberia is so poorly known archaeologically that there is no reason to believe that the earliest sites there have yet been found

(Morlan 1987:270). Before we can talk about the peopling of Siberia, Siberia will have to be peopled by archaeologists (A. Derevyanko, personal communication 1988).

Knowing what the pre-12,000 B.P. record should look like might help, but opinions on that score vary widely. Some hold that the toolkit will resemble an Upper Paleolithic assemblage with eastern European (Haynes 1987:87), or Siberian/Beringian (West 1983:372) affinities. Others believe that it will ("does") look "completely different" (Bryan 1986:5): more related to a Middle Paleolithic industry (Carter 1978:10), a Lower Paleolithic industry (Irving 1985:541; Simpson et al. 1986:92), or what has been termed a "paralithic" industry (Irving 1985:545, 1987; Irving et al. 1986). Old World archaeologists have not been especially impressed by efforts to class New World materials in these categories (Bordes 1978), perhaps because here in America, it seems, one can plunge from Clovis to Oldowan merely by crossing the 12,000 B.P. line.

Common to all such arguments of early technological affinities is the assumption that ultimately these pre-12,000 B.P. assemblages must "logically lead to Clovis" (Haynes 1987:86; also Bonnichsen and Young 1980; West 1983). That, in turn, implies it is possible to evaluate pre-12,000 B.P. claims by the degree to which they "fit" later Clovis assemblages.

But should a lack of affinities with Clovis be grounds for rejecting pre-12,000 B.P. claims? Consider what happened to the American "Paleolithic." Beginning in the 1870s there was much discussion about what American Paleolithic assemblages of Pleistocene age should look like (Meltzer 1983:7–17). All of the discussion was ultimately useless. When the Folsom evidence was confirmed in 1927, its assemblage looked nothing like what an American "Paleolithic" was supposed to look like. It was still late Pleistocene in age.

One can be assured nobody *could* have predicted what Folsom would look like, by the fact nobody *did* predict what Folsom would look like. So novel was such a thing as a fluted projectile point that it was not until he had seen nearly a dozen that Jesse Figgins, Folsom's investigator, was convinced that the "hollowed sides of the artifacts are *not* accidental, but the results of remarkable skill in chipping" (Figgins, J. Letter to O. P. Hay. September 7, 1927. Hay Papers. Smithsonian Institution Archives, Washington, D.C. [emphasis in original]).

In retrospect, and after the details of Holocene culture history had been filled in, the Folsom and Clovis assemblages were seen to conform quite nicely to later materials. It is now clear that there is a strong continuity between Paleoindian and subsequent early Archaic materials (which supports arguments of adaptive continuity as well [Meltzer and Smith 1986:18]). None of that was known at the time of the Folsom discovery (cf. Cook 1928:40). If a *pre-Clovis* record exists, then it, too, will in time be seen to "logically lead to Clovis." If a *pre-12,000* B.P. record is not a pre-Clovis record, it may never lead to Clovis.

When the first people came to the New World cannot be bracketed on a priori grounds, archaeological or otherwise (cf. Owen 1984:550–551). Quite simply, the timing of the entry of people into the New World is a question that can be answered only by doing archaeology in the New World (Bryan 1986:3; Graham and Heizer 1967:227; Grayson 1988:114; Turner 1985b:13). Chamberlin (1903:64) and others (e.g., Sapir 1949:398; Nelson 1933:95) realized this well over half a century ago; it remains so to this day.

Yet, what of the claims that there already is archaeological evidence of pre-12,000 B.P. occupations in America? Their proponents argue that evidence is genuine, and reason the claims have not been accepted because of certain obstacles, largely historical.

DOES HISTORY REALLY MATTER?

Mammoth Myths

Alan Bryan (1977, 1986) and others (e.g., Adovasio et al. 1987; Dillehay 1985, 1988) believe that our views of possible pre-12,000 B.P. occupations are influenced by expectations born of the Clovis material itself: "Because of the historical accident that the first recognized early sites were kill sites containing . . . projectile points in association with the bones of large mammals, the generally accepted model has been that the early colonists were specialized big-game hunters" (Bryan 1977: 355; reiterated in Bryan [1986:5]). The discovery of Paleoindian kill sites was no "historical accident." In preradiocarbon days the only means of determining the age of archaeological materials was if they were associated with known Pleistocene chronological markers. For more than half a century, artifacts were reported from glacial gravels, loess of purported Pleistocene age, and deposits containing Pleistocene fauna. But in all cases the evidence was rejected, largely because the association was deemed adventitious (e.g., Chamberlin and Salisbury 1906:509–512; Holmes 1893:161; Hrdlička 1917:48; McGee 1891:73). It is therefore pointless to speculate about what the subsequent history of Paleoindian studies would have been like had Meadowcroft been discovered before Folsom, for in those pre-Libby days such a site would not have been recognized as Pleistocene in age. Folsom, a kill site, was different: Projectile points embedded between the ribs of an extinct Pleistocene bison could not be so readily dismissed. In North America, at least, a kill site was crucial in the effort to tell time (Meltzer 1983).

Further, it was no accident that soon after the initial discoveries at Folsom and Clovis (in 1933), a series of like sites was found. Most sites were initially *paleontological* discoveries (Meltzer 1988: 3)—the bones of megafauna are visible more readily than artifacts—and the attention to the bone-bearing deposits led to the artifacts and, thus, to a Paleoindian record largely comprised of kill or scavenging sites (Dillehay 1988; Grayson 1988:114–115). In the early days Clovis sites with artifacts but no bones (non-kill sites) frequently were overlooked. There are still fewer non-kill Paleoindian sites known from the High Plains and Southwest.

The repeated discovery of "kill" sites led Roberts (1940:104) to conclude the "first migrants were unquestionably hunters." Later finds of fluted points outside the High Plains and Southwest, although lacking associated megafauna, were assumed to represent this pattern of big-game hunting as well (Grayson 1988:114; Meltzer 1988:4). This is the "fallacy of initial predication" (where a familiar characteristic of a phenomenon, or some characteristic known sooner than others, is taken as definitive of the phenomenon itself). Binford has called it myth-making (Binford 1981).

There are good reasons not to extend inferences about adaptive strategies from a biased sample derived from restricted, monotypic, low-diversity environmental settings to high-diversity ecological settings that would have required a wholly distinct set of adaptive responses (Meltzer and Smith 1986). Big-game hunting *may* (though many have serious reservations on this score) have played an important role in the lives of High Plains groups, but extending this pattern across environments to areas such as the Eastern Woodlands, as is routinely done, is questionable (Bryan 1977:357–360).

More important here, however, is whether the Clovis "myth" has been extended through *time,* and whether it influences views of what the earlier record should look like (Alsoszatai-Petheo 1986: 19; Bryan 1986:5). It has, to the degree that a few skeptics dismiss pre-12,000 B.P. claims because they fail to provide evidence of big-game hunting, which in their minds was the only adaptation that would have allowed a successful crossing of Beringia (Diamond 1987:580; Haynes 1982:397; Martin 1987:12–13). But the insistence that pre-12,000 B.P. groups had to have been big-game hunters certainly is not held by all skeptics, and Schweger et al. (1982:437) rightly insist that the question of whether people were present at a given time must be kept separate from more complex questions about how they lived (also Morlan 1987:294).

In any case, pre-12,000 B.P. advocates believe that unrealistic expectations of adaptive strategies are not entirely the problem. Rather, they assert that because of the influence of the Clovis myth, "acceptable criteria for early man must include definitely shaped ("diagnostic") artifacts (preferably bifaces) found in undisturbed Pleistocene contexts" (Bryan 1986:7; also Alsoszatai-Petheo 1986:21; Irving 1985:532). Because of this purported influence, skeptics naturally reject any discoveries of nondiagnostic pebble tools (Bryan 1986:9), which leads "to the recovery and recording of many fluted points . . . but it is not intended to lead to the discovery of evidence for 'pre-Clovis'" (Bryan 1986:7).

The fact of the matter is, however, that these sites are not being rejected for want of "diagnostic" artifacts; they are being rejected for want of artifacts, solid dates, and so on (Dincauze 1984). Furthermore, these portrayals of the criteria for an ancient site are not entirely accurate. The oft-cited criteria of Haynes are no more than "the minimum requirements met for the Folsom site" (Haynes 1969:714):

> The primary requirement is a human skeleton, or an assemblage of artifacts that are clearly the work of man. Next, this evidence must lie *in situ* within undisturbed geological deposits in order to clearly demonstrate the primary association of artifacts with stratigraphy. Lastly, the minimum age of the site must be demonstrable by primary association with fossils of known age or with material suitable for reliable isotopic age dating.

Other versions of these criteria are quite similar (e.g., Chamberlin 1903; Griffin 1979:44; Hrdlička 1907:98, 1912:2; Stanford 1983:65), and differ little from the criteria used to evaluate the world's earliest archaeological sites (Toth and Schick 1986:20). Such criteria could profit by expansion to include redeposited specimens (Morlan 1987:295), features, and other nonartifactual human traces, and could make explicit a requirement generally left unsaid, that there should be publication that allows the evidence to be assessed and areas of ambiguity or interpretive differences to be resolved.

Regardless, none of the versions demand (at least explicitly) "diagnostic" artifacts. Some individuals, of course, might *expect* to see such "diagnostic" forms in a pre-12,000 B.P. assemblage, on the assumption that any occupation preceding Clovis also must be historically and technologically related to it. Nonetheless, the criteria themselves separate what is expected from what is required. If legitimate pre-12,000 B.P. nondiagnostic artifacts are found, that merely indicates our expectations, but not our criteria, are wrong.

While the criteria as stated seem fairly straightforward, some believe they are vague and in need of clarification (Bray 1988). Others claim the criteria are insufficient to account for a potentially quite different pre-12,000 B.P. record (Bonnichsen 1978:102; Morlan 1987:295); still others argue that those criteria have been met many times over (Bryan 1978:323). The problem, it seems, is not in the criteria, but in the manner in which the data are perceived as evidence (thoroughly explored by Dincauze 1984) and evaluated by the archaeological community.

Ghosts of Hrdlička's Past

Recently Warwick Bray (1988:107) wondered aloud about the "make-up of the jury" in the controversy over pre-12,000 B.P. claims. He observed that for historical reasons "the international opinion-formers in Paleoindian studies are mostly English-speaking and based in North America, where convincing early sites have not been found. Inevitably, this situation leads to: "behind-the-scenes mutterings about sour grapes, nationalism, double standards of proof, and a view that some sites (and some researchers) are required to pass more stringent tests than others" (Bray 1988:107). Behind-the-scenes mutterings about critics are nothing new in this debate. They go back to the 1890s when William Henry Holmes and WJ McGee of the Bureau of American Ethnology came on the scene, and Paleolithic advocates grumbled of government intimidation and a "conspiracy" to thwart archaeological progress (Baldwin, C. C. Letter to G. F. Wright. April 3, 1893. Holmes Papers. Smithsonian Institution Archives, Washington, D.C.).

Indeed, it remains a popular notion (e.g., Alsoszatai-Petheo 1986:18; Lorenzo 1978) that because of the intransigence of a few early critics (notably Holmes and physical anthropologist Aleš Hrdlička), their dogmatism in the face of undeniable facts and, most important, the highly critical tradition they established in American archaeology, that human antiquity in America remains controversial to this day. Such would be a satisfying explanation and might serve as grounds to dismiss one's critics, were it not for a few troubling facts.

Holmes and Hrdlička were not alone in criticizing alleged Pleistocene-age materials (and certainly those who blame only Hrdlička miss the historical fact that the controversy began fully a decade before he became involved). They are merely the better known today of a group of anthropologist skeptics that included McGee, John Wesley Powell and, later, Neil Judd, Nels Nelson, and Leslie Spier. They were joined in their efforts by geologists and paleontologists, notably Thomas and Rollin Chamberlin, and Rollin Salisbury (University of Chicago); John C. Merriam (University of California, Berkeley, and later President of the Carnegie Institution of Washington); Chester Stock (California Institute of Technology); and Bailey Willis (Stanford). Early opposition to great antiquity on this continent was from many of the elite figures in turn-of-the-century anthropology and earth history.

In contrast, most of the advocates of great antiquity (with a few notable exceptions) were amateurs who lacked theoretical and methodological sophistication or (what then passed as) deep preparation in science (this nicely parallels the situation in mid-nineteenth-century Europe [see Grayson 1983]). So, for example, Sellards's (1916:131) report that a Pleistocene human skeleton at Vero had been found by Mr. Ayres and verified by Mr. Weills understandably prompted Hrdlička to wonder (as he wrote in the margin of his reprint of Sellards's paper): "Who are Ayres and Weills?" It was a legitimate question. In those days prior to radiocarbon dating and the development of more-sophisticated field techniques, and when claims of great antiquity were being made based on unverified discoveries by relic hunters or fossil collectors, critics had reason to wonder on what basis such finds were valid. This was particularly a problem because, as Frank Roberts (1940:51) later admitted, those propounding such finds displayed a "lack of knowledge and overenthusiasm . . . [that] led to conclusions that are obviously erroneous."

The idea that a critical individual, or even a cabal of such critics, dictated opinion *contrary to fact* (Alsoszatai-Petheo 1986:18–19) is also wrong. A useful exercise, as Griffin (1976:12, 1977:4) points out, is to consider the sites these critics rejected. In "no instance" is there a locality that they "checked out as postglacial or Recent in age [that] has proven otherwise" (Griffin 1977:4). Today, no one learns about the Claymont, Madisonville, or Newcomerstown paleoliths, the Nampa Image, or *Homo novusmundus* (Figgins), and for good reason. None of those artifacts, sites or human remains proved to be what they were claimed to be. Not one is even late Pleistocene in age; all are recent or in some cases downright frauds.

Lest these seem distant examples, there are some two dozen sites that Krieger (1964:Table 1) lists as preprojectile (pre-12,000 B.P.) in age, and a comparable number from a more recent list (MacNeish 1976:Table 1). Only a very few remain as viable candidates for great antiquity, even among pre-Clovis proponents (cf. Alexander 1978:20). The remainder failed to produce valid dates or artifacts under the glaring light of careful analysis. This, by the way, provides some measure of the shelf life of a controversial site (Martin 1987:13). It appears to be a little over a decade.

Returning to historical matters, consider the critics' reaction to the Folsom discovery. In June of 1927 Figgins visited the Smithsonian Institution and brought with him the Folsom material recovered the previous summer. Although expecting "some real cross words and scolding," he reported that Hrdlička was "extremely pleased and courteous," and advised him to leave any future discoveries "in situ and call in two or three scientists from other institutions to make detailed studies of the whole occurrence" (Figgins, J. Letter to O. P. Hay. July 1, 1927. Hay Papers. Smithsonian Institution Archives, Washington, D.C.). This was good advice: Site visits were then the only viable means of evaluating claims of great antiquity, and such visits were common from the 1890s to the 1940s.

At Folsom, two months after his visit with Hrdlička, Figgins's crew found more points in situ directly associated with bison bone. This time everything was left in place while telegrams were sent out. Roberts, A. V. Kidder, and Barnum Brown responded, and the three carefully examined the evidence and jointly concluded that the points had entered the deposit at the same time as the bison (Brown 1928; Kidder 1927; Roberts, F. Letter to J. W. Fewkes. September 13, 1927. Bureau of American Ethnology, National Anthropological Archives, Smithsonian Institution, Washington, D.C.).

Holmes, when queried about Folsom, deferred to the prevailing judgement (Holmes, W. H. Letter to E. H. Sellards. March 6, 1930. Holmes Papers. Smithsonian Institution Archives, Washington, D.C.). Hrdlička shied from the issue, but when confronted, spoke of the lack of evidence for a Paleolithic, or of skeletons of premodern sapiens (e.g., Hrdlička 1928:809). He also observed that the extinct animals with which Folsom was associated may "not have been extinct very long" (Hrdlička 1942:54; also Judd 1929:413–414). On all these points he essentially was correct. It was not then "wrong" to see a late survival of certain extinct species, and such a notion was common before (e.g., Antevs 1935:302–303; Colbert 1942:23; Roberts 1940:104–105; Romer 1933:66–74) and even after (e.g., Martin 1958) the development of radiocarbon dating, and was not fully disposed of until the 1960s (Martin 1967:97–102; Meltzer and Mead 1985:146–147).

The assertions that the Folsom finds were "rejected out of hand by established authorities" (Alsoszatai-Petheo 1986:18), or that they became "non-controversial" only after the development of radiocarbon dating twenty years later (Rogers and Martin 1987:82), are not supported in the historical record. Within a month of his visit to Folsom, Kidder confidently stated that the first journey to America "took place at least fifteen or twenty thousand years ago" (Kidder 1927:5, also Bryan 1929:128; Nelson 1928:823).

Some believe the ghosts of Hrdlička's past still haunt American archaeology. Lorenzo claims that "In our time, because of having been handed down this heavy burden, some archaeologists and geologists maintain the same position and reject or disqualify findings prior to a certain date" (Lorenzo 1978:2; see also Alsoszatai-Petheo 1986:18).

It is difficult to trace an intellectual ancestor-descendant relationship between Holmes and Hrdlička and, say, C. Vance Haynes (or even one between Paleolithic advocate Charles Abbott and Alan Bryan). Rather, as Grayson suggests, this "tradition of extreme skepticism" has an analog in the Holmes-Hrdlička era, but it thrives today because it has been reinforced so many times since Hrdlička's death (Grayson 1988:112). This is a case of convergence, not divergence.

To understand the emergence of the modern skeptics one need only turn to the recent rejection of the plethora of claims for ancient Californians (Taylor and Payen 1979; Taylor et al. 1985), ancient Canadians (Wilson 1984), ancient Texans (Stanford 1983), and ancient caribou-tibia fleshers (Nelson et al. 1986). Rejecting those claims has the same effect on the modern archaeological community as did the rejection of "one alleged find after another" (Bryan 1937:139) on the community a century earlier. As Nels Nelson put it, under such circumstances "no archaeologist can be expected to relinquish at once his skepticism" (Nelson 1918:394, also Bray 1988; Martin 1987: 13; Wissler 1916:235).

Skepticism particularly is warranted when the proponents themselves fail to evaluate critically alternative hypotheses for their own data. As F. W. Hodge once observed: Those "who are in favor of Pleistocene or even earlier man, always assume the highly unscientific attitude of endeavoring to prove the case without considering the evidence to the contrary" (Hodge, F. W. Letter to A. Hrdlička. June 1, 1928. Hrdlička Papers. National Anthropological Archives, Smithsonian Institution, Washington, D.C.). Recent examples of this can be found in Irving et al. (1986), Lorenzo and Mirambell (1986), and Simpson et al. (1986). Morlan (1986, 1987) is a noteworthy exception.

Given the nature of the dispute and the ambiguity in much of the data, no site can be accepted at face value, and only after critical evaluation will it be possible to accept or reject the evidence. Some have suggested that to undertake such a critical evaluation there should be a return to the time-honored tradition of visits by skeptics and proponents to the site during excavation (Adovasio et al. 1980:589; Dincauze 1984:295; Haynes 1969:714, personal communication 1988; Martin 1987: 13).

The zeal displayed by some of the critics in this debate has led a number of proponents to cry foul, a response understandable in light of the intellectual and emotional currency invested in these sites, and the tactics of those critics whose arguments are subject to the same flaws they find in others (e.g., Owen 1984). With that response is occasionally the claim that pre-12,000 B.P. sites are being judged by excessively rigid standards (Bray 1988; Bryan 1986:5). This claim has less merit. The criteria for an early site have not been met "at the level of scientifically acceptable demonstration" (Dincauze 1984:290). The assertion that following the application of those same criteria "we would be left wondering whether or not anyone occupied the eastern part of North America between 2000 and 7000 B.C." (Irving 1985:533) is specious. The claim that Clovis sites are not subject to the same scrutiny (Bryan 1989) hardly is relevant. The post-12,000 B.P. occupation in North America is a *solved* issue, a pre-12,000 B.P. occupation is not.

Conjectures and refutations are, of course, fundamental to science and to our efforts at understanding this archaeological problem. Assertions that criticism has "severely harmed the discipline" (Irving 1985:533) or "smothers scientific advance" (Bryan 1986:5) are no more acceptable than Holmes's claim, also uttered in the heat of battle, that speculations about great antiquity are "dangerous to the cause of science" (Holmes 1925:258).

Abbott once expressed the hope that "Perhaps the 'doubting Thomases' will be fewer by the year 2000" (Abbott, C. Letter to F. W. Putnam. June 20, 1888. Peabody Museum Papers, Harvard University, Cambridge, Massachusetts). That seems unlikely.

MUST ALL ROADS LEAD TO CLOVIS?

If one looks beyond conspiracy theories and mammoth myths, and more deeply into the strategies used to determine the number and timing of migrations to North America, it is evident that there are subtle assumptions in this dispute that might constrain the manner in which the problem is approached or the evidence is evaluated. It is assumed tacitly in virtually all discussions that there were a low number of "arrow-like purposeful migrations" (Morlan 1987:267), each a discrete episode involving a small coherent founding population. This finds its most explicit expression in Ruhlen's claim that at "most we can conclude that there were not more than three" migrations (Ruhlen 1987:10), and Martin's (1973:970) view that the earliest migration was a one-time event involving 100 individuals. It is further assumed that the Clovis occupation (whose descendants some identify as Amerinds) is the earliest migration (Workman 1985:17). Proponents and skeptics alike speak of a pre-12,000 B.P. occupation as a pre-Clovis one (Bryan 1986:7; Dincauze 1984:275), the traces of which should "logically lead" to Clovis (Haynes 1987:86).

In evaluating these assumptions one should begin with the observation that throughout the late Pleistocene there were no longstanding physical barriers that would thwart migration across Beringia. The region was unglaciated, the climate was not beyond the capacity to cope, and there were few obvious ecological obstacles. The fact that the late Pleistocene fauna of Siberia and Alaska is virtually identical (Harington 1980:47) makes this clear (Grayson 1988:112). Even if the Cordilleran and Laurentide ice sheets coalesced to form a barrier to the south, or even after Beringia drowned, these were still no barriers to people with watercraft traveling down the coast. Indeed, it may be that the only barriers to colonization of North America came later and these were social ones, where the presence of already established groups prevented subsequent colonization by later ones. Such *might* explain (recognizing the inherent limitations of equating social barriers with archaeological debris) the spread of microblades and microcores, which occur possibly as early as 10,000 B.P. in southeastern Alaska, and are arrayed to the south in a time-transgressive pattern, but reached their southernmost limit by 6500 B.P. in the state of Washington (Dumond 1980:990).

In the absence of Pleistocene barriers, and in light of what is known of the patterns of human migration (e.g., Keegan and Diamond 1987), one might hypothesize that the earliest migration to North America was not necessarily a single episode that brought over a small group of early Amerinds, but perhaps a "dribbling over from northeastern Asia, extending probably over a long stretch of time" (Hrdlička 1926:9). The number and timing of these migratory "dribbles" obviously would be a function of many factors, including the size and number of the source populations, reproductive rates, subsistence and settlement systems, changing competitive relationships, and changes in ecological structure (Diamond 1977:251).

There are distinct implications for a single, homogeneous migratory population as opposed to a series of early migratory "dribbles" of distinct populations. In the case of the former, in situ evolution should be sufficient to explain the physical and linguistic diversity evident among the presumed (Amerind?) descendants of that earliest founding population. In the case of the latter, it would not be, if one assumes for the sake of discussion the extreme case that these migratory "dribbles" either dispersed from physically and linguistically unrelated northeast Asian sources, or dispersed from a large, highly complex source though at widely separated intervals of time.

Genetic evidence from modern North American populations is somewhat equivocal on resolving this issue. The picture that emerges from comparing various gene distributions across those populations is one of "discordant variation"—even within major groupings such as "Amerind" (Zegura 1987:10). Genetic studies thus far cannot confirm conclusively how many major groupings there are of modern native North Americans, much less the presumed number of migrations. One might be inclined to explain this "kaleidoscope of gene frequencies" (Zegura 1987:11) as the inextricably mixed product (through mate exchange, etc.) of a multiple series of genetically distinct founding populations, instead of the product of a single founding population evolving over time. However, that conclusion would be premature, since such genetic discordancy might merely reflect limitations of the methods or the effects of admixture (particularly following population disruption at European contact).

Linguistic evidence presents a similar picture. Sapir (among others) was long ago struck by the variety of languages even within his groupings, and thus proposed not a single early migration but a "series of movements of linguistically unrelated peoples, possibly from different directions and certainly at very different times" (Sapir 1949:454–455). Greenberg (1987:42) would seem to corroborate this observation. Again, such a situation presumably would not be expected from a single, linguistically coherent founding population, but cannot be ascribed to multiple migratory "dribbles" since it might result from limits of our knowledge about the relations of languages.

At this juncture the possibility that the earliest migration involved a single migratory pulse cannot be falsified, any more than one can demonstrate that there were multiple early migrations. In either case, it is reasonable to take the argument one step further, and note that the possibility that there was one or many early *successful* migrations logically implies the possibility of early *unsuccessful* migrations (Haynes 1967:280). Unsuccessful or *failed* migrations, in strict evolutionary terms, would be those that penetrated the continent but subsequently disappeared without issue or without detectable mixture (genetic) with indigenous groups. A prime example would be the unsuccessful efforts by Norse groups one thousand years ago to move into eastern Canada (McGhee 1984).

Naturally, failed migrations would be invisible among modern populations (and thus of no special interest to those studying the origins of those populations). In *any* reconstruction of ancestral populations and migratory pulses using evidence from modern populations, one can "see" only those colonizers who made it, the ones who penetrated the continent, were reproductively viable, and whose genetic material survived the long haul of American prehistory (however evolved or otherwise incorporated through gene flow and its cultural analogues with indigenous human populations). Thus, reconstructions from modern populations 12,000 years after the fact may make it appear as though there were only three migrations, but at best these represent clusters of successful migratory "dribbles," and at worst miss earlier (or even later) migrations that disappeared without issue, with or without any interaction with populations ancestral to modern native North Americans. Ruhlen's conclusion must be restated that at "most we can conclude that there were not more than three" *successful* migrations (Ruhlen 1987:10).

Greenberg, Turner, and Zegura recognize the possibility that there may have been only three migrations that "left linguistic traces" (Greenberg et al. 1986:479), and that others may have disappeared (Greenberg 1987:333). However, they deem migratory failure highly unlikely—for three reasons. First, the suggestion that there were migrations without traces "is not a parsimonious way to explain away the Clovis-dental-time relationship" (Greenberg et al. 1986:484). Of course, the possibility there were migrations before and unrelated to Clovis says nothing about whether Clovis was ancestral to their presumed Amerind dental groups. They could well be correct in their grouping Clovis with Amerind, but that itself is independent of whether there was a non-Clovis pre-12,000 migration that failed (similarly, a demonstration that there was a pre-12,000 B.P., non-Clovis migration is *not* evidence that their grouping of Clovis and Amerind is wrong).

Second, they deem it unlikely because they see little empirical support. As Turner (1985b:9) put it: "There is not a single skeletal find studied by any physical anthropologist suggesting an earlier founding population that went extinct, to be followed by a subsequent population related to the Clovis culture-bearers." The rarity of late Pleistocene skeletal materials, and the geological antiquity of the modern sapiens form, makes this a moot point.

Finally, they note the "proven reproductive success of the Paleoindian colonizers and their dogs" (Greenberg et al. 1986:484), and wonder "what mechanism would have to be proposed to explain the reproductive failure of the hypothetical pre-Clovis people?" Natural selection is the answer. Extinction is the natural by-product of migration by founder populations that failed to respond adaptively to changing environmental conditions or were unable to reach sufficiently large size for viable reproduction (MacArthur 1972:89; Simpson 1953:293–303). Such is the regular fate of peripheral isolate populations under adverse conditions (Mayr 1970:305).

The possibility that the earliest migration was not a single episode but a multiple series, and that some of those in that multiple series may have failed, has consequences for understanding the origin and variation of modern populations. It would be useful, although beyond the scope of this paper, to pursue some of the issues in population biology and human adaptive strategies that such possibilities imply. What is important to consider here are the implications for the Clovis and pre-12,000 B.P. archaeological record.

First, it becomes useful henceforth to abandon the term pre-Clovis as it is used as a generic referent to that time period before Clovis, and use it only in the sense where a historical relation to Clovis is implied. Pre-12,000 B.P. groups should be just that, with no affinities implied. Thus, it may be quite correct that there is no pre-Clovis, but there may nonetheless have been a pre-12,000 B.P. migration and occupation. A demonstrated pre-Clovis occupation is also a pre-12,000 B.P. one, but the reverse is not true.

Second, the Clovis occupation, which has long been recognized as distinct from the contemporary northeast Asian archaeological record, may prove to be distinct even after that Asian record is better understood. If the Clovis record is a North American composite of a series of early migrations from distinct Asian groups, there is no reason to expect Clovis in Siberia. Any Old World homologs will occur only as scattered traits.

Third, if there were pre-12,000 B.P. migrations that failed, that has obvious implications for interpreting the earliest archaeological record. For one thing, the traces of those earlier migrations will be much more difficult to detect (after all, they failed to make it). It goes without saying that it will also be impossible to predict beforehand what such will look like (aside from the guess that they will likely not look like Clovis). It implies that understanding these early sites cannot be in terms of what followed them (Clovis), but rather what preceded them (in northeast Asia and Siberia)—which, of course, is known poorly. And, finally, it complements an observation recently made by Butzer (1988), that there may be spatial and temporal "gaps" in the Pleistocene archaeological record. He attributes such gaps partly to risk minimization on the part of unspecialized hunter–gatherers avoiding some areas and selectively utilizing others, and partly to our lack of attention to the proper geological settings (Butzer 1988:201–202). The possibility that the gaps represent discontinuities following a failed migration warrants consideration as an alternative hypothesis.

It might further be speculated that *if* there were a series of unrelated failed migrations, and *if* they are detected, the various assemblages will not necessarily all look alike. West (1983:368) is struck by the fact that no two purported pre-12,000 B.P. sites look alike, and uses this as grounds for dismissing the lot. This suggestion has little intrinsic merit (assemblages widely separated in time and space need not look alike), and without discussing the merits of each of those cases, the fact remains that not looking alike *may* prove to be a characteristic of a pre-12,000 B.P. record.

Finally, if there was a pre-12,000 B.P. population that reached the size where it was archaeologically detectable, but then failed, that has important implications for understanding the adaptive process of human migration. But what ultimately may have far greater evolutionary significance for North American prehistory is the fact that a group (or groups) came together to comprise Clovis, and survived.

All this raises the question of whether the idea of failed migrations is presently falsifiable. It is not. It can only be tested *after* it has been demonstrated that there were human occupations in North America prior to 12,000 B.P., and determined whether those are or are not related to Clovis. The suggestion that a pre-12,000 B.P. occupation may look different than Clovis, coupled with the observation that many of the purported pre-12,000 B.P. assemblages do look different than Clovis, is not itself confirmation there were failed migrations before 12,000 B.P. Once again, therefore, demonstrating that there were humans in America before 12,000 B.P. is strictly an archaeological issue.

CONCLUSIONS

Why don't we know when the first people came to America? It is entirely possible we already do. Perhaps it was the Clovis group, whose descendants are among those physically and linguistically linked as "Amerinds" (Greenberg et al. 1986:480). Their presence can be explained without recourse to earlier colonizers, and a post-14,000 B.P. crossing to Alaska and arrival on the Plains by 12,000 B.P. fits part of the available geological evidence. There is not now compelling evidence for a pre-

Clovis or pre-12,000 B.P. migration. If this remains unchanged, then we must face the harsh realization that however much was learned in the process, many people spent a great deal of time in what amounts to a fruitless pursuit.

Even so, just as there is no compelling evidence to accept a pre-Clovis occupation, there is no compelling reason to deny one either. There is nothing in the historic or neontological evidence that precludes pre-Clovis or pre-12,000 B.P. migrations to the Americas. The fact that many sites have been offered and none so far accepted as evidence of a pre-12,000 B.P. occupation is grounds for skepticism, but cannot lessen the likelihood that pre-12,000 B.P. sites will be found. Our archaeological forebears rejected dozens of sites before they reached Folsom. We may not yet know when the first people arrived, and will not know until the evidence is forthcoming. Although much of this debate is not solely attributable to a factual gap, one exists.

There the matter stands, surrounded by a swarm of controversial statements for and against claims of great antiquity in the Americas.

The Clovis occupation is the earliest, well-defined archaeological trace currently known in North America. Yet, we lose sight of the fact that Clovis may represent a composite of migratory "dribbles," which in turn might go some distance toward explaining not just the differences already evident between Clovis and contemporary Siberian archaeological traces, but also the variation present among their modern descendants. Similarly, by virtue of the success of the Clovis groups, we miss the possibility that others, for want of sufficient numbers or in the face of adaptive challenges beyond their abilities, simply disappeared without issue. Their traces will be very difficult to find.

To seek and understand such traces will require continued research on the natural processes whose products mimic artificial ones (Morlan 1987:293–294), in order to resolve some of the ambiguity that plagues many claims for pre-12,000 B.P. sites. There should be more concern for the processes (biological and cultural) of human migration, successful and unsuccessful, coastal and interior, and how to recognize their archaeological signature. Such would benefit by looking beyond the issue of the peopling of the Americas to migration in other areas at other times. There should be redoubled efforts to understand the environments and geological setting of all possible routes of migration. This includes a firmer handle on the geomorphic contexts of late Pleistocene and earlier landscapes (Butzer 1988:201–202). Finally, there should be a greater concern for the adaptive strategies of human foragers in nonmarginal environments, particularly environments without other people.

A better understanding of such things will not set the boundaries within which humans came to the Americas. None can give that sort of information. Rather, their value will come in an ability to predict and, possibly, discover the earliest traces of the first North Americans, and evaluate and understand that evidence if we do.

If a pre-12,000 B.P. occupation is forthcoming with evidence that meets all the requirements, I have confidence that it will be accepted, as Folsom was 60 years ago. Critics then, and critics now, realize that if there were Americans earlier than we think, their traces will sooner or later be found (Dincauze 1984:311; Holmes 1925:258). What that evidence will look like and where it will first be found is an open question. South America seems to have surpassed California as a prolific source of claims for early sites, and I await the full presentation of the results from Boqueirão da Pedra Furada (Guidon and Delibrias 1986) and Monte Verde (Dillehay and Collins 1988) for what they imply of our understanding of the process and antiquity of the earliest migration to North America.

I do not believe, as do others (Bray 1988; Diamond 1987:580), that there will have to be multiple sites to prove the case. One site will suffice. Folsom is evidence of that.

Acknowledgments. This paper was written for a symposium at the 53rd Annual Meeting of the Society for American Archaeology, April 1988, organized by Margaret W. Conkey and Carmel Schrire. John M. Beaton, Larry Gorenflo, Ann F. Ramenofsky, Dennis Stanford, and David J. Weber provided valuable advice and references. Others read the paper in manuscript form, and I would like to thank for their thoughtful comments Alan L. Bryan, Christopher Chippindale, Tom D. Dillehay, Dena F. Dincauze, Donald K. Grayson, Ruth Gruhn, C. Vance Haynes, Jesse D. Jennings, Robert D. Leonard, Jeffrey C. Long, Anthony E. Marks, Richard E. Morlan, William C. Sturtevant, Christy G. Turner, W. Raymond Wood, and an anonymous *American Antiquity* reviewer.

I am especially grateful to Bill Sturtevant and Tony Marks for each independently asking the obvious question, "Why is the early one migration not an early two migrations?" Why not, indeed.

The historical research touched on here was supported by the National Endowment for the Humanities, and the Department of Anthropology, National Museum of Natural History, Smithsonian Institution (through the good offices of Bruce Smith) and it, along with the writing of this paper, was made possible by a Research Fellowship Leave from Southern Methodist University, for which I am most grateful.

REFERENCES CITED

Adovasio, J., A. Boldurian, and R. Carlisle
1987 Who are Those Guys?: Early Human Populations in Eastern North America. Paper presented at the 58th Annual Meeting, Texas Archeological Society, Waco.
Adovasio, J., J. Gunn, J. Donahue, R. Stuckenrath. J. E. Guilday, and K. Volman
1980 Yes Virginia, It Really Is That Old: A Reply to Haynes and Mead. *American Antiquity* 45:588–595.
Alexander, H.
1978 The Legalistic Approach to Early Man Studies. In *Early Man in America,* edited by A. Bryan, pp. 20–22. Occasional Papers No. 1. Department of Anthropology, University of Alberta, Edmonton.
Alsoszatai-Petheo, J.
1986 An Alternative Paradigm for the Study of Early Man in the New World. In *New Evidence for the Pleistocene Peopling of the Americas,* edited by A. Bryan, pp. 15–23. Center for the Study of Early Man, Orono.
Antevs, E.
1935 The Spread of Aboriginal Man to North America. *The Geographical Review* 25:302–309.
Binford, L.
1981 *Bones: Ancient Man and Modern Myths.* Academic Press, New York.
Blackburn, T. (editor)
1985 *Woman, Poet, Scientist: Essays in New World Anthropology Honoring Dr. Emma Lou Davis.* Ballena Press, San Diego.
Bloom, A.
1983 Sea Level and Coastal Morphology of the United States Through the Late Wisconsin Glacial Maximum. In *Late Quaternary Environments of the United States: The Late Pleistocene,* edited by S. Porter, pp. 215–229. University of Minnesota Press, Minneapolis.
Bonnichsen, R.
1978 Critical Arguments for Pleistocene Artifacts from the Old Crow Basin. In *Early Man in America,* edited by A. Bryan, pp. 102–118. Occasional Papers No. 1. Department of Anthropology, University of Alberta, Edmonton.
Bonnichsen, R., and D. Young
1980 Early Technological Repertoires: Bone to Stone. *Canadian Journal of Anthropology* 1:123–128.
Bordes, F.
1978 Preface. In *Early Man in America,* edited by A. Bryan, pp. v–vi. Occasional Papers No. 1. Department of Anthropology, University of Alberta, Edmonton.
Bray, W.
1988 The Paleoindian Debate. *Nature* 332:107.
Brown, B.
1928 Recent Finds Relating to Prehistoric Man in America. *Bulletin of the New York Academy of Medicine* 4:824–828.
Bryan, A.
1977 Developmental Stages and Technological Traditions. In *Amerinds and Their Paleoenvironments in Northeastern North America,* edited by W. Newman and B. Salwen, pp. 355–368. Annals of the New York Academy of Sciences 288. New York.
1978 An Overview of Paleo-American Prehistory from a Circum-Pacific Perspective. In *Early Man in America,* edited by A. Bryan, pp. 306–327. Occasional Papers No. 1. Department of Anthropology, University of Alberta, Edmonton.
1986 Paleoamerican Prehistory as Seen from South America. In *New Evidence for the Pleistocene Peopling of the Americas,* edited by A. Bryan, pp. 1–14. Center for the Study of Early Man, Orono.
1989 The Relationship of the Stemmed Point and Fluted Point Traditions in the Great Basin. In *The Clovis-Archaic Interface in the Western United States,* edited by C. M. Aikens and J. Willig. Nevada State Museum Anthropological Papers, Las Vegas, in press.
Bryan, A. (editor)
1986 *New Evidence for the Pleistocene Peopling of the Americas.* Center for the Study of Early Man, Orono.
Bryan, K.
1929 Discussion of "Folsom Culture and Its Age" by B. Brown. *Geological Society of America Bulletin* 40: 128–129.
1937 Geology of the Folsom Deposits in New Mexico and Colorado. In *Early Man in America,* edited by G. G. MacCurdy, pp. 139–152. Lippincott, Philadelphia.

Butzer, K.
1988 A "Marginality" Model to Explain Major Spatial and Temporal Gaps in the Old and New World Pleistocene Settlement Records. *Geoarchaeology* 3:193–203.
Carlisle, R. (editor)
1988 *Americans before Columbus: Ice Age Origins.* Ethnology Monographs 12. Department of Anthropology, University of Pittsburgh, Pittsburgh.
Carter, G.
1978 The American Paleolithic. In *Early Man in America,* edited by A. Bryan, pp. 10–19. Occasional Papers No. 1. Department of Anthropology, University of Alberta, Edmonton.
Chamberlin, T. C.
1903 The Criteria Requisite for the Reference of Relics to a Glacial Age. *Journal of Geology* 11:64–85.
Chamberlin, T. C., and R. D. Salisbury
1906 *Geology: Earth History.* Henry Holt, New York.
Colbert, E. H.
1942 The Association of Man with Extinct Mammals in the Western Hemisphere. *Proceedings of the Eighth American Scientific Congress* II:17–29. Washington, D.C.
Cook, H. J.
1928 Glacial Age Man in New Mexico. *Scientific American* 139:38–40.
Diamond, J.
1977 Colonization Cycles in Man and Beast. *World Archaeology* 8:249–261.
1987 Who Were the First Americans? *Nature* 329:580–581.
Dillehay, T.
1985 A Regional Perspective of Preceramic Times in the Central Andes. *Reviews in Anthropology* 12:193–205.
1988 How New is the New World? *Antiquity* 62:94–97.
Dillehay, T., and M. Collins
1988 Early Cultural Evidence from Monte Verde in Chile. *Nature* 332:150–152.
Dincauze, D.
1984 An Archaeo-logical Evaluation of the Case for Pre-Clovis Occupations. In *Advances in World Archaeology,* vol. 3, edited by F. Wendorf and A. Close, pp. 275–323. Academic Press, New York.
Doran, G., D. Dickel, W. Ballinger, O. Agee, P. Laipis, and W. Hauswirth
1986 Anatomical, Cellular and Molecular Analysis of 8,000 Yr. Old Human Brain Tissue from the Windover Archaeological Site. *Nature* 323:803–806.
Dumond, D.
1980 The Archaeology of Alaska and the Peopling of America. *Science* 209:984–991.
1987 A Reexamination of Eskimo-Aleut Prehistory. *American Anthropologist* 89:32–56.
Ericson, J., R. Taylor, and R. Berger (editors)
1982 *Peopling of the New World.* Ballena Press, San Diego.
Fladmark, K.
1979 Routes: Alternate Migration Corridors for Early Man in North America. *American Antiquity* 44:55–69.
1983 Times and Places: Environmental Correlates of Mid-to-Late Wisconsinan Human Population Expansion in North America. In *Early Man in the New World,* edited by R. Shutler, pp. 13–42. Sage Publications, Beverly Hills.
1986 Getting One's Berings. *Natural History* 95(11):8–19.
Fox, J. A.
1986 Comment on "The Settlement of the Americas: A Comparison of the Linguistic, Dental, and Genetic Evidence," by J. H. Greenberg, C. G. Turner II, and S. L. Zegura. *Current Anthropology* 27:488–489.
Graham, J. A., and R. F. Heizer
1967 Man's Antiquity in North America: Views and Facts. *Quaternaria* 9:225–235.
Grayson, D. K.
1983 *The Establishment of Human Antiquity.* Academic Press, New York.
1988 Perspectives on the Archaeology of the First Americans. In *Americans Before Columbus: Ice Age Origins,* edited by R. Carlisle, pp. 107–123. Ethnology Monographs 12. Department of Anthropology, University of Pittsburgh, Pittsburgh.
Greenberg, J. H.
1987 *Language in the Americas.* Stanford University Press, Stanford.
Greenberg, J. H., C. G. Turner, II, and S. L. Zegura
1986 The Settlement of the Americas: A Comparison of the Linguistic, Dental, and Genetic Evidence. *Current Anthropology* 27:477–497.
Griffin, J.
1976 Comments on the Quest for Early Man in North America. In *Habitats humains anterieurs a l'Holocene en Amerique,* edited by J. Griffin, pp. 7–17. International Union of Prehistoric and Protohistoric Sciences, Nice.
1977 A Commentary on Early Man Studies in the Northeast. In *Amerinds and Their Paleoenvironments in*

Northeastern North America, edited by W. Newman and B. Salwen, pp. 3–15. Annals of the New York Academy of Sciences 288. New York.

1979 The Origin and Dispersion of American Indians in North America. In *The First Americans: Origins, Affinities and Adaptations,* edited by W. Laughlin and A. Harper, pp. 43–55. Gustav Fischer, New York.

Gruhn, R.

1988 Linguistic Evidence in Support of the Coastal Route of Earliest Entry into the New World. *Man* 23: 77–100.

Guidon, N., and G. Delibrias

1986 Carbon-14 Dates Point to Man in the Americas 32,000 Years Ago. *Nature* 321:769–771.

Harington, C. R.

1980 Faunal Exchanges Between Siberia and North America: Evidence from Quaternary Land Mammal Remains in Siberia, Alaska and the Yukon Territory. *Canadian Journal of Anthropology* 1:45–49.

Harper, A., and W. Laughlin

1982 Inquiries into the Peopling of the New World: Development of Ideas and Recent Advances. In *A History of American Physical Anthropology,* edited by F. Spencer, pp. 281–304. Academic Press, New York.

Hassan, F.

1981 *Demographic Archaeology.* Academic Press, New York.

Haynes, C. V.

1967 Carbon-14 Dates and Early Man in the New World. In *Pleistocene Extinctions,* edited by P. S. Martin and H. E. Wright, pp. 267–286. Yale University Press, New Haven.

1969 The Earliest Americans. *Science* 166:709–715.

1982 Were Clovis Progenitors in Beringia? In *Paleoecology of Beringia,* edited by D. Hopkins, J. Matthews, C. Schweger, and S. Young, pp. 383–398. Academic Press, New York.

1987 Clovis Origins Update. *The Kiva* 52:83–93.

Holmes, W. H.

1893 Traces of Glacial Man in Ohio. *Journal of Geology* 1:147–163.

1919 *Handbook of Aboriginal American Antiquities,* pt. 1. Bulletin 60. Bureau of American Ethnology, Smithsonian Institution, Government Printing Office, Washington, D.C.

1925 The Antiquity Phantom in American Archaeology. *Science* 62:256–258.

Hopkins, D.

1982 Aspects of the Paleogeography of Beringia During the Late Pleistocene. In *Paleoecology of Beringia,* edited by D. Hopkins, J. Matthews, C. Schweger, and S. Young, pp. 3–28. Academic Press, New York.

Hrdlička, A.

1907 *Skeletal Remains Suggesting or Attributed to Early Man in North America.* Bulletin 33. Bureau of American Ethnology, Smithsonian Institution, Government Printing Office, Washington, D.C.

1912 *Early Man in South America.* Bulletin 52. Bureau of American Ethnology, Smithsonian Institution, Government Printing Office, Washington, D.C.

1917 Preliminary Report on Finds of Supposedly Ancient Human Remains at Vero, Florida. *Journal of Geology* 25:43–51.

1918 *Recent Discoveries Attributed to Early Man in America.* Bulletin 66. Bureau of American Ethnology, Smithsonian Institution, Government Printing Office, Washington, D.C.

1925 The Origin and Antiquity of the American Indian. *Smithsonian Institution Annual Report for 1923,* pp. 481–494.

1926 The Race and Antiquity of the American Indian. *Scientific American* 135:7–9.

1928 The Origin and Antiquity of Man in America. *Bulletin of the New York Academy of Medicine* 4:802–820.

1942 The Problem of Man's Antiquity in America. *Proceedings of the Eighth American Scientific Congress* II:53–55. Washington, D.C.

Irving, W. N.

1985 Context and Chronology of Early Man in the Americas. In *Annual Review of Anthropology,* vol. 14, edited by B. J. Siegel, A. R. Beals, and S. A. Tyler, pp. 529–555. Annual Reviews, Palo Alto.

1987 New Dates from Old Bones. *Natural History* 96(2):8–13.

Irving, W. N., A. Jopling, and B. Beebe

1986 Indications of Pre-Sangamon Humans near Old Crow, Yukon, Canada. In *New Evidence for the Pleistocene Peopling of the Americas,* edited by A. Bryan, pp. 49–63. Center for the Study of Early Man, Orono.

Jefferson, T.

1787 *Notes on the State of Virginia.* John Stockdale, London.

Johnston, W. A.

1933 Quaternary Geology of North America in Relation to the Migration of Man. In *The American Aborigines: Their Origin and Antiquity,* edited by D. Jenness, pp. 9–45. University of Toronto Press, Toronto.

Judd, N.

1929 The Present Status of Archaeology in the United States. *American Anthropologist* 31:401–418.

Keegan, W. F., and J. Diamond
1987 Colonization of Islands by Humans: A Biogeographical Perspective. In *Advances in Archaeological Method and Theory,* vol. 10, edited by M. B. Schiffer, pp. 49–92. Academic Press, New York.
Kidder, A. V.
1927 Early Man in America. *The Masterkey* 1(5):5–13.
Kirk, R., and E. Szathmary (editors)
1985 *Out of Asia: Peopling the Americas and the Pacific.* The Journal of Pacific History, Canberra.
Krieger, A.
1964 Early Man in the New World. In *Prehistoric Man in the New World,* edited by J. Jennings and E. Norbeck, pp. 23–81. University of Chicago Press, Chicago.
Laughlin, W. S.
1986 Comment on "The Settlement of the Americas: A Comparison of the Linguistic, Dental, and Genetic Evidence," by J. H. Greenberg, C. G. Turner II, and S. L. Zegura. *Current Anthropology* 27:489–490.
Lorenzo, J.
1978 Early Man Research in the American Hemisphere: Appraisal and Perspectives. In *Early Man in America,* edited by A. Bryan, pp. 1–9. Occasional Papers No. 1. Department of Anthropology, University of Alberta, Edmonton.
Lorenzo, J., and L. Mirambell
1986 Preliminary Report on Archaeological and Paleoenvironmental Studies in the Area of El Cedral, San Luis Potosi, Mexico, 1977–1980. In *New Evidence for the Pleistocene Peopling of the Americas,* edited by A. Bryan, pp. 107–113. Center for the Study of Early Man, Orono.
MacArthur, R.
1972 *Geographical Ecology: Patterns in the Distribution of Species.* Harper and Row, New York.
MacDonald, G.
1987 Postglacial Vegetation History of the Mackenzie River Basin. *Quaternary Research* 28:245–262.
McGee, WJ
1891 Some Principles of Evidence Relating to Early Man. *American Antiquarian* 13:69–79.
McGhee, R.
1984 Contact Between Native North Americans and the Medieval Norse: A Review of the Evidence. *American Antiquity* 49:4–26.
McManus, D., and J. Creager
1984 Sea-level Data for Parts of the Bering-Chukchi Shelves of Beringia from 19,000 to 10,000 ^{14}C Yr. B.P. *Quaternary Research* 21:317–325.
MacNeish, R.
1976 Early Man in the New World. *American Scientist* 63:316–327.
Martin, P.
1958 Pleistocene Ecology and Biogeography of North America. In *Zoogeography,* edited by C. Hubbs, pp. 375–420. Publication 51. American Association for the Advancement of Science, Washington, D.C.
1967 Prehistoric Overkill. In *Pleistocene Extinctions: The Search for a Cause,* edited by P. S. Martin and H. E. Wright, pp. 75–120. Yale University Press, New Haven.
1973 The Discovery of America. *Science* 179:969–974.
1987 Clovisa the Beautiful. *Natural History* 96(10):10–13.
Mayr, E.
1970 *Population, Species and Evolution.* Belknap Press, Cambridge.
Meltzer, D.
1983 The Antiquity of Man and the Development of American Archaeology. In *Advances in Archaeological Method and Theory,* vol. 6, edited by M. B. Schiffer, pp. 1–51. Academic Press, New York.
1988 Late Pleistocene Human Adaptations in Eastern North America. *Journal of World Prehistory* 2:1–52.
Meltzer, D., and J. I. Mead
1985 Dating Late Pleistocene Extinctions: Theoretical Issues, Analytical Bias and Substantive Results. In *Environments and Extinctions: Man in Late Glacial North America,* edited by J. I. Mead and D. J. Meltzer, pp. 145–173. Center for the Study of Early Man, Orono.
Meltzer, D., and B. Smith
1986 Paleo-Indian and Early Archaic Subsistence Strategies in Eastern North America. In *Foraging, Collecting and Harvesting: Archaic Period Subsistence and Settlement in the Eastern Woodlands,* edited by S. Neusius, pp. 1–30. Center for Archaeological Investigations, Southern Illinois University, Carbondale.
Morlan, R.
1986 Pleistocene Archaeology in the Old Crow Basin: A Critical Reappraisal. In *New Evidence for the Pleistocene Peopling of the Americas,* edited by A. Bryan, pp. 27–48. Center for the Study of Early Man, Orono.
1987 The Pleistocene Archaeology of Beringia. In *The Evolution of Human Hunting,* edited by M. Nitecki and D. Nitecki, pp. 267–307. Plenum, New York.
Nelson, D. E., R. Morlan, J. Vogel, J. Southon, and C. R. Harington
1986 New Dates on Northern Yukon Artifacts: Holocene not Upper Pleistocene. *Science* 232:749–751.

Nelson, N. C.
1918 Review of "Additional Studies in the Pleistocene at Vero, Florida" edited by E. H. Sellards. *Science* 47:394–395.
1928 Discussion of "The Origin and Antiquity of Man in America" by A. Hrdlička. *Bulletin of the New York Academy of Medicine* 4:820–823.
1933 The Antiquity of Man in America in the Light of Archaeology. In *The American Aborigines: Their Origin and Antiquity,* edited by D. Jenness, pp. 87–130. University of Toronto Press, Toronto.
Owen, R.
1984 The Americas: The Case Against an Ice-Age Human Population. In *The Origins of Modern Humans: A World Survey of the Fossil Evidence,* edited by F. Smith and F. Spencer, pp. 517–563. R. Liss, New York.
Porter, S. C.
1988 Landscapes of the Last Ice Age in North America. In *Americans Before Columbus: Ice Age Origins,* edited by R. Carlisle, pp. 1–24. Ethnology Monographs 12. Department of Anthropology, University of Pittsburgh, Pittsburgh.
Ramenofsky, A.
1987 *Vectors of Death: The Archaeology of European Contact.* University of New Mexico Press, Albuquerque.
Roberts, F.
1940 Developments in the Problem of the North American Paleo-indian. *Smithsonian Miscellaneous Collections* 100:51–116.
Rogers, R.
1985a Glacial Geography and Native North American Languages. *Quaternary Research* 23:130–137.
1985b Wisconsinan Glaciation and the Dispersal of Native Ethnic Groups in North America. In *Woman, Poet, Scientist: Essays in New World Anthropology Honoring Dr. Emma Lou Davis,* edited by T. Blackburn, pp. 105–113. Ballena Press, San Diego.
Rogers, R., and L. Martin
1987 The Folsom Discovery and the Concept of Breakthrough Sites in Paleoindian Studies. *Current Research in the Pleistocene* 4:81–82.
Romer, A. S.
1933 Pleistocene Vertebrates and Their Bearing on the Problem of Human Antiquity in America. In *The American Aborigines: Their Origin and Antiquity,* edited by D. Jenness, pp. 49–83. University of Toronto Press, Toronto.
Ruhlen, M.
1987 Voices from the Past. *Natural History* 96(3):6–10.
Sapir, E.
1949 Time Perspective in Aboriginal American Culture: A Study in Method. In *Selected Writings of Edward Sapir in Language, Culture and Personality,* edited by D. Mandlebaum, pp. 389–462. University of California Press, Berkeley. Originally published 1916, Department of Mines, Geological Survey, Ottawa, Canada.
Schweger, C., J. Matthews, D. Hopkins, and S. Young
1982 Paleoecology of Beringia—a Synthesis. In *Paleoecology of Beringia,* edited by D. Hopkins, J. Matthews, C. Schweger, and S. Young, pp. 425–444. Academic Press, New York.
Sellards, E. H.
1916 Human Remains and Associated Fossils from the Pleistocene of Florida. *Eighth Annual Report of the Florida State Geological Survey,* pp. 121–160. Tallahassee.
Shutler, R. (editor)
1983 *Early Man in the New World.* Sage, Beverly Hills.
Simpson, G. G.
1953 *The Major Features of Evolution.* Columbia University Press, New York.
Simpson, R. D., L. Patterson, and C. Singer
1986 Lithic Technology of the Calico Mountains Site, Southern California. In *New Evidence for the Pleistocene Peopling of the Americas,* edited by A. Bryan, pp. 89–105. Center for the Study of Early Man, Orono.
Stanford, D.
1983 Pre-Clovis Occupation South of the Ice Sheets. In *Early Man in the New World,* edited by R. Shutler, pp. 65–72. Sage Publications, Beverly Hills.
Suarez, B. K., J. Crouse, and D. O'Rourke
1985 Genetic Variation in North Amerindian Populations: The Geography of Gene Frequencies. *American Journal of Physical Anthropology* 67:217–232.
Szathmary, E. J. E.
1985 Peopling of North America: Clues from Genetic Studies. In *Out of Asia: Peopling the Americas and the Pacific,* edited by R. Kirk and E. Szathmary, pp. 79–104. The Journal of Pacific History, Canberra.
1986 Comment on "The Settlement of the Americas: A Comparison of the Linguistic, Dental, and Genetic Evidence," by J. H. Greenberg, C. G. Turner II, and S. L. Zegura. *Current Anthropology* 27:490–491.
Taylor, R. E., and L. Payen
1979 The Role of Archeometry in American Archaeology: Approaches to the Evaluation of the Antiquity

of *Homo sapiens* in California. In *Advances in Archaeological Method and Theory,* vol. 2, edited by M. B. Schiffer, pp. 239–283. Academic Press, New York.

Taylor, R., L. Payen, C. Prior, P. Slota, R. Gillespie, J. Gowlett, R. Hedges, A. Jull, T. Zabel, D. Donahue, and R. Berger

1985 Major Revisions in the Pleistocene Age Assignments for North American Human Skeletons by C-14 Accelerator Mass Spectrometry: None Older Than 11,000 C-14 Years B.P. *American Antiquity* 50:136–140.

Toth, N., and K. D. Schick

1986 The First Million Years: The Archaeology of Protohuman Culture. In *Advances in Archaeological Method and Theory,* vol. 9, edited by M. B. Schiffer, pp. 1–96. Academic Press, New York.

Turner, C.

1983 Dental Evidence for the Peopling of the Americas. In *Early Man in the New World,* edited by R. Shutler, pp. 147–157. Sage, Beverly Hills.

1985a The Dental Search for Native American Origins. In *Out of Asia: Peopling the Americas and the Pacific,* edited by R. Kirk and E. Szathmary, pp. 31–78. The Journal of Pacific History, Canberra.

1985b The Modern Human Dispersal Event: The Eastern Frontier. *The Quarterly Review of Archaeology* 6: 8–9, 13.

1986 The First Americans: The Dental Evidence. *National Geographic Research* 2:37–46.

1987 Telltale Teeth. *Natural History* 96(1):6–10.

Waters, M.

1985 Early Man in the New World: An Evaluation of the Radiocarbon Dated Pre-Clovis Sites in the Americas. In *Environments and Extinctions,* edited by J. Mead and D. J. Meltzer, pp. 125–143. Center for the Study of Early Man, Orono.

Wendorf, F.

1966 Early Man in the New World: Problems of Migration. *The American Naturalist* 100:253–270.

West, F. H.

1983 The Antiquity of Man in America. In *Late Quaternary Environments of the United States: The Late Pleistocene,* edited by S. Porter, pp. 364–382. University of Minnesota Press, Minneapolis.

White, J., R. Mathewes, and W. Mathews

1985 Late Pleistocene Chronology and Environment of the "Ice-Free Corridor" of Northwestern Alberta. *Quaternary Research* 24:173–186.

Williams, R., A. Steinberg, H. Gershowitz, P. Bennett, W. Knowler, D. Pettitt, W. Butler, R. Baird, L. Dowda-Rea, T. Burch, H. Morse, and C. Smith

1985 GM Allotypes in Native Americans: Evidence for Three Distinct Migrations Across the Bering Land Bridge. *American Journal of Physical Anthropology* 66:1–19.

Wilson, M.

1984 Stalker Site (Taber Child Site) Investigations, Alberta. *Current Research in the Pleistocene* 1:27–29.

Wissler, C.

1916 The Present Status of the Antiquity of Man in North America. *The Scientific Monthly* 2:234–238.

Workman, W.

1985 Comment on "The "Dyuktai Culture" and New World Origins" by S. Yi and G. Clark. *Current Anthropology* 26:17.

Yi, S. and G. Clark

1985 The "Dyuktai Culture" and New World Origins. *Current Anthropology* 26:1–20.

Young, D.

1986 The Paleoindian Skeletal Material from Horn Shelter, Number 2, in Central Texas—An Analysis and Perspective. Unpublished Master's thesis, Department of Anthropology, Texas A & M University, College Station.

Zegura, S.

1985 The Initial Peopling of the Americas: An Overview. In *Out of Asia: Peopling the Americas and the Pacific,* edited by R. Kirk and E. Szathmary, pp. 1–18. The Journal of Pacific History, Canberra.

1987 Blood Test. *Natural History* 96(7):8–11.

Received May 17, 1988; accepted September 10, 1988