

REVIEW ESSAY

THE HUMAN CROSSING OF BERINGIA: GUN FIGHT AT THE OLD CLOVIS CORRAL

Christy G. Turner II

School of Human Origins and Social Change, Arizona State University, Tempe, AZ 85287-2402. chrstygturner@aol.com

THE EARLY SETTLEMENT OF NORTH AMERICA: THE CLOVIS ERA. BY GARY HAYNES. UNIVERSITY OF CAMBRIDGE PRESS, CAMBRIDGE, 2002 . 360 PP., 10 LINE DIAGRAMS, 32 HALF TONES, 25 TABLES, 6 GRAPHS, 14 MAPS. \$80.00 HARDBACK, \$29.00 PAPERBACK. ISBN 0521524636.

Gary Haynes' two page preface summarizes the entire book: (1) There is virtually no evidence of pre-Clovis. (2) Clovis is found nearly everywhere in North America. (3) It (meaning the morphology of the spear points) is everywhere very similar, implying close social ties or rapid dispersal from Alaska. (4) There were Clovis adaptations to the large changes in the late Pleistocene environment. (5) The last of the mammoths and mastodonts and the first appearance of people are temporally related at 11,000 radiocarbon (rcybp) years before the present (13,000 calendar years B.P.). (6) From an extensive review of the ideas derived from fluted point sites, and new information about mammoth and mastodont bone assemblages, a new ecological perspective for Clovis and the megamammals is proposed. (7) Clovis is unique. And last, (8) the only way that a unified theory of the Clovis phenomenon can be accomplished is with a multidisciplinary approach. All of these points are elements in Haynes' primary goal of demonstrating that the movement of the Clovis explorers was "not by slow diffusion but by rapid dispersal" (p. xii). This issue heads the list of causes for gun play in the American Paleo-Indian bar room quarrel over who and how Siberians got here first. In a western America land usage-sense, were sheep herders (pre-Clovis) here before the big cattle outfits? On the basis of what Haynes itemizes in the preface, he intentionally or subconsciously signals his preference even in the one and one half pages of acknowledgments, because no devout or even partial believer in a pre-Clovis occupation of the New World that I know of was acknowledged except Dennis Stanford. Moreover, limiting his analysis to North America must mean that he dismisses the numerous claims for a pre-Clovis presence in South America earlier than that known for North America, because the early colonists had to have occupied North America before arriving in South America. Either that, or he may intend to track down the black hats in their own hideouts in Brazil, Chile, and

elsewhere south of the border. I will review these issues shortly, but first a few words about why the reader should not immediately skip over this essay because of my limited experience in Paleo-Indian archaeology. I have been tagged as only "a partial exception" by our distinguished Editor-in-Chief, Fred West (1987:12), when remarking on the lack of an archaeologist in the multidisciplinary Greenberg, Turner and Zegura (1986) article on the linguistic, dental, genetic, and archaeological evidence for the peopling of the New World. In friendly spirit I take Editor West's remark to mean that I was then not fully qualified to discuss the archaeological evidence. In anticipation of such a remark again or others less friendly, a little background might be useful.

I began this review in Siberia, at the home of my co-PI, Olga Pavlova, located in the magnificent pine- and birch-forested outskirts of Academgorodok. This huge "Science City" was laid out next to the Mississippi-sized Ob River that flows through nearby Novosibirsk on its lengthy meandering northward way from the Altai Mountains on the Mongolian border across the vast West Siberian Plain discharging finally into the Arctic Ocean. The Ob could have been one of several possible river basin routes used by the ancestral epi-Clovis folk (a term I coin to mean the near-ancestral people and culture of Clovis) in their travels to Beringia. In Academgorodok, a great deal of late Pleistocene Siberian archaeology and vertebrate paleontology has been conducted by staff of the Institute of Archaeology and Ethnology, initially under the Directorship of Academician A. P. Okladnikov, now directed by Academician Anatoly P. Derevyanko. This research has direct bearing on the first people that eventually reached eastern Beringia, hence, an ideal setting in which to review Gary Haynes' book. I brought his book with me, certain that it will be helpful as my Russian co-PIs (Pavlova and Nicolai D. Ovodov) and I continue our multi-year perimortem taphonomy and Paleo-Indian ori-

gins research, as well as providing a fun summer read (a quick glance showed that Haynes has a witty Mark Twainian style of satirical skepticism, an LAPD Joe Friday manner of wanting “only the facts, ma’am,” and a strong dislike of “possibilism,” a term applied to pre-Clovis beliefs that I first heard from Haynes’ fellow University of Nevada-Reno academic, G. Richard Scott).

What is most important to our Siberian work is what Haynes has to say about late Pleistocene perimortem taphonomy in North America. Given his extensive African elephant taphonomic research on this subject, his thinking about the evidence for animal butchering by Clovis hunters, and claims for pre-Clovis butchering and processing marks in the absence of clear-cut cultural remains, is something that can be assessed here in Siberia, where there are dozens of well-excavated, stratified, artifact- and fauna-rich, and well-dated sites that antedate Clovis. If the American pre-Clovis evidence does not match the well-established Siberian late Pleistocene archaeology, then something is either amiss on the American side, or the vastness of Siberia has yet to be sufficiently sampled by survey and excavation despite artifact discoveries going back into the late 1800s at locations such as the Hospital site in Irkutsk and Afontova Gora in Krasnoyarsk. We are also interested in the criteria for features and contexts that Haynes would require in order to infer that Clovis or earlier hunters were responsible for such-and-such kind bone damage rather than some other taphonomic mechanism or agency. In other words, is there a minimal late Pleistocene perimortem bone damage signature that most zoo-archaeologists and anthropological-taphonomists would agree was done by humans and only humans in the absence of unquestionable artifacts? In our Siberian research we are trying to define damage signatures that usually distinguish between what late Pleistocene humans and carnivores did to bones.

Since there is a long period of human occupation in southern Siberia that precedes Clovis, we are curious to know what Haynes thinks pre-Clovis might possibly have looked like in order to see if we can identify with the help of Russian archaeologists a homologous assemblage here in Siberia. If there are no correspondences (perhaps due to the non-existence of pre-Clovis), then our project still has the problem of explaining why the Americas were settled nearly last in the worldwide rapid dispersal of modern humans (I am unconvinced that cold alone was responsible, especially as more and more late Pleistocene Siberian sites are discovered and described. Because of this viewpoint our group is interested in learning what role large carnivores, especially hyenas, might have played in delaying an earlier drift north to eastern Beringia).

Because there is considerable artifact variation in southern Siberia, we are interested in learning if Clovis is one or a set of closely related cultural or ethnic groups. While fluted points similar to Clovis have been found in Siberia (for example, at Uptar), so far none is as old as Clovis. However, there are some artifact assemblages with some stone tool elements that might correspond to one or another regional Clovis assemblage.

Despite the comment in the opening paragraph, one still has to wonder what Haynes thinks about inter-site variation in Clovis points relative to the population structure and genetics of the Clovis explorers. Are the points sufficiently similar to infer that they represent the products of a single band or set of closely-related bands of Paleo-Siberians who first entered and spread all over the New World? This is especially important since, as Haynes notes, there are almost no human skeletal remains in the New World that are as old as Clovis. Hence, the biology of the Clovis people has to be inferred from Archaic and more recent skeletal remains, or worse, inferred from living Indian populations that are usually admixed, whose historic ancestors have likely been affected by European disease selection, European and African admixture and by population and genetic bottlenecks (drift), and are separated from the Clovis people by thousands of years of potential prehistoric microevolution.

Can looking from a Siberian vantage point at Haynes’ inferences about the early peopling of North America, lend support or lead to disagreement? This question might better be asked, and the answer accepted with less doubt, by *real* archaeologists such as Robert Ackerman, Ted Goebel, John Hoffecker, Maureen King, John Olson, (the late) Roger Powers, David Yesner, and other New World archaeologists who have or are working in northern Eurasia, and are well versed in Russian and Mongolian prehistory. However, we have some expertise that is usefully unique arising from: (1) the six year study by Ovodov, Pavlova and me of never-before-described perimortem bone taphonomy of many Siberian late Pleistocene archaeologically- and paleontologically-derived faunal assemblages as well as our visits to several of these archaeological and paleontological sites; (2) our examinations of many of the recovered artifacts; (3) Pavlova’s professional translations of scientific references practically unobtainable outside of Russia; and (4) the exchange of information with several of the Russian Quaternary and Arctic researchers going back to my first visit to Siberia in 1979.

But, despite the above, and many years of dental anthropological research on the peopling of the Ameri-

cas, I actually know very little about the basic data and interpretations of the Clovis and epi-Clovis phenomena. I stand to learn a great deal of factual, methodological, and theoretical information from this book, as will many professional and avocational archaeologists, and laypersons (including Native Americans) interested in this fascinating subject. Just how ignorant am I about Clovis?

One measure of my ignorance about Clovis archaeology is the very small number of references in Haynes' huge bibliography that I have read (only 77 of his 1,064 citations, or 7.2 percent), despite having just published a review article myself on the colonization of the New World (Turner 2002). My ignorance level goes down a little bit when I match my bibliography of the just mentioned article with Haynes' to see how we correspond. There was a 10.7 percent (14 out of 131) matching in my bibliography. If I remove the dental anthropology references in my bibliography, then the match is a little better (17.9 percent; 14 out of 78). If Haynes' non-archaeological proboscidean references are removed for this bibliographic comparison, my ignorance is lessened a trifle bit more. Still, by this literature conversancy measure I am not the best qualified reviewer of the many who will produce critical reviews that reflect agreement or disagreement with Haynes' "shoot-em-up" conclusions. The disappointing personal quality of these citation comparisons is that Haynes' did not use any dental anthropology as a line of evidence for the peopling of the New World, citing Greenberg et al. (1986) only for its linguistic content. On the other hand, I cited not a single proboscidean reference, yet we both are attempting multiple database syntheses of the human colonization of the Western Hemisphere. Hence, in theory at least, there is no reason to expect full agreement on the primary issues of New World colonization. In my view, it is exactly this uneven playing field that lies at or near to the heart of the many controversies surrounding the study of the peopling of the New World. Said another way, if all I read are papers in genetics, and paid no attention to the archaeological literature, then I am not constrained in what I infer about colonization timing, numbers of migrations, cultural inventory, numbers of migrants, etc., and can say whatever I want—something that actually characterizes a lot of genetical physical anthropology (for example see the announcement by geneticist Crawford [1998] for a 30,000 to 40,000 BP colonization of the New World, and Fiedel's [2001] reaction to this sort of anserous conjecture).

Naturally, I bring to this review some pre-conceived notions, which I feel in all fairness should be identified, especially so for readers unfamiliar with what I have assumed and inferred about the peopling of the Americas.

These are: (1) I believe that dating and chronological sequences are best done by direct diachronic archaeological measurements. While other synchronic approaches (mutation rates, glottochronology, dento-chronology, etc.) may offer interesting dates, possibly even correct ones occasionally, ultimately they have to be validated and calibrated by a direct diachronic record of some sort. I have held this view for 20 years (Turner 1985). It seems to me that Haynes also favors this supposition. (2) I have personally visited only three mammoth sites, all in Arizona. These are Naco, when it was being excavated by Emil W. Haury and associates in the mid-1950s; and Lehner Ranch and Murray Springs, both visited with guidance from C. Vance Haynes, and accompanied by Marie H. Wormington, Gai Pei (Beijing), and the students in Haynes' and my graduate seminars. These sites are not far from the old mining town of Tombstone, famous for its gun fight at the OK Corral, hence the title of this essay. All three impressed me immensely, so there is no doubt in my mind that Clovis hunters were as deadly and patient at finding, tracking, stalking, chasing down, trapping, and killing prey as any pack of wolves or hyenas. Despite their size, intelligence, skin thickness, and terrifying strength, giant mammoths and mastodons were no match for these Upper Paleolithic Americans with their lethal hunting skills honed for tens of thousands of years in Siberia. Nevertheless, I have long felt that Clovis people ignored nothing edible on the landscape. After all, they surely preserved lessons learned by their ancestors' struggles for survival in resource patchy Beringia (Laukhin 1993), including the need for cannibalism in extreme situations. Mammoth was not the only item on their menu as is made clear by the late Pleistocene Alaskan interior sites, particularly Broken Mammoth (Yesner 2000, 2001, and elsewhere). The only forms of life that we do not eat (in principle) are those species prohibited by belief systems or species with high toxicity. Like sea gulls and magpies, humans everywhere at one time or another eat everything including ourselves (Atkins 1984; Napton and Heizer 1970; Turner and Turner 1999; White 1992). To drive this point home, one needs only to recall *pica* consumption including dirt, and other non-nutritive substances. A recent account (Donn 2004) of a mentally-ill Frenchman who was hospitalized for stomach pains caused by his having swallowed some 350 coins over a period of ten years is only one in a long list of unusual consumptive predilections. Nevertheless, Haynes places his bets on more specialized foraging, and only grudgingly allows that his Clovis specialists might have occasionally stooped to fry a lizard or two for dinner after a fruitless day of megagame hunting. (3) I hold the view that the Clovis phenomenon presents the best evidence for the earliest migration to the New World, not only because of its abun-

dance, but also because there is identifiable cultural continuity from Clovis up to the present day in both North and South America. There is no evidence that I am aware of for continuity from pre-Clovis to Clovis except possibly in Alaska (Nenana to Clovis; Hoffecker, Powers, and Goebel 1993). But, here my ignorance about things Clovis and epi-Clovis may be showing. Nevertheless, my own studies have repeatedly shown that there are very few dental morphology differences between prehistoric North and South American Indians that cannot be explained by genetic drift, so only one Paleo-Indian migration needs be hypothesized, at least on dental grounds. No disagreement here, although Haynes is a bit more cautious (political?) in allowing for the very remote chance of a pre-Clovis occupation. After all, he has to interact more frequently with the pre-Clovis gun-slingers than I ever will. I can always take shelter in my dental refugium. (4) The claims for a pre-Clovis migration based on the morphology of archaeologically-derived and/or historic New World crania I find unconvincing because there is known *in situ* and rapid secular changes in head shape as well as body size in many parts of the world, including in the Aleutians (Laughlin 1980) where the change in prehistoric Aleut head shape is rapid and unassociated with any archaeologically, linguistically, or ethnographically identifiable migration.

Moreover, much of what is considered as cranial evidence for pre-Clovis is largely the expression of robusticity. Robusticity decreases with developments in food-processing techniques and food types, as well illustrated with the changes in both head shape and ruggedness in the Basketmaker to Pueblo continuum of the American Southwest (Seltzer 1944). Cooking pots greatly reduced the need for heavy mastication, so heavy cranial bone growth becomes unnecessary. Besides, who knows how regional dietary specialization affected post-natal childhood skeletal growth over the past 10,000 or more years? Haynes doesn't get involved with this particular gunfight but later in the book he does stand tall and ready to draw in support of the importance of trace nutrients, i.e., iodine, for the proper growth and development of megamammals (The hypothesis of late Pleistocene Siberian "mineral oases" [*beast solonetz*] has been well developed by S. V. Leshchinskiy [2001]). (5) There is well definable dental variation in northwestern North America that I have long held is most parsimoniously interpreted as the result of migration episodes from Siberia shortly after or at the same time but with some manner of group separation, when the ancestors of Clovis reached eastern Beringia as well represented by the interior Alaskan sites like Dry Creek, Broken Mammoth, Swan Point, the Mesa site, and others. Haynes has nothing to say on this

matter of migration number except to more-or-less ignore the three migration idea and devote several hundred uncritical words to opposing views of a few linguists and geneticists. Finally (6), three nearly tragic boating events in only modestly stormy seas during my years of fieldwork in the Aleutians (once with Allen P. McCartney, twice with Gerald A. Bair) left me with a terrifyingly strong impression of just how dangerous the north Pacific Ocean and Bering Sea actually can be. Those archaeologists who advocate late Pleistocene pre-Clovis boating from Siberia to the New World would do well to examine the U.S. Coast Pilot (1964 and various supplements) for the north Pacific. This issue of a coastal entry route is of little interest to Haynes. His mental landscape is filled with elephants, not sea cows. Now, with my partialities on the table, what does Gary Haynes actually have to say?

Physically, he covers his subject matter in seven chapters and 273 pages of text, tables, and very high quality illustrations from various sources, notably Ted Goebel and Dennis Stanford. There is as already mentioned, a terrific bibliography, plus a very good index, and a valuable table drawn from the work of others, including Stuart Fiedel, that provides the correspondences between radiocarbon years and calendrical years. The chapters are fittingly titled, but do not give a clue to the shoot-outs that occur within most: (1) Fluted points and the peopling of the Americas. (2) What is Clovis? The archaeological record. (3) Clovis archaeological culture. (4) The Old and New World patterns compared. (5) Figures in the landscape: foraging in the Clovis era. (6) Colonizing foragers. And, (7) Unified conclusions about the Clovis era. While these chapters do not exhaust the subject of New World colonization, since most Mesoamerican, Central, and South American sites are not discussed at all, what is reviewed is done so in careful and thoughtful ways for dating, stratigraphy, patterning, content, evidence of actual human presence, and so forth. Haynes' *forte* is his scientific skepticism and a wealth of knowledge about bones that enables him to come up with reasonable alternatives for claims of exceptionally ancient human presence. These alternatives are often in the form of *natural* taphonomy, be it perimortem or postmortem.

Given the vast number of artifacts and amounts of human-modified materials that are often recovered from later North American archaeological sites, for example, 4,000 year-old Aleut shell mounds, 2,000 year-old Basketmaker caves, 1,000 year-old Anasazi villages, and even early historic desert encampments, one can sense throughout this book Gary Haynes' frustration with having so little material culture to use in his unmistakable

desire to know how the Clovis people actually lived. I can imagine that in his mind there is a wonderful documentary film running day and night. Unfortunately it has no sound, no color, and most of the frames are black due to underexposure, or badly blurred due to all manner of taphonomic disturbances. To carry the analogy further, those researchers who also watch this film disagree on just about every single frame that they believe they have seen. But no matter how little of the movie actually exists, Haynes' will undoubtedly be long remembered as one of its strongest promoters and yet probably its most severe critic.

The book's first two illustrations, drawn by Ted Goebel, are as symbolic as they are informative. The first shows eight Clovis points, mostly from western North America. There is considerable variation in these points, but all share the same diagnostic combination of features that set them apart from all other types of projectile points found in the New World: bifacial chipping, bifacial fluting, basal grinding, lengths at least twice as long as breadth, and with bases generally a little narrower than the greatest breadth. A boat-shape outline comes readily to mind. This book is about fluted points and every conceivable related phenomenon. The line drawing symbolizes the book's content and concern.

The second illustration is a schematic map of Beringia at the Last Glacial Maximum (ca. 18,000 ¹⁴Cyr B.P.). It shows the places where humans probably could have, and not have, lived. It symbolizes the relative ease of reaching Alaska across the great expanse of the Bering land bridge, while at the same time forcing the reader to wonder why it took the epi-Clovis folk so long to reach Alaska, especially in the context of the rapid dispersal of anatomically modern humans out of Africa to most everywhere in the world, as well as back to Africa. All of the great northward flowing Siberian rivers reach the Arctic continental shelf and provide routes to western Beringia, while the eastward flowing Amur empties into the Okhotsk Sea coast, along the continental shelf of which maritime folk could also have reached western Beringia. All of these great northward-flowing rivers may have terminated in massive frozen ice barriers whose accumulations chilled and depressed the surrounding landscape even in the fleeting summer months. Still paleontological finds show that the Siberian far north was never totally closed to the advance of humans from southern Siberia.

Chapter 1 provides an abbreviated but not simplistic story of the many studies on the colonization of North America. While the insightful review, its well-chosen quotes, and numerous references by the diverse players

in this story are brilliantly composed, Haynes leaves out a couple of pioneers that I feel should have been mentioned. Most serious in this regard is the inattention paid to the very important role Aleš Hrdlička (1907, 1918; Hrdlička et al. 1912) played early in the twentieth century in his debunking of so many of the wild claims for very ancient human skeletal remains in North and South America. Hrdlička asked for nothing more than reasonable morphological and contextual evidence that was consistent with what was then known about human biocultural evolution and skeletal variation in the Old World. I suspect that if Hrdlička were alive today he would give his wholehearted approval to Haynes' book. The other scientist is William Healy Dall (1877), whose importance to North American archaeology is seldom recognized. Soon after the United States purchased Alaska from Russia, Dall conducted the first explicitly defined stratigraphic and evolutionarily-interpreted archaeological excavations in the New World. This he did in the Aleutian islands of Alaska. From this and other lines of evidence, including linguistics, he also argued forcefully against the Aleutians ever having been stepping stones in the colonization of the Americas, thus setting the stage for the eventual acceptance of the land bridge route. Hrdlička (1945) later demonstrated that despite the outstanding watercraft (large lifeboat-sized umiaks and one- and two-hatch kayaks) designed and built by Aleuts, these maritime people never reached the Commander Islands. The Commanders are located between the far western Aleutian group (Near Islands) and Kamchatka. Nor did any of the Kamchadal, or anyone else for that matter before the Russians crossed the north Pacific using wind-powered sail boats. In other words, Haynes is justified in discounting the possibility of a coastal entry into the New World, but his argument (p. 253) would have had much more scholarly fire power had he included the empirical and inferential findings of these Alaskan pioneers.

Early speculation, flawed interpretations, and just bad archaeology characterized studies of early man in the Americas, up to the critical moment when human artifacts and extinct mammoth bones were found in unquestionable association near Clovis, New Mexico (Haynes prefers to use Blackwater Draw No. 1 for the name of this site rather than the more common "Clovis" appellation). This discovery and others soon to follow demonstrated the co-occurrence of extinct animals and humans, and drew the line in the sand between the quick to emerge Clovis first and pre-Clovis outfits who have had a running gun battle ever since.

Twenty years later, the development of radiocarbon dating would establish for certain that humans using fluted

spear points had reached the New World at the very end of the Pleistocene, and that their camps, at least those that had datable organic materials, were remarkably similar in age. These *Clovis era* people, a term Haynes prefers over *Clovis culture*, would become the temporal bar over which claims for earlier occupation would have to vault. Soon after word was out about Blackwater Draw No. 1, the hunt for pre-Clovis sites can be said to have begun as a sort of “record-breaking” endeavor. And, quite properly so in my view, it continues to the present day in much the same pioneering spirit as astronomers seek to determine how old the universe is.

There is much fine reading, outstanding writing, clever expressions, and fascinating tidbits of information about who’s who in the Clovis shoot out. Even modern Indians who doubt the land bridge hypothesis get to have their say, for example Vine Deloria. However, Haynes gets to the political heart of their disbelief. He says (p.10): “Native people refuse to allow themselves to be defined as just another migrant influx.” Facing opposition from Indian country as well as pre-Clovis advocates, Haynes allows that the Clovis-firsters may have lost the emotional if not scientific battle with the pre-Clovis enemy armed with their deadly “new paradigm.” A brief quote relates his depressed yet pugilistic mood (p. 11):

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.

Haynes responds to these absolutists, first arming himself with a tabulation of early sites with well-accepted radiocarbon dates. These sites with their unquestionable man-made artifacts and datable materials, center at 11,000 rcybp (calendar years 13,000 ¹⁴Cyr B.P.; Appendix, p. 274). Haynes lists 19 such North American sites, including Anzick that also had a few human bones. Conspicuously missing in this listing are any sites that have been dated earlier that possess unquestionable artifacts and fairly straight forward stratigraphy. The temporal pattern is obvious. Clovis and Clovis-like artifacts are found nearly everywhere in North America within a narrow time range of a few radiocarbon centuries. Two explanations eventually emerge: Either Clovis explosively entered North America as Paul S. Martin has tenaciously argued, or there was some sort of remarkable pre-Clovis North

American social network that facilitated the incredibly rapid spread the fluted point concept. Such networks are well known, for example, the introduction of prehistoric Mesoamerican ideas and foodstuffs into the American Southwest. But, the acceptance of such a network is enabled by the abundant evidence of a previously existing archaeological presence, namely the Archaic Basketmaker people and their non-Mesoamerican way of life. There is no secure pre-Clovis analogy to the Basketmaker recipients. And one can only wonder, as Haynes does, why the production of fluted points would be of such great interest and value to a pre-Clovis culture who hypothetically adopted them.. Somehow, the pre-Clovis immigrants managed to cross the Siberian Arctic Circle with their simple stone tool industry as implied by the Monte Verde findings. So, given the “success” of these simple tools in the world’s most hostile environment, why was their manufacture abandoned and the fluted point technology adopted everywhere in North America within a period of a few centuries? Patently, the social network idea is neither parsimonious, empirically supported, nor analogous to a any number of ethnographic realities.

Haynes next tallies the 20 sites with human remains that are older than 8,000 ¹⁴Cyr B.P. Four of these (Pelican Rapids, Sauk Valley, Shifting Sands, and Vero Beach) have no date indicated, and no explanation as to why they are included in the table. And, what is the significance of the cut-off date of 8,000 B.P.? Haynes doesn’t explain, but the implication is as resounding as the discharge of a high-powered rifle. There are no human remains older than Clovis era finds in North America. There are no human remains like the Siberian finds at Afontova Gora, Malta, Denisova Cave, Okladnikov Cave, and others that clearly predate Clovis. I have examined some of the fragmentary New World skeletal remains listed by Haynes, primarily for the purpose of describing their dental morphology by a standardized reference system. My formal univariate and multivariate analyses of these old teeth, including others from South America, repeatedly suggest a common bond and close relationship between one another and with more recent North and South American Indians. For example, the Nevada Wizard’s Beach and Spirit Cave individuals in Haynes tabulation are dentally indistinguishable from modern Nevada Paiutes, but vastly dissimilar from Old World teeth belonging to groups such as the Ainu, Southeast Asians and Europeans who have been suggested as possibly closer relatives than are modern Native Americans (Turner 2002).

What transformed the general acceptance of Clovis first to pre-Clovis thinking? To Haynes it must have been Tom Dillehay’s work in Chile at the Monte Verde site,

because this is the only site in South America that he discusses. He does so under a subheading titled “the Monte Verde moment.” Along with the Monte Verde distraction, Haynes briefly discusses (1) the recent and contradictory migration number and timing proposals made by several geneticists, namely one to four or more migrations, some as much as 40,000 years ago (this lack of even partial agreement among geneticists tends to cast doubt on all their New World migration and dating efforts); (2) schemes about boating from the Old World across the Atlantic and Pacific oceans, untestable because all possible coastal landing sites are now under 300 feet or more of post-glacial sea level rise; and earlier in Chapter 1 (p. 14), a general disregard of the Greenberg, Turner, and Zegura (1986) three wave scenario for the peopling of the New World. This dismissal is confusing to me, because had Haynes read Greenberg et al. (1986) more attentively, he would have found friendly support for a Clovis first model of New World settlement. Despite the multitude of contradictory proposals for the number of migrations to the New World, when all is said and done in the realm of human biological variation only one migration needs to be hypothesized for most of North and South America, and one or two more to explain biocultural diversity of far northwestern North America (Turner 2002, n.d.a, n.d.b; Zegura, n.d.).

Haynes ends Chapter 1 (p. 35) with a brief discussion of what he considers to be bad science, summarized best by his view that “...skeptical replication has been eliminated from the cabinet of methods.” By this he means that pre-Clovis advocates no longer tolerate questioning of their evidence, for example, bone damage that is attributed to butchering marks. He also notes that some of the evidence for Clovis first is a bit shaky, so Chapter 2, “what is Clovis,” is devoted to an intensive review of the Clovis archaeological record.

Haynes’ approach to this review is to set up nine large geographic regions for discussion of the Clovis sites in these regions. These are almost exclusively U.S. regions that exclude most of Canada and all of North America south of the U.S.-Mexico border. Finds from regional sites are carefully inventoried in capsule form, which leads up an interesting argument: Among the thousands of fluted points that have found, there is unquestionable variation, however, Haynes proposes that all the variation can be “...reasonably accommodated with the single type concept” (p. 83). He notes that this view is contrary to that of the late R. Bonnichen who believed that the variation in fluted points was too great to be due to any single Clovis culture. There follows a rather academic discussion that reduces to the never-ending ques-

tion what is a culture, which I was uncomfortable with. In my view Haynes should have approached the variation question from a biological taxonomic perspective. The types or classes in biology are defined as natural classes or species, within which there can be great or little variation. The biological class is not defined by its variation, but by its reproductive isolation. However, variation is the criterion for paleo-species. Cultures can be usefully viewed as quasi-species, the question to my mind being, can cultural species be identified by a single element, i.e., fluted points? Now, sometimes a single tooth can provide enough information to tell if it belonged to one species or another, for example, a sea otter molar is markedly different from a human molar with no overlap in form whatsoever. We know these are molars because we have living populations that provide the information to show that sea otters and humans belong to different natural classes. But with fluted points we have no living examples that independently determine if we have one or more natural classes. Hence, in my view, neither Bonnichen nor Haynes has a sufficient argument. But when we add factors such as time, distribution, choice of materials, other associated artifacts (spurred scrapers, for example) and faunal types, and especially the rare apparent pre-Clovis site, something approaching a species definition begins to emerge. This is done on a probabilistic basis to define reproductive isolation, just as it has to be done with paleontological species because there is no way to determine if there was reproductive isolation. Now the importance of biological taxonomy is that it also is a reconstruction of evolution based on a series of relatively simple but powerful assumptions, possibly the most important of which is that *similarity in form is a good indicator of closeness of relationship in an evolutionary tree, excepting for convergence*. Hence, we can say that sea otters are not closely related to humans because their molars are greatly different from ours, among many other relative differences. These coupled with relative similarities indicate that we are both descended from some ancient mammalian ancestor. Using this analogy, can we say whether the Clovis artifact assemblage could have been descended from the assemblage attributed to epi-Clovis people? I do not know, but I suggest that this line of questioning would be a useful one to pursue, and the best place to find the characteristics of epi-Clovis would be in Siberian archaeological assemblages older than 13,000 years B.P.

Haynes (p. 92) provides a list of about a dozen features that together define Clovis sites, everyone of which is an open site. First listed are fluted points; and last, if bones are preserved, they most likely are those of mammoth. In between are other sorts of stone artifacts, radiocarbon dates centered at 11,000 ¹⁴Cyr B.P., and sev-

eral rarities such as hearth pits. From a taphonomic perspective the content of these open sites seem to have suffered severely from differential preservation since the inventory consists of hard, dense or very big bones, and small less dense items that occur in almost any Alaskan ethnographic material culture inventory are missing. In other words, the “definition” of Clovis may largely miss what was the average way of life or culture because of preservation problems and a highly nomadic way of life.

The end of Chapter 2 (pp.98-108) is rich in interesting views that Haynes holds about Clovis. He allows (p. 99) that the ancestral homeland of Clovis was in Siberia, where food procurement emphasized the exploitation of (terrestrial) megamammals (those greater than 44 kg.). Yet on the same page he says the origin of Clovis does not lie directly in Asia, rather it is an “American culture.” While fluted points have been found in Siberia, Haynes feels they are not ancestral because they are too recent, as well as being technologically different from Clovis points. Rejection on the grounds of dating is acceptable, ignoring the possibility of redeposition, but the technological distinction is hard to swallow viewed against all the variation in fluted points illustrated in this chapter. It is unclear whether Haynes rejects the Siberian fluted points as being Clovis because of the noted differences or because of, or in addition to, the fact that he follows Hoffecker, Powers and Goebel’s (1993) proposal that the Nenana tradition of Alaska was the cultural ancestor to Clovis. He also follows Goebel (1999) who has proposed that the human population disappeared from most of Siberia at the height of the last glacial and most likely did not reappear in western Beringia until around 14,000 BP, which until that time possessed severe environmental barriers to eastern Beringia. According to Laukhin (1993) these included limited and nutritionally poor foodstuffs as well as icy plains, limited fuel, and unknown sources of good stone. Haynes goes on to suggest that the North American Clovis population was about 25,000 people living in some 1,000 groups at 11,000 ¹⁴Cyr B.P. He notes that this is about half the population size proposed by Stuart Fiedel. These estimates are formulated from the number of known Clovis and Clovis-like fluted points and various sites, compared with more recent times when there are more artifacts associated with higher estimates of population size. Here, I would have liked to have seen the calculations and examples used. My gut reaction is that 25,000 people is too many until early Holocene times, but I have no way to support this feeling. Clearly, there is room for lots more discussion about Paleo-Indian population size, which Haynes says spread in a wave-like diffusion.

In my view these final pages of Chapter 2 are very interesting but much too brief, and the significance of Siberia to the initial and later colonization of the Americas is slighted and somewhat misrepresented. Three of several considerations will illustrate my concern: (1) Why in fact are there *any* fluted points in eastern Siberia, whereas none have yet been found in European Russia or western Siberia? Might the few known examples of Siberian fluted points have been redeposited by nature or humans in more recent contexts? What’s more, there are basally-thinned and end-ground bifacial “points” along with spurred scrapers, adzes, and microblade technology known for late Pleistocene north China.(2) If the many late Pleistocene Siberian sites such Denisova, Kaminnaya, and Ust-Kan in the Altai; Kurtak and Afontova Gora on the Yenisei; Kurla on the Lake Baikal coast; Kamenka and Varvarina Gora in the Transbaikalia; Geographic Society Cave in Primoria; and others, are any indication of the ancestral late Pleistocene economy(ies), then all sorts of animals were being hunted and eaten, not just megafauna (Defined later as equal to or greater than the size of a petite person or wolf but including horse, bison, rhinoceros and others. My co-PI, Olga Pavlova, is delightfully petite, but hardly in the same class with rhinos. Here, some redefinition is in order.), or megamammals (elephant-sized). While there are no late Pleistocene coastal sites anywhere in far eastern Siberia, nevertheless, they must have existed but are now under 300 feet of water. However, east Siberian rivers flowing to the Sea’s of Japan, Okhotsk, and the Pacific do have riverside sites, and at least one, Ushki, in Kamchatka, has faunal refuse that includes bones of salmon. Coastal food resources on the continental shelf would have been bountiful and varied: sea mammals, birds, fish, marine invertebrates, in addition to the on-shore terrestrial forms. I suspect that all over Siberia fish, marmots, molting birds, and subadults of all species were far more commonly sought after and consumed than adult mammoths. (3) Kuzmin (1997; Vasil’ev et al. 2002; elsewhere) and associates have recently updated the inventory of radiocarbon-dated sites in Siberia. There are sites in northeast Siberia that were occupied during the very cold *Late Glacial Maximum*. The dates for this very cold period of human use of Siberia are perfectly reasonable given all the taphonomic possibilities for the derived dates being inexact by a few hundred or thousand years (Kuzmin et al. (2002).

Chapter 3 attempts to understand and reconstruct the life way of the Clovis era, almost exclusively in economic terms because stone tools and faunal remains are about all that have been found. The associated faunal assemblages are unique because they consist almost entirely of large mammoths and mastodons that disappeared

within a few centuries of the Clovis appearance in North America. In this chapter Haynes extracts every possible bit of information that can be gleaned from the meager Clovis archaeological remains. He does a terrific job, and it is here that he gets into bone breakage, which was one of my chief reasons for wanting to read this book in the first place. Haynes includes in the bone-breakage discussion a listing of pre-Clovis sites generally lacking stone tools and where perimortem damage had been interpreted as culturally modified. Haynes includes sites such as Old Crow in Canada, and, curiously given his marked North American emphasis, Monte Verde in South America. He then lays out the rules for identifying and distinguishing damage caused by humans and non-human agencies. The first rule is “replication,” which in archaeology has three meanings: (1) experimental, (2) discovery of similar types of sites or artifacts, and (3) inter-individual agreement based on objective criteria that everyone can evaluate. Haynes gives a couple of examples of ambiguous bone breakage, each of which he proposes could just as well have happened by either natural or human processes, drawing heavily upon his African field studies of perimortem and postmortem trampling damage by elephants and other sources of scratches, polishing, fracturing, and scattering. He realistically concludes that human and non-human agencies can sometimes leave marks on bone that are indistinguishable. Based on our Siberian studies I would certainly agree, especially in the form of what we call pseudo-cuts, the frequency of which is around 5 percent in several faunal assemblages. Despite the ambiguity that the taphonomist may have with a single bone or bone fragment, I believe that several bones found in a well-defined context, even in the absence of unquestionable human-made artifacts, hearths, tent rings, etc., can up the probabilities from total uncertainty to a pretty good bet for or against human involvement. This would be especially true if an assemblage consisted of largely unburned bone fragments and a few small pieces that were fully burned. That combination would never be the signature of grass or forest fires, and human activity would be near certain. Haynes doesn't say these things exactly, but I suspect he would agree.

The last section of Chapter 3 is a bit of a stretch, namely a few pages of text and poor examples of Clovis works of art and symbolism. Scratched pebbles and possible incisions on a few bones are not worthy examples of what I feel the Clovis creative ability was capable of producing, especially viewed from pre-Clovis Siberia, but Haynes feels that “...Clovis decoration and design is exceptionally limited” (p. 158), in contrast to what was known for the abundant mobile and fixed art of the north European Upper Paleolithic. Much of the art-deficiency

problem, to my way of thinking, is attributable to the near absence of Clovis burials, especially those individuals who might have had elevated social ranking. As such, they might have been buried with distinctive mortuary objects such as those found with the two Upper Paleolithic adolescents at Sunghir, east of Moscow, and the two children recovered at Mal'ta, near Irkutsk. Even the late Pleistocene Ust-Kova open site, north of Irkutsk, has wonderful examples of art work. Another factor that may have contributed to the poor recovery of Clovis art expression in bone or stone must have been the context of these Clovis kill sites. To avoid unnecessary encounters with large predators, camp sites were probably set up some distance away from a freshly killed mammoth. Recent mountain lion attacks on humans in California attest to the danger posed by even these medium-sized predators. Haynes suggests that the absence of Clovis era rock art could be due to the needlessness of possessive territorial postings. Another possibility to consider is the general location of Clovis sites, that is, are they near rock outcrops with surfaces suitable for rock art? I recall no rock outcrops whatsoever at Naco, Lehner, or Murray Springs.

Chapter 4 is labeled as a comparison of Clovis and the north Eurasian Upper Paleolithic, however, northeastern Asia is not mentioned. This gives rise to odd comments such as “[Venus]...figurines are distributed from France to Ukraine” (p. 161). Yet, as is well known, large-buttocked, pendulous-breasted, genitally-explicit female figurines have been found in the Siberian sites of Mal'ta and Buret' in the vicinity of Lake Baikal. Haynes emphasizes that the European Solutréan and Clovis chipping methods are not similar, as D. Stanford and B. Bradley claim for their trans-Atlantic Clovis maritime origin model. Here, it would have been useful had Haynes commented on how the Siberian fluted points differ from those of Clovis since he did not do so earlier in the book. The chapter ends with the sound suggestion that Clovis is American Upper Paleolithic but there are differences between the richer and more abundant European situation and the “modest and newborn” (p. 168) Clovis. Haynes allows that “timespan” alone might account for the difference. Another source of difference might be that Clovis is more like the Siberian Upper Paleolithic, so the European comparison unfairly leads to the modest and newborn inference. However, the inference is certainly reasonable given the strong likelihood that the Beringian trek acted as bottleneck that surely filtered out some or even much of the Siberian biocultural variation.

Chapter 5 is the most theoretical in the book. The theory is derived in part from Haynes' years of African

elephant research, and from ecological models of animal dispersal, number, and their subsistence needs. Haynes asks numerous questions, such as “why are there fewer killsites of mastodonts in the east than there are of mammoths in the west?” (p. 188), and “did Clovis people hunt mammoths and, possibly as often, mastodonts?” (208). The latter question has to do with the issue of whether or not Clovis hunters “specialized” in megamammal hunting. Haynes makes a good case for some degree of specialization. Finally, he approaches the often debated “extinction” controversy, which in the polar extremes has on the one hand, “climate only” advocates and on the other, “humans were mainly responsible.” Haynes argues that humans were involved, and all the counter-arguments he dispels with his congenial but “just the facts, Ma’am” Joe Friday style, or his gun-them-down without regrets alter-ego. This is a very meaty chapter and along with the following one, together are alone worth the price of the book.

Chapter 6 provides some theoretical perspectives on colonization phenomena, largely from ecological considerations, but also from social science ideas that lean on more on economic than social or historical ideas. What emerges is a well thought out argument in support of Paul Martin’s hypothesis for rapid human colonization of the New World. The rapid dispersal model is sufficient and satisfactory, and as Haynes presents the case, better accommodates more kinds of evidence than does a scenario of slower and longer dispersal time. I especially liked Haynes additions of the lack of Clovis rock art, cumulative burial grounds, and clear-cut symbolic artifacts as useful evidence that Clovis groups were true transients, or has he says (p. 262): “...always just passing through. But however Clovis people emotionally defined themselves in the landscape, they were the discoverers and pioneers in most of the continent, and they knew it.”

Chapter 7 is a wrap up. But clearly not content with a simple summary, Haynes continues his questioning about numerous facets of Clovis life, the answers to which I sense he realizes will never be his or anyone else’s to know in much detail. But he takes one last shot at the pre-Clovis gunfighters using his best weapon—the Haynes long-barreled six shot logic special. First, he tests the wind for dispersal, finding that there are two points of view. In Pacific studies these have been called the fast and slow train models for the origin and migration of Polynesians.. For the Clovis migration the same concerns about speed and adaptation are involved. The pre-Clovis advocates adhere to what can be called a “stop and go” model that envisions a long time for the first Americans to make adaptations to the new environments they en-

countered. On the other hand, the scenario that Clovis-firsters hold can be called the “go, go, go” model that sees no problems in new environments for a culture that had evolved in Siberia to successfully deal with the world’s harshest environment, namely the high Arctic. All things considered, the majority of evidence that Haynes presents in the earlier chapters favors the go, go, go dispersal model.

As for the megamammal extinction, Haynes envisions late Pleistocene climate shifts worsening by creating vegetative zones and patches of habitat usable for the increasingly stressed megamammals. And at the time, in come the epi-Clovis Siberian explorers who find the big animals’ trails easy to read and follow. The megamammal patch recognition led to rapid “drive-by” kills over wide areas of the continent. This model envisions moving camp about once a month. Haynes gives some thought to how nomadism might have affected Clovis demography, and individual biology. There is insufficient information to deal realistically with fertility and fecundity, but he infers that mortality was likely early in adulthood, and morbidity may have been rather high. He feels that the people were physically short inferring this from skeletal remains of post-Clovis age and from southern African foragers

Shooting at everything in sight, Haynes even discharges a round at “range management,” for example, the use of fire as an aid to promote useful plant growth. This shot hits nothing, but he blames it on “taphonomic problems,” although he concludes that Clovis people did not use fire for resource management (p. 271). I would go even further and propose that resource conservation was rare to non-existent in all prehistoric New World groups, and probably so all over the prehistoric world. One only needs to review Polynesian prehistory to see how much ecological damage humans are capable of doing with even low levels of technology (Turner 2002:148).

Thus, his gun fight ends. In essence Haynes concludes that Clovis people dispersed very widely and very fast, not because they were chasing mammoths, but because they knew where to find the habitats that supported the last of the mammoths and mastodonts whose masses of meat, fat, other edible tissue, and fabrication materials made them risky but very high-value targets of opportunity. All-in-all it is a reasonable story. It satisfied my needs for a better understanding of elephant perimortem taphonomy and a good survey of information and careful thought about Clovis.

After all the smoke has cleared from the gun fight at the Old Clovis Corral, it is clear that Sheriff Haynes is

still on his feet. His deputies, Fred West (1997), and Stuart Fiedel (2000) while wounded, also remain standing. The black hats have been thwarted and driven out of town, at least for the time being. But, there is no doubt they will ride in again with renewed beleaguering of the peaceful law-abiding community of Clovis firsters.

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