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The role of mammoths in rapid Clovis dispersal

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Both empirical and theoretical evidence indicate that mammoth-hunting by Clovis foragers contributed to extinction at the same time as it encouraged a very rapid dispersal of fluted-point-making people into North America. The empirical evidence comes from fossil site studies and actualistic research on African elephant bonesites; the theoretical evidence comes from generalizations about foraging in Pleistocene environments. Rapid dispersal of hunter-gatherers is not impossible, and it was probably inevitable under certain conditions in the past. Overkill was not impossible, and perhaps also inevitable under terminal Pleistocene conditions.

De rol van mammoeten bij de snelle verspreiding van de Clovis cultuur – Zowel empirisch als theoretisch is aan te tonen dat de mammoet-jacht door Clovis jagers bijdroeg aan het uitsterven van de mammoeten alsook aan de snelle verspreiding van dit volk in Noord Amerika. Het empirische bewijs volgt uit studies aan fossielvindplaatsen en uit actualistisch onderzoek aan bot-accumulaties van Afrikaanse olifanten. Het theoretische bewijs volgt uit aannames omtrent de jacht in het Pleistoceen. Een snelle verspreiding van jager-verzamelaars kan niet worden uitgesloten en was vermoedelijk onvermijdbaar gegeven de toenmalige omstandigheden. Ditzelfde geldt voor overbejaging tijdens laat-Pleistocene omstandigheden.

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INTRODUCTION: PROBLEM STATEMENT, CLOVIS REVISIONS, AND THE PLEISTOCENE

The extent to which humans hunted elephants and mammoths at any one time period in pre-history has not been confidently established. Many of the large mammoth-bone sites of Eurasia and North America contain stone tools, clearly indicating a human presence, but the tools do not unambiguously prove that the mammoths were killed and butchered by people. This is an unanswered question of more than mere curiosity value; its answer will have a direct bearing on our understanding of disruptive changes in whole ecosystems, a topic with greatly increasing potential interest as global climates change in the future due to atmospheric pollution. The classical

views of 'Clovis Culture' in North American archeological literature (see, for example, C. V. Haynes 1980, 1987) have recently been revised according to new interpretive fashions (see the volume of papers edited by Bonnicksen & Turnmire 1991, or Meltzer 1993). The revisions also attempt to account for mammoth extinction and human dispersal into the New World. In this paper I critically review some re-visions of interpreted Clovis megamammal hunting behaviors, and I try to evaluate Clovis mammoth sites using an interpretive framework that is applicable specifically to proboscidean-hunting. I also make some propositions about mammoth behavior

that would have been important to Clovis people, and I hope to place these within an evolutionary perspective, thereby helping to explain Clovis foraging behavior and rapid dispersal throughout Pleistocene North America.

PART I: CLOVIS DISPERSAL

THE SAMPLE OF FOSSIL AND RECENT PROBOSCIDEAN SITES

This sample of Eurasian mammoth sites is from Soffer (1985, 1993; see also G. Haynes 1989, 1991). For the North American mammoth sites, I have followed Saunders (1980, 1990, 1992) and my own studies (G. Haynes 1991). I have examined some of these samples myself. For comparison, I here describe two other samples, both from modern African elephants (*Loxodonta africana*); one sample is of bones from recent die-offs, and the other is from recent culls (or 'kill-offs').

Mammuthus sites

Some of the Eurasian mammoth sites are associated with broad river valleys (as in the Russian plain and Siberia; examples are Mezin, Mezhirich, Yudinovo, Mal'ta); in these sites are possible dwellings made of mammoth bones. Other sites are situated in upland locales well away from flood plains, although often above broad river valleys. Most of the bones in them usually are situated apart from cultural 'floors' and dwellings, and in fact may be dump deposits (an example is Krakow-Spadzista Street). Fetal remains or animals of all age groups are apparently present in both types of site. Different weathering stages on the bones have been noted, along with a sparseness of cutmarks (but not necessarily on the bones of taxa other than mammoth). Gnaw-marking by carnivores is noticeable in some assemblages, but varies among the different collections.

The Eurasian assemblages (from *Mammuthus primigenius*) and the North American ones (from *M. columbi/jeffersonii*) are listed in

Table 1. The Clovis mammoth sites seem to be unique in world prehistory. Their unusualness shows up when the rest of the world's sites containing proboscidean bones are compared. The Clovis sites appear to be kills, not processing or dump sites. The largest sites contain relatively few artifacts. They are tightly clustered in time. Eastern Europe's mammoth sites that also contain artifacts are quite distinct in at least four important respects: (1) they occur in loess on upland slopes or large valley terraces, whereas Clovis sites often are in smaller stream valleys, usually within channel-fill deposits; (2) the European sites contain thousands of bones from dozens of individual mammoths, spatially associated with hundreds of stone artifacts, overwhelmingly blade technology; (3) they date to a lengthy interval, ranging from the Glacial Maximum to about 12,000 BP, whereas Clovis sites consistently date to around 11,000 BP; and (4) the mammoth mortality profiles in the Eurasian sites are characterized as Type A (see G. Haynes 1991), similar to a stable age distribution in a living population, indicating that whatever killed the animals was - in the long run - not a selective process, whereas Clovis mammoth mortality profiles are either Type B or D (this last type possibly created by opportunistic serial killing of individual mammoths).

Loxodonta sites

In the 1980s Zimbabwe saw the formation of elephant-bone sites that match the sizes and dimensions of the fossil sites. Zimbabwe contains the world's only documented and published mass-kill sites of modern elephants, and also the only known mass die-off sites (see G. Haynes 1991). Both classes of sites possess a cultural association, which in the case of the kills is a result of the direct causal process - people deliberately killed the elephants and then butchered them. In the second case, that of the die-offs, the associations are a result of visitors to the sites making use of existing bone accumulations, either while studying bones, making shelters out of the large

Table 1 Sites of *Mammuthus primigenius* (MNI greater than 10) and *M. columbi* (MNI greater than 5) referred to in this paper. Note: Spellings are from Soffer (1993). For references and more information, see G. Haynes (1989, 1991), Saunders (1980, 1990) and Soffer (1985, 1993).

<i>M. primigenius</i> SITES	
LOCATION	SITE NAMES
Central Europe	Vogelherd, Dolni, Vestonice, Predmosti, Pavlov, Milovice, Krakow-Spadzista Street
Russian River Valleys & Russian Plain	Voronovitsa, Berdych, Kirillovskaya, Radomyshl, Mezin, Pushkari, Mezhirich, Timonovka, Yudinovo, Gontsy, Dobranichevka, Borschevo, Kostenki 11 (Anosovka 2), Kostenki 2, Byzovaya
Siberia	Malta, Berelekh

<i>M. columbi</i> SITES	
Clovis Associations	Blackwater Draw, Colby, Dent, Lehner, Miami (Texas)
Noncultural/Pre-Clovis	Hot Springs, Lamb Spring, Waco

bone elements, banking up campfires, weighing down ground cover mats, or sitting on bones. Both types of sites contain human camps or sleeping areas, secondary utilization loci, and cooking areas.

In the killsites the elephants were butchered ‘en masse’. Fires were lit in shallow basins on the ground surface and kept burning much of the day. Elephant meat was skewered and roasted over the low fires. Few bones were to be found in the hearths after abandonment, but charcoal is abundant. Cloth scraps from very worn garments, buttons, coins out of pockets, match sticks, cigarette ends, newspaper bits (used to roll cigarettes), plastic bag scraps, and other kinds of cultural debris were deposited around the butchering and cooking areas. The projectiles which killed the elephants were generally left inside the

skeletons or carcasses, although a few were actively sought for recovery. The area surrounding each butchering locus was trampled or cleared by cutting shrubs and trees. After butchering was completed, vultures and mammalian scavengers (jackals, hyenas, lions, mongooses) made use of the remaining carcasses. Elephants visited many sites repeatedly, in small groups or individually, to examine the bones and to move them around. Other ungulates visited to eat bone fragments.

In the die-off sites, repeatedly re-used campsites are located along the outer edges of the central death areas. A large number of elephants died within the campsites themselves. Heavy charcoal accumulations - subsurface lenses up to 2 feet thick and 15 feet in diameter, as well as surface pavements 10 feet in

diameter - can be found surrounded by preferred bones, such as mandibles or skulls, used to make shelters or seats. The central attraction to elephants in the die-offs was water, and where the water was most abundant there are no campsite features and no preferential hoarding of elements, but there are charcoal accumulations, usually shallow and discretely bounded, as well as segments of buried very old wood (which is unusual in the campsites, where termites destroy fallen wood quickly). Artifacts such as buttons, food containers, clothing scraps, manuported stones, wood, and metal are present as a thin scatter just below the ground surface in the campsites.

Large concentrated deposits of artifacts do not occur at either the die-offs or killsites. However, I predict that since 10 years after creation both site types now provide visible artifact deposits, admittedly sparse, if both were re-visited a half-dozen times over the next century by elephants and humans they would probably provide future archeologists with fairly large assemblages. Other similarities and differences between kills and die-off sites are discussed in G. Haynes (1991). The differences relevant here are: cultural sites are not young-animal-dominated; larger adult males are rare in them; intensive butchering occurs on some or all carcasses, hence disarticulation and scattering are encouraged. The relevant differences between mass and serial deathsites are these: in mass deaths, some skeletal overlap smears the boundaries among individual skeletal foci; little carnivore utilization can be found on some individual skeletons; weathering stages differ significantly only when bone-burial is gradual or cyclic; completeness of skeletons (or bone representation) often is not skewed towards specific elements unless burial is cyclic. Slight reworking and redeposition of mass bonesites can occur and lead to the disarticulation and disassociation of individual skeletons; these post-mortem processes also may create 'preferences' in overall skeletal representation, as

certain bone elements become unidentifiably fragmented or destroyed through weathering, trampling, and scavenging.

These sites form my database. Below I consider the differences and similarities amongst them all, and offer some interpretations and propositions that may have a bearing on our understanding of Clovis hunting, Clovis dispersal, and the Clovis role in mammoth extinction.

ARE CLOVIS, MEGAMAMMAL HUNTING, AND RAPID DISPERSAL THE SAME THING?

C. V. Haynes (1984, *inter alia*) proposed that the peak of late Pleistocene megafaunal extinction in North America occurred between 11,500 and 10,500 radiocarbon years ago. (Recent attempts to calibrate the ^{14}C time-scale using tree-ring series, Greenland ice-cores, and Uranium-Thorium dating [Bard *et al.* 1990, Becker 1993] suggest that the calendar-year age of the extinction interval may be 2,000 calendar years or more older). This is the same time interval that saw a widespread occurrence of very similar projectile point types throughout unglaciated and deglaciated North America. (Because a calendar-year calibration of the ^{14}C time-scale is not yet firmly established, it is possible that this apparently brief time interval actually was longer). Not only are the point types similar (the lanceolate unfluted or fluted [Clovis] type), but the other tools found at the sites of this time interval are relatively standardized: small formal scrapers, multiple-function implements, knives and other bifacial items, perforators, and so forth (see, for example, Dincauze 1993, Gardner 1974, Hester 1972, MacDonald 1968, Tankersley & Isaac 1990). Most sites are either single-episode occupational locales, or re-used camps or kill-processing sites. P. Martin (1973, 1984, *inter alia*) has proposed that Clovis foragers spread rapidly through North America because they were big-game hunting specialists who took advantage of megafaunal

taxa (such as mammoths) that had no experience with human hunting practices. The hunters wiped out the big animals very quickly during their spread, and succeeded in populating the Americas at the same time. Later peoples (Early Archaic in the East and Far West, later Paleo-Indian in the Plains) continued camping in some of the same locales their Clovis ancestors had occupied, and also continued the tradition of carefully flintknapping tools from high quality cryptocrystalline rocks, but eventually established different patterns of resource use, scheduling, and raw material utilization.

Americanist archeologists have long suspected that Clovis hunters accounted for the deaths of at least 50 mammoths in the New World's Late Pleistocene archeological record; Agenbroad (1984) counted at least 56, referring only to multiple mammoth sites. Published interpretations of other proboscidean sites in the eastern United States (Fisher 1988, for example) now may have raised the potential total to over 60 animals. If we were to accept even the most speculative evidence advanced in recent American archeological literature, the total may climb to 75. Table 2 shows my selection of Clovis-proboscidean sites (including both the strongly supportable and the doubtful). This number strikes some archeologists as very small, when these archeologists consider the total proboscidean population that must have existed in late Pleistocene North America. My point of view is that an archeological record of 50-75 mammoths is exceptionally rich, even for a continent as large as North America, specifically because killsites will not automatically preserve (and, in fact, in my taphonomic field experience with over 10,000 African elephant carcasses in Africa, I have learned how unusual it is for skeletal sites to be preserved at all, even in moderate environments, except under rare conditions). Conditions of bone preservation were not especially favorable over most of North America at the end of the Pleistocene; and conditions of discovery are

at best limiting. Fifty mammoths in 15 sites is a fine record of kills.

WERE THE MAMMOTHS KILLED, OR WERE THEY FOUND DEAD?

Before addressing the questions about how fast Clovis could have spread (as a dispersing human population rather than as a diffusing set of ideas), I first address the bottom-line question about whether Clovis mammoth sites actually do represent the killing of mammoths, and are not the residue of time-averaged events or the remains of mere scavenging activities. Many prehistorians have no problem accepting the close spatial association of Clovis spearpoints with mammoth bones as proof beyond a reasonable doubt that Clovis people killed the mammoths with those points. The evidence is certainly direct for some sort of temporal association, but the argument for killing is on only a *prima facie* level. In African fieldwork with modern elephant-bone deposits I have seen cases where artifacts and bones are bedded together, but no behavioral association ever occurred, no face-to-face encounter between elephants and people. This is an example of time-averaging, in which even very slight differences in the time of separate events (such as the dying of elephants, the skeletonization of their carcasses, and the camping of people around elephant die-off sites) cannot easily be detected in the nature of the enclosing sediments, or in stratigraphic analysis or spatial-distributional studies of commingled artifacts and bones. I report here a preliminary attempt to investigate whether there are in fact any significant differences between the African elephant bonesites that are strictly cultural in origin (resulting from deliberate kills) and those that are wholly noncultural (resulting from starvation deaths), so that the distinguishing characteristics can be sought also in fossil accumulations. Then I describe tentative analyses of several fossil sites using these same comparative criteria.

Table 2 Clovis Mammoths and Mastodonts. Not all these sites contained Clovis points (or any other stone tool assignable to Clovis 'culture'), but they are radiometrically or stratigraphically dated to the Clovis interval, and they have been interpreted (not without controversy) as cultural. The list is not intended to be complete or comprehensive; some of the more doubtful

STRONGER CASES	
Name of Site	MNI
Blackwater Draw, New Mexico	13
Colby, Wyoming	7
Dent, Colorado	14
Domebo, Oklahoma	1
Escapule, Arizona	1
Kimmswick, Missouri	2+
Lange-Ferguson, South Dakota	2
Lehner, Arizona	13
Miami, Texas	5
Murray Springs, Arizona	2
Naco, Arizona	1
WEAKER CASES	
Angus, Nebraska	1
Duewall-Newberry, Texas	1
Dutton, Colorado	1+ (?)
Lubbock Lake, Texas	1+ (?)
Manis, Washington	1
Pleasant Lake, Michigan	1
Rappuhn, Michigan	1
Rawlins, Wyoming	1

ARE DIE-OFF SITES DIFFERENT FROM KILL-OFF SITES?

The die-off sites where enormous accumulations of *Loxodonta* bones build up are described in Conybeare & Haynes (1984) and G. Haynes (1991). The killsites, where elephant population-reduction occurred, are described in G. Haynes (1991). Table 3 lists the main characteristics of bones from untransported

assemblages formed at noncultural die-off sites. Table 4 lists the characteristics of cull-site bone assemblages. Note that at both kinds of sites elephant bones greatly outnumber bones from other taxa, which are invariably present.

The main features of these sites are:

(1) Cutmarks are very rare or nonexistent at

Table 3 Main characteristics of bones from untransported assemblages formed at noncultural die-off sites.

<i>Loxodonta</i> Die-Off Site Characteristics
Multispecies (but about 90% elephant)
Variable (but mostly light) carnivore utilization
Rare butchering signs
Dominated by young age-classes
Thin scatter of artifacts, some buried charcoal lenses
Variable areal extent

Table 4 Characteristics of cullsite bone assemblages.

<i>Loxodonta</i> Mass Killsite Characteristics
Other species not recorded, except carnivores
Fuller carnivore utilization at edges of sites, and on largest carcasses at site centers
Rare butchering signs
All age classes present; around 50% youngest class
Scattered artifact distribution; one-time use of features

Table 5 Abbreviated description of some main differences in gnaw-damage 'stages' (see G. Haynes [1980, 1981, 1982] for more detail).

Carnivore Utilization Stages		
STAGE 1 Light to Moderate	STAGE 2 Full	STAGE 3 Heavy
femur trochanter & trochlea scored	some bone shafts lightly scored	scattered bones
humerus tuberosity furrowed	longbone epiphyses removed	vertebral segments possibly articulated
articulated head, legs, thorax	leg bones disarticulated	longbone shafts partly broken open
scapula & ribs broken, scattered	thorax articulated	

both types of sites;
 (2) All age classes are recorded as present in all known examples of both site types (there are 6 known die-off sites and over 250 cullsites). Figure 1 shows age classes at four cullsites; Figure 2 shows a typical

age distribution in one die-off site;
 (3) Carnivore gnaw-damage is mostly light to moderate, and almost never heavy in both site types. Table 5 gives a very abbreviated description of some main differences in gnaw-damage 'stages'. See G. Haynes

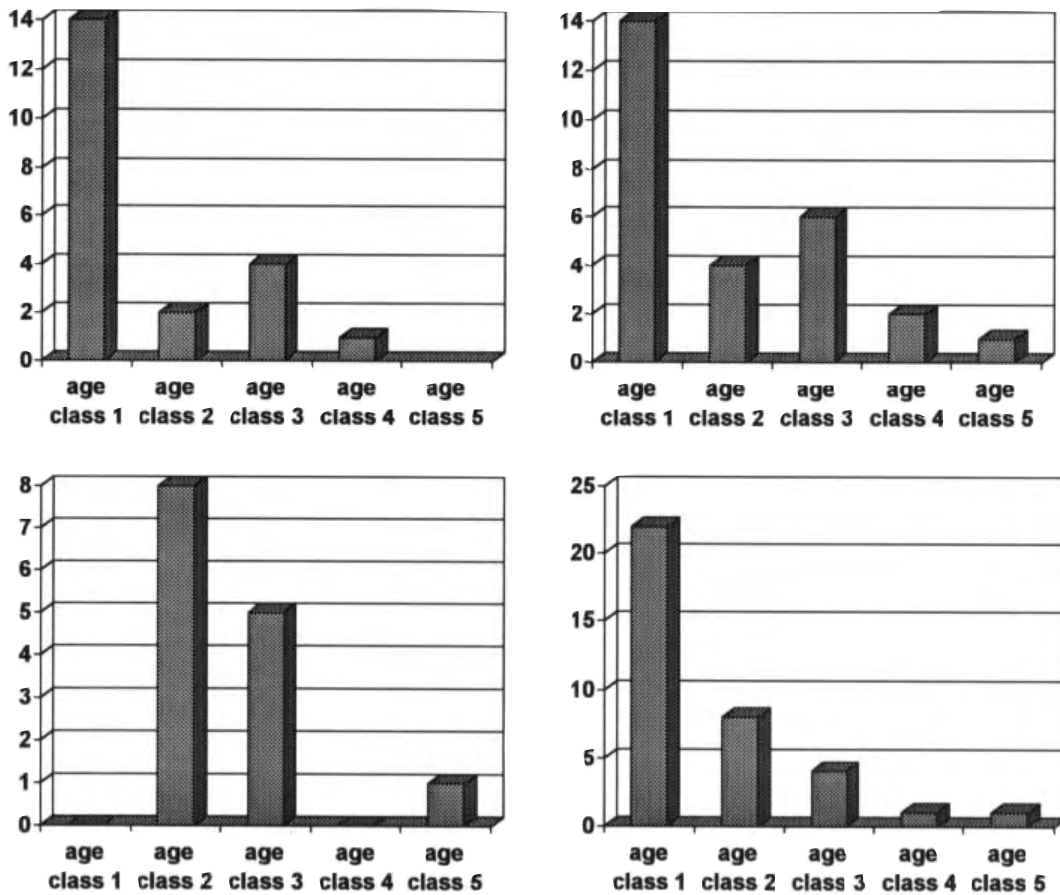


Figure 1 Age profiles at four cullsites, 1984.

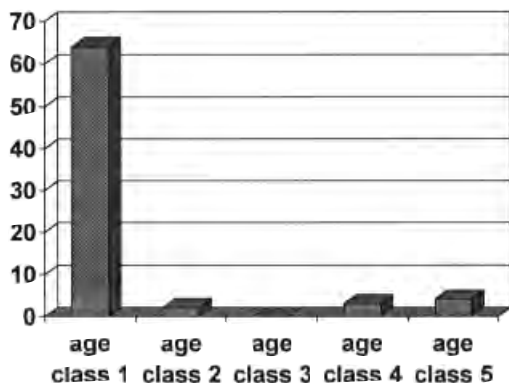


Figure 2 Age profiles at Nehimba die-off, 1994-1995 (n=73).

- (1980, 1981, 1982) for more detail;
- (4) Artifacts and fire-features can be found bedded with bones in both types of site;
- (5) Bone representation in cullsites may show some 'biasing', since certain elements were transported away for further processing; skulls and mandibles or parts of them are sometimes missing; leg bones were occasionally removed with meat attached; feet were sometimes taken away.

Table 6 shows that there are certain differences between *in situ* and transported mass-sites and serial sites. I refer to four variables: (1) age profile; (2) bone representation; (3) carni-

Table 6 Differences between *in situ* and transported mass-death sites and serial death sites.

Mass Death Sites	
<i>in situ</i>	transported
Nonselective age-profile	selective age-profile
Nonselective bone representation	selective bone representation
light carnivore utilization	light carnivore utilization
similar weathering stages	similar weathering stages
Serial Death Sites	
Nonselective age-profile	selective age-profile
Nonselective bone representation	selective bone representation
heavy carnivore utilization	heavy carnivore utilization
Different weathering stages	different weathering stages

Table 7 Comparison of the weathering stages and passage time within each stage for tropical environments (see Behrensmeyer 1978, Tappen 1994) and northern temperate and subarctic environments (G. Haynes 1981, 1982).

Stage	Tropics	Northern
0	greasy (0-3 yrs)	greasy (0-3 yrs)
1	split-line cracks (0-3 yrs)	split-line cracks, fibrous peeling (3-12 yrs)
2	thin-flaking, cracking (2-6 yrs)	deeper cracking, roughening surfaces (10-15+ yrs)
3	fibrous flaking (4-15 yrs)	fibrous peeling, splittling of diaphyses along cracks (15-25+ yrs)
4	slab-flaking (6-15+ yrs)	seen on bones once buried, reexposed; slabs of bone peeling off
5	falling apart (6-15+ yrs)	seen only on recycling old specimens

vore utilization; (4) weathering stages. In Table 7, I compare the weathering stages and passage time within each stage for tropical environments (see Behrensmeyer 1978, Tappen 1994) and northern temperate and subarctic environments (G. Haynes 1981, 1982). The actual stages differ in each environment; in the north the intensity of weathering is reduced, as well as the rate of weathering. Hence the degree of weathering per stage seems impeded. Following from Behrensmeyer's (1978) original suggestion, I propose that the differing amounts of solar radiation and ultraviolet light create the distinctions in passage time and stage intensities. Solar radiation in the tropics is about 2-3 times more intense than

in sunny northern latitudes. In Zimbabwe, for example, summer radiation reaches 1.4 calories per square centimeter per minute; in England, a sunny summer day may see 0.6 calories per square centimeter per minute (Stevens 1970).

To quantify the intrassemblage differences in *Loxodonta* sites I have assigned numerical scores to the four variables. Light carnivore use scores 1 point, while heavy use would score 3. Generally similar weathering among bones scores 1, while a mixture of significantly different weathering stages scores 3. Nonselective bone representation scores 1, while selective or clearly biased representa-

tion scores 3. A nonselective age profile scores 1, while a clearly selective profile scores 3 and an ambiguous one scores 2. Using these scores I compared *in situ* and transported assemblages (resulting both from mass and serial deaths), and cultural and noncultural assemblages (also distinguished as mass or serial in origins). Table 8 shows the scores for each kind of *Loxodonta* assemblage. Scores of 4-7 points suggest mass accumulations in which the numerous animals died at the same time or very nearly the same time; scores of 9-12 from multi-individual bone accumulations indicate that death occurred at different times. Very low numbers result when humans killed the elephants, either en masse or one at a time; very high numbers also indicate this same origins for the sites. Hence mid-range numbers result when the deaths were noncultural.

Wishing to know if these numbers could predict the origins of fossil mammoth sites, I have scored a few better-described assembla-

ges or ones that I examined firsthand (Table 8). My scoring subsample of *M. columbi* includes Hot Springs, Lamb Spring, Waco, Dent, and Lehner. The first three lack clear associations with stone artifacts and pre-date Clovis in the New World. I have examined them personally. The other two are Clovis sites that are either well described or well studied. My scoring subsample of *M. primigenius* includes Berelyokh, Krakow-Spadzista Street, Mezin, and Mezhirich. I have studied part of the first two assemblages, and the literature about the second two is easily accessible. Tables 9 and 10 show the tentative scores I have assigned to the sites. I emphasize that these numbers are first approximations and subject to change as more studies are completed. The scores are quantifications of features that are partly defined impressionistically and partly objectively. I hope other analysts will further objectify the scoring process. The scores seem to agree with the conventional interpretation that the Clovis sites Dent and Lehner are actual kills. The scores

Table 8 Scores for different types of recent *Loxodonta* sites

<i>In situ</i> versus Transported					
Serial Deaths Only			Mass Deaths Only		
Variable	<i>In situ</i>	Transported	Variable	<i>In situ</i>	Transported
Carnivore Use	heavy (3)	light (1)	Carnivore Use	light (1)	light (1)
Weathering	mixed (3)	similar (1)	Weathering	similar (1)	similar (1)
Bone Representation	nonselective (1)	selective (3)	Bone Representation	nonselective (1)	selective (3)
Age Profile	varies (2)	selective (3)	Age Profile	varies (2)	varies (2)
SCORES	9	8	SCORES	5	7

Cultural versus Noncultural					
Serial Deaths Only			Mass Deaths Only		
Variable	Cultural	Noncultural	Variable	Cultural	Noncultural
Carnivore Use	heavy (3)	heavy (3)	Carnivore Use	light (1)	light (1)
Weathering	mixed (3)	mixed (3)	Weathering	similar (1)	mostly similar (2)
Bone Representation	selective (3)	nonselective (1)	Bone Representation	nonselective (1)	nonselective (1)
Age Profile	selective (3)	selective (3)	Age Profile	nonselective (1)	selective (3)
SCORES	12	10	SCORES	4	7

- if valid - also seem to indicate that all the mammoths in these sites were killed either 'en masse', together that is, or over a very short period of time (less than 5 years, if my scoring of weathering is correct [see Tables 7 and 9]). Saunders (1980, 1990) has suggested that some Clovis sites were mass kills, but other interpreters argue that serial killing was a more likely strategy to explain Clovis sites (Frison & Todd 1986, G. Haynes 1991). The scores also seem to indicate that all four Eurasian woolly mammoth sites in the sample are cultural in origin, including Berelyokh, which I have never before interpreted this way. The scores suggest the mammoth-bone dwellings at Mezin and Mezhirich were gathered together from serial kills.

Keeping these scores in mind, I will discuss some important characteristics of proboscidean behavior and optimal foraging decisions by late Pleistocene people, and the possible links between them that explain rapid human dispersal and mammoth extinction. I structure the discussion as a set of arguments replying to recent revisions of Clovis culture. Finally I will propose some reasons to explain the differences between Clovis mammoth sites and Eurasian mammoth sites.

HOW FAST DID CLOVIS SPREAD?

Here I examine two problems associated with explaining Clovis behavior: how fast did Clovis spread (or how slow was the spread of the first peoples in the Americas), and how

potentially destructive could Clovis have been, especially to mammoth populations. Rogers *et al.* (1992: 284) state that a 'postulated high speed of migration through varied ecological zones seems unlikely' in late Pleistocene America, but they cite only MacNeish (1976) as their authority and source. MacNeish (1976: 326) had argued that P. Martin's (1973) postulated rate of movement of 12-16 km a year was extremely rapid and not supported by early chronometric dates on several North and South American sites. Other prehistorians have doubted that foragers could have spread throughout new range at high rates, but their reasoning seems weak. For example, Whitley & Dorn (1993) are troubled by the improbability of several different migration and reproduction rates proposed in archeological interpretations of Clovis migrations; they scorn P. Martin's (1973, Mosimann & Martin 1975) preferred 16 km and 3.4% per annum, dislike C. V. Haynes' (1966) suggested 6.4 km and 1.3% per annum, and seem to like Hassan's (1981) favored 1 km and 0.1% per annum. Apparently Whitley & Dorn (1993) reject the faster rates because the ethnographic analogies behind them did not involve temperate-zone Pleistocene foragers; Hassan's (1981) figures seem to be preferred for no better reason than that they are so conservative. It seems to me that any proposed rate is hypothetically acceptable, whether based on analogy or not, as long as it is in line with the biologically possible parameters of human reproduction. MacNeish (1976: 326) weakens his 'argument' for rejecting the 16 km/year

Table 9 Scores from the sample of *M. columbi*

<i>Mammuthus columbi</i> Scores		
Name of Site	Score	Meaning
Hot Springs	8	noncultural, mass/serial
Lamb Spring	8	noncultural, mass/serial
Waco	7	noncultural, mass
Dent	5	cultural, mass
Lehner	6	cultural, mass

Table 10 Scores from the sample of *M. primigenius*

<i>Mammuthus primigenius</i> Scores		
Name of Site	Score	Meaning
Berelyokh	5	cultural, mass
Krakow-Spadzista Street	5	cultural, mass
Mezin dwellings	11	cultural, serial
Mezhirich	11	cultural, serial

rate of spread by proposing that this very same rapid rate did occur by his Stage IV of the peopling process. According to his reckoning, 'movement was occasionally, if not regularly, relatively rapid' within broad eco-zones, but movement from one zone to the next (such as from northern Mexican deserts to the tropical jungles of Central America) 'would require that [the migrants] build up a whole new adaptive subsistence complex', and therefore must have slowed down a great deal. The underlying argument here is based on the idea of barriers to rapid colonization, barriers that humans could not cross without stopping for a while and rethinking their foraging strategies, as well as (apparently) re-designing their mobility, re-scheduling their patch use, expanding or contracting their diet breadth, restructuring their toolkits, and changing everything else, including clothing, one supposes. We may call this the Doctrine of Barriers, a preconceived set of assumptions that many prehistorians seem to possess about human abilities to change habitats, to disperse, and to invade new ranges.

Meltzer (1993: 102), a thoughtful leader in the debate, writes that archeologists 'have long assumed' migrations to the Americas were few in number, each consisting of a small number of people who were closely related but different from those before and after them. Migration was a slow, time-transgressive event, because to the first Americans (Meltzer 1993: 80), 'this was new and exotic land, which became ever more alien to their Beringian experience as they encountered different habitats on their journey south'. 'With every ecological boundary they crossed, and there were many, the process of finding water, food, and critical materials began anew'. 'The migration process has slow rhythms'. Once again, the underlying assumption seems to be that ecological barriers were like walls in front of advancing human populations. In the eastern United States, according to Meltzer, there were two very different biotic communities that Paleo-

Indians adapted to: the northern tundra and spruce parkland, and a boreal-deciduous forest. Caribou were hunted in the north, while in the 'species-rich forests' of the south the people were generalists who depended on 'extensive but dispersed' resources. The southern nut-collectors left 'little trace in the archaeological record', while the caribou hunters left 'a site record more conducive to discovery' (Meltzer 1988: 41-42). Here were people living at the same time but, if we are to believe in the doctrine of barriers, they must have been forced to develop very different adaptive complexes, in MacNeish's (1976) terminology. Yet the archeological record shows they were all using similar toolkits. Meltzer proposed the generalists used huge, dispersed patches of resources, while the specialists used small patches. Nut collectors would have utilized very labor-intensive and costly (but reliable) resources, and should be expected to have left heavy technological remains at sites that are consistently re-used over many years; caribou-hunters may or may not have intercepted herds at the same sites regularly, but their archeological visibility will be relatively low. The caribou-hunters should not be as archeologically visible as the nut-collectors; but in fact this is the opposite of what Meltzer predicts.

HOW DESTRUCTIVE COULD CLOVIS HAVE BEEN?

Perhaps corollary to a belief in barriers and human inability to find food, water, lithic resources, and other necessities, is a belief that Clovis foragers could never have hunted whole mammoth populations to extinction so quickly. I have suggested (along with some other prehistorians) that perhaps Clovis could have spread so widely and so quickly because megamammals were under increasing seasonal stress at the end of the Pleistocene (see C. V. Haynes 1991 for evidence of a Clovis-age drought). This proposal has been misunderstood; one reviewer (Mithen 1993: 713) of the book where it appeared, for example, thought I was saying

that Clovis was 'specifically adapted to finding and exploiting mammoth die-offs'. 'Unfortunately', the reviewer comments, my case 'is not very convincing'. Meltzer (1994: 420) could not believe that Clovis 'pursued dying and easy-to-scavenge proboscideans', because he expected that there would be many more mammoth and mastodont (scavenged) archeological sites 'than we possess'. What I actually proposed was not that humans pursued dying mammoths or were specifically adapted to finding them, but that humans rapidly spread in North America because they were capable foragers who were able to take advantage of mammoth die-offs in refuges where crowding exacerbated climate-mediated stress. My argument was about forager opportunism, the cultural flexibility expected from alert and responsive foragers who could capitalize on natural windfalls; I was not claiming that Clovis had developed 'special adaptive complexes' to do nothing but scavenge elephants in the terminal Pleistocene.

Because the idea of 'overkill' is often intellectually resisted as a linchpin of rapid Clovis migration, it is worth addressing a few of the arguments that are currently thought to demolish it. Meltzer (1993: 109) calls overkill 'unrealistic'. He does not believe that Clovis populations reproduced at 3.4% per year and that hunter-gatherers can come in waves and kill as many animals as Mosimann & Martin's (1975) model postulated. However, opinions about these issues should not carry the weight of fact: human populations can and do reproduce at this high rate, and higher, under favorable conditions. Meltzer also refers to the Marginal Value Theorem of Foraging theory, which he paraphrases as predicting that foragers generally decide to leave habitats when 'food supplies dip below what is available elsewhere'. This is not exactly what the classic version of this theorem actually predicts: it predicts that foragers who optimize will drop an item from their diet or leave a cer-

tain habitat when they perceive the benefits of that item or habitat as dipping below an average value from all other items/habitats. While Meltzer appeals to this theorem to refute the overkill hypothesis, and not to help him justify his belief that migrations into North America were very slow, another researcher - in fact, a prominent anthropologist and foraging theorist (Winterhalder 1981: 97) - has postulated that Marginal-Value decision-making can explain why the first populations of people migrated so rapidly throughout North America.

An evolutionary perspective on the possible heavy hunting of mammoths may be useful here. Evolution acts on individual variability, of course, and natural selection favors individuals with behavior that has a high fitness payoff. Keeping this in mind, then, what would be the definable goals of a dispersing forager group entering unoccupied ranges in the New World? One goal might have been range expansion and estate settlement (see Beaton 1991). If so, it makes sense to predict that a high reproductive rate and quick movements away from birthing sites would also be positive goals under conditions of high returns from hunting and gathering activities. To optimize, to minimize search times for high-ranked resources and thereby save time and energy, and to maximize food returns and social payoffs to the dispersing group, the decision would have been made to take very high-ranking resources when encountered. If encountered often they would have been taken often.

Now, according to Meltzer, Clovis people would have violated the Marginal Value Theorem if they continued to hunt mammoths even as mammoth densities perceptibly diminished as a result of the hunting. As a resource is harvested, according to ecological theory, its density logically is expected to decline, hence the cost per unit harvested will proportionately increase. I suggest here that at the end of the Pleistocene, when mammoths were

clustering in refuges and ranges in response to global climate changes, it is possible that the removal of dying mammoths by Clovis people actually improved the forage for surviving or immigrating mammoths in the refuges. The killing of mammoths by Clovis people may have taken a load off local biotic communities; too many mammoths crowded into refuges is bad for local biodiversity, and the removal of dying and stressed animals may have led to improved range conditions, and thereby increased carrying capacity. In other words, mammoth-hunting by Clovis people could have had a positive feedback effect on overall resource levels in specific patches and habitats.

The Marginal Value Theorem predicts that when average returns from all of an environment exceeds the returns from one patch (such as a mammoth refuge), the patch is abandoned. But if a forager has no significant effect on the density of the major resource in the patch (the mammoths), there would be no perception of diminishing return and no patch abandonment as predicted. So even if Clovis mammoth-hunting was not 'improving' local patch conditions, the Marginal Value Theorem would still allow heavy mammoth-hunting to take place; dying and stressed mammoths may have seemed plentiful to Clovis hunters, even as they killed all they wanted. The lag time between Clovis perceptions of no decline in mammoths and the actual extinction of mammoths may have been a decade or shorter. By the time the mammoths died out entirely, possibly in huge numbers, Clovis decisions to abandon refuge patches no longer would have been possible. A third set of conditions would have allowed Clovis overkill of mammoths without violating the Marginal Value Theorem. If a forager completely harvests all there is to take of one resource (a point abruptly reached), he may decide to abandon a patch without ever perceiving diminishing returns (Kelly 1995: 92), perhaps a 'fairly common situation in human foraging'. Furthermore, the Marginal Value

Theorem assumes that travel time between foraging patches is unproductive; yet Clovis travel time between patches of mammoths may in fact have been very productive. Searching was not random; mammoth tracks and trails would have been clear indications of which patches to head for, and Clovis people would have been assured of where the resources were to be found. The predictability would have allowed extra exploration, extra toolstone quarrying treks, extra experimentation with new resources, etc.

Another model in optimal-foraging thinking is the patch-choice model. This states that when resources are distributed in nonuniform patches, and when patches are sequentially encountered at random, a forager should decide to abandon any one patch when resources are perceived as dropping. The basic query is: which patch to exploit (not which resource to exploit)? It may seem difficult to reconcile a postulated heavy Clovis hunting of mammoths with this model. However, upon reflection, it is no problem at all helping to explain rapid Clovis dispersal and very heavy Clovis hunting. Late Pleistocene patches were multi-resource refuges, according to recent interpretations of deglaciation processes. Hence they were not sequentially encountered by Clovis foragers, but were traveled to directly. I would expect that to Clovis foragers the cost of abandoning such rich patches clearly outweighed the costs of harvesting diminishing returns.

This postulated gorging of Clovis foragers on mammoths may have been cyclic and ephemeral, as well. The hunting of mammoths at refuges must be averaged into a coarser-scale (longer-term) model of Clovis strategies. Mammoth resources in patches would have varied in intensity (they may have been abundant temporarily), frequency (there were yearly changes), spatial extent (affecting thousands of square kilometers), and predictability (mammoth stress periods and crowding into refuges may have been

known to Clovis people months in advance) (Kelly 1995: 101).

My disagreement here is over whether or not Clovis people would have evaluated the costs of search and pursuit as going up, and the returns as going down, at the end of the Pleistocene, when they were dispersing throughout an unoccupied continent and killing megamammals at will. If the human dispersers settled into specific ranges (which the archeological evidence indicates), they need not have perceived certain dietary items (mammoth, for example) as providing less and less return, but instead as providing more and more return the more they were hunted. This is because mammoths were not dispersing into scarcer and scarcer little bands, but instead were crowding into refuge areas as climate change affected their ranges.

According to Meltzer (1993: 109), 'If they resembled most foragers', Clovis populations hunted big-game animals only occasionally, and then they targeted only a few taxa (and not the most dangerous ones). Olsen (1990) argues along the same vein. But these opinions are not especially strong evidence for the case that Clovis did not specialize in the hunting of mammoths. Of course Clovis did not resemble most foragers - Clovis lived in a very different world, at first without competition from other humans in adjoining regions. Clovis people were pioneers, which no modern forager ever will be. Clovis people inhabited nonanalogue habitats, with nonanalogous demands for mobility, storage, range sizes, and so forth (see Kelly & Todd 1988, also Binford 1990, for a discussion of some possible Pleistocene and Holocene differences among foragers). Clovis witnessed mass extinctions, which modern foragers have not experienced. Clovis had no dominating neighbors who demanded land and pasture for domesticated foods; they had no market economy to participate in. Modern foragers do. And whether or not Clovis hunted 'dangerous' animals is

not for twentieth-century urban scientists to guess at. Personally I find elephants dangerous and often frightening; but I do not dictate that my emotions must be the same ones programmed into every other human being who encounters elephants.

WHO CARRIES THE BURDEN OF PROOF?

Meltzer (1993) argues that late Holocene Moa killing in New Zealand left many sites behind as a clear example of the process of overkill, whereas mammoth-hunters left what he thinks of as very few sites.

Therefore, he reasons, mammoths were not overkilled. His argument is based upon what I call the Doctrine of Visibility: it seems to be widely assumed that prehistoric behaviors will leave archeological visibility in proportion to the importance of the behavior.

Meltzer implies that killing many animals leaves many sites with many bones in them. I consider this line of reasoning to be very weak. In the first place, three million years of hominid elephant-hunting (or scavenging) in all of Africa have produced far fewer than 15 clear archeological sites (see G. Haynes 1991). Africa is more than three times larger than the 48 contiguous United States and if three million years and three times the land area cannot provide a large number of elephant-killing or butchering sites, why should the very brief Clovis interval (11,500-10,500 yBP) in a smaller landmass produce a large number?

It can be argued that African elephant-hunting probably was never 'overkill', and therefore its three million years of site scarcity should not be compared with the Clovis 500-1,000 years of site abundance, but I would argue in rebuttal that even when unambiguous 'overkill' cases are found, sites are still scarce. Two specific cases are relevant here. The first case involves elephants that were killed in great numbers during the Victorian-era ivory boom; northwest Zimbabwe was emptied of elephants very quickly between

1868 and 1878 (G. Haynes in preparation). The hunters shot any and all elephants for their tusks, and I think this kind of hunting with rifles and muskets clearly qualifies as 'overkill'. In an attempt to reconstruct elephant densities before and during the slaughter, I have read through unpublished diaries, letters, and other papers in Zimbabwe's National Archives. As well, I have examined the published discussions of hunting success, the records of ivory shipments, and commercial records (for example, Kunz 1916, Selous 1881), and I have estimated the total numbers of elephants shot in the Hwange National Park region of Zimbabwe at about 6-8,000, a very generous estimate indeed. This was not all the elephants living there, but in the end it was all that could be found by the expert trackers who spent weeks searching the woodlands in the dry season (see, for example, Selous 1881). Many sources contain maps showing where the hunters established camps and shot elephants for several months at a time. Not a single bone from a single elephant shot in these kills has ever been discovered over my 13 years of intensive surveying in Hwange National Park. The skeletons simply weathered away into dust, because most of them were located in upland locales where preservation was not optimal. Skeletons that were located around water sources may or may not be awaiting discovery underground.

A second case of African-elephant overkill occurred in the mid-1980s when Hwange National Park herds were reduced by culling. Culling is a nonselective kill of whole groups encountered by professional hunters. During the culls of 1984-1986, about 9,000 elephants were shot, averaging 3,000 a year over a three-month period each year. The culls occurred in an area of about 6,000 km². The bones of the shot herds are located in more than 250 different sites in upland bush and woodlands, and while skeletal elements can still be identified in these assemblages, the bones are rapidly weathering into soon-to-be

unidentifiable splinters and fragments.

Trampling continues to destroy some bones, while bone-eating by carnivores, rodents, and ungulates removes others. These upland sites will eventually disappear from the potential future fossil record, because of poor preservational conditions. If 9,000 elephants shot in 280 days could not produce an archeological record of overkill, why should we expect the mammoths and other megamammals killed by Clovis spears over a mere several hundred years or less to have produced a huge record?

Meltzer (1993) compares Moa-sites with the sites of African elephant-kills that I witnessed in Zimbabwe. In the prehistoric New Zealand Moa sites, according to Meltzer, Moas 'were killed in far greater numbers than African elephants' (Meltzer 1993: 110), in a narrow range of time, and there are lots of killsites to be found. Therefore, he implies, if Clovis people were killing off huge numbers of mammoths at the end of the Pleistocene in North America, they should have left huge numbers of sites, just as the Moa hunters apparently did. My problem with the reasoning is that I find it unlikely that prehistoric Moa-hunters killed tens of thousands of Moas as quickly as did the cullers of African elephants, 3,000 every 3 months; the archeological record in New Zealand may not indicate slaughter on this rapid and intense a scale. Large accumulations of Moa bones in midden deposits were once common sites in New Zealand (see Trotter & McCulloch 1984). However, many Moa sites contain the remains of relatively small numbers of individual Moas, not massive piles of large numbers of birds (Kooyman 1990). Moa sites are situated in a range of settings, but many were located in lowland locales, along rivers for the most part, and sometimes in shelters and caves; in contrast, the killsites of African elephants were found exclusively in open upland woodlands, where their bones suffer considerable loss from weathering and other subtractive processes. So even though more elephants than Moas were killed in a shorter

time period, there are far fewer remains of them left to find than there are of Moas, due to differences in preservational factors.

The Moa bonesites may not be comparable in location, MNI, or in many other characteristics with the Clovis mammoth sites. In fact, many Moa sites appear to be re-used habitation sites (not killsites) that contain several different species of Moa (Anderson 1984, Kooyman 1990, some of the nineteenth century literature is cited in Trotter & McCulloch 1984), suggesting several different episodes of hunting. Earth ovens, midden deposits also containing eggshells, and artifacts occur associated with the Moa bones. A reasonable interpretation of these sites is that they are locations to which Moa bones were carried from killsites over and over again. The Moa hunts may have been modest in scale but regularly repeated by individual hunters. The Moa midden-sites are very different from the Clovis mammoth sites. The Clovis sites are short-term kills or scavenge sites; the Moa sites may be mostly transported (consumption) sites created over extended periods of time. Therefore, the existence of so many Moa sites is wholly irrelevant to the arguments about mammoth-site scarcity and overkill. The reason the mammoth sites do not look like the Moa sites has to do with transportability of bones and body parts; Moa hunters brought meat back to camps, but Clovis hunters brought their camps to the kills.

Because there are only 15 or 20 proboscidean sites in North America, Meltzer (1993: 112) proposes that mammoths were hunted 'only occasionally and at great risks'. I do not agree with this conclusion or with the reasons for reaching it. If archeologists in New Zealand had clearly discovered 15 or 20 (or more) unambiguous cases of Moa killsites, instead of the many (probably) time-transgressive and transported assemblages that are known, then it would be appropriate to compare the Moa overkill signature to the mam-

moth pattern in North America. Right now, however, the Moa sites are not a valid yardstick to measure mammoth sites against. If they are consumption sites rather than killsites (Kooyman 1990), a high number of them proves nothing about the expectable number of overkill-associated killsites.

As far as the argument goes that Paleo-Indians would never have chosen to hunt proboscideans, because modern hunters avoid them, I can only offer opinion. Hunters who live with elephants learn how to hunt them. I know from years of personal experience that elephants are approachable when under stress, such as starvation or drought. In fact, a person can easily slap the side of a drinking elephant when it is in a state of stress during the dry season; otherwise, elephants are alert, irritable, and very difficult to approach without creating some sort of distraction. Of course, anyone foolish enough to slap a drinking elephant may not survive the encounter, but an armed paleolithic hunter supported by other hunters can outrun and outmaneuver an elephant. These are opinions, not proclamations about Clovis abilities, but I think they make an effective counter-argument against the opinion that Clovis hunters rarely (or never) preyed upon mammoths.

ECO-BARRIERS?

If 'overkill' cannot be refuted by opinion, then the idea of very rapid migration by Clovis people also cannot be refuted. The proponents of a slow migration insist that paleolithic cultures will behaviorally freeze upon encountering ecological differences along the path of their movements. I return now to the Doctrine of Barriers. My arguments against this doctrine have to do with the actual nature of 'barriers' that may have confronted Clovis dispersers. The late Pleistocene in the Americas was a time of unusual mosaic habitats, very much unlike the zonal habitats of today. The Pleistocene mosaic possessed different species associations in different cells of the mosaic, but

diversity was exceptionally great, due to the fact that the cells were not pushed together too closely nor were they widely removed; instead, free access to all the cells must have been allowed. The cells could not have been 'fenced' from each other, either in the form of ecological barriers or in the form of distance separating them. I am not inventing this idea; it is a principle familiar to conservation biologists.

Ecozones in mosaic habitats are not distinct, or even zonal, on either a narrow or broad scale, unless abrupt altitudinal changes are taken into consideration. Even today, in post-Pleistocene North America, if a mammal were to undertake a walk from Canada to Mexico, without deliberately seeking mountains to cross, it would at no point encounter an impassable, distinct, and sharp transition from one ecozone to another. During the final phases of the Pleistocene, diversity was higher in many locales than it is today, offering richer and possibly a more even distribution of food resources to our traveling mammal, making the trek less costly and less difficult.

Take the example of the armadillo. They certainly do not possess 'adaptive subsistence complexes' built up in their dispersal from the southern part of North America, yet they are found from Texas nearly to South America, from prairie habitat to thick forests. How can they survive in such different ecozones with no ingenious culture? Coyotes and wolves also are found in a wide range of different ecozones, from high arctic to thick warm forests. How do they survive? The point is that these animals, herbivore and carnivores, do survive in an amazing range of conditions, and move successfully through the gradient linking all the conditions. Clovis people would have been able to move along the gradient as well. One demand that Clovis had and that the armadillos do not, of course, is for cryptocrystalline stone, but I would imagine that explorers and pioneers can get along with already-made tools, or improvise

bone and wood tools while searching for stone-sources. I guess that it would probably take no more than a few weeks for a band of 25 expert foragers to find out where the stone sources are located in any 1000 km² range.

Furthermore, I would argue, it cannot be said that temperate ecozones actually do have barriers separating them. Different ecozones do not normally meet at distinctly defined walls. Features like the Great Wall of China do not separate North America's latitudinally defined habitats. Clovis foragers would not have been confronted by closed and locked doors and windows that they had to learn how to open. The Pleistocene and Holocene are characterized by ecozones with broad, mosaic ecotones linking them. It is clear that jungle differs greatly from desert, but it is also clear that ecotones act to 'blend' different habitats. It is clear that survival in the Amazonian rain forests requires behavior that is different than that required for survival in the North American high plains. Or is it so clear after all, especially when dealing with the late Pleistocene period?

My quibble here is twofold - I do not think there is clear evidence that jungle Paleo-Indians existed; and I do not think that the toolkits and the general behavior patterns exhibited by Pleistocene foragers needed to be all that different for the successful colonizing of a huge area of the Americas. This last statement may give some archeologists an opportunity to feel superior, since they have been persuaded that people were adapted to an Amazonian ecosystem at nearly the same time as other people were adapted to different environments far away in the Americas. The radiocarbon record may or may not support the proposition that Clovis is just one of many separate 'cultures' inhabiting the New World at the end of the Pleistocene. Ruth Gruhn (1993), Alan Bryan (1986), and Richard MacNeish (1976), among others, have argued this proposition numerous times, but to a cautious and skeptical audience for

the most part. Other archeologists have been mystified by this insistence that different contemporaneous 'cultures' co-existed at the Pleistocene/Holocene boundary; what is the evidence? Certainly some archeologists have referred to the diverse shapes and forms of fluted points throughout North America as one indication of a diversity of 'cultures', but this is a naive and insupportable point of view. If projectile points were always manufactured to be ideal art forms (according to normative standards or mental templates in the mind of the prehistoric makers), then variation and diversity can only be understood as reflecting changes in a culture's fashions or views of 'style' in finished artifacts. However, if diversity within and between assemblages depends not on 'stylistic' factors, but on technological factors such as raw material availability or other variables in the reduction sequence, then end-product variability is completely expectable. Why doesn't diversity in the manufacturing of Clovis points more likely reflect diversity in the technological repertory of Pleistocene/Holocene tool-stone-knappers, and not reflect different ancestries, different cultural 'norms', or different evolutionary histories?

Gruhn (1993: 477) has asked, 'By what possible conceptualization of human population dynamics can one explain the unquestioned presence of populations adapted by distinctive technologies to the lower Amazon Valley, and to Tierra del Fuego, within just a few centuries of a 12,000 yBP date set for the time of initial entry into the New World?' I do not know what the 'population dynamics' are that so irresistibly limit human adaptive flexibility, response time, migration speed, and technological knowledge; these dynamics are yet to be demonstrated, and therefore any 'conceptualization' of them seems reasonable when dealing with exploratory human dispersals into new ranges. I think what surprises archeologists such as Gruhn is the extreme linear distance that seems to have been traveled over a few centuries, and the local changes in artifact

types, styles, or technology. In Gruhn's view, it seems, material culture does not change in response to changing habitat requirements unless great spans of time are involved.

Gruhn also may be surprised that late Pleistocene foragers carried infants long distances on their migrations; but modern San in southern Africa, Inuit in northern Alaska, and other foraging peoples in between managed to move families on seasonal rounds, sometimes over long distances. Even if Clovis foraging families moved only a little bit every year, or waited for children to reach 10 years of age before moving much at all, such as on long-distance migratory treks, they still would have been able to cross enormous land areas in a lifetime. Migratory displacement of families and bands may have taken place in punctuated cycles (such as every decade or 20 years), or regularly throughout every person's lifetime. Dispersing adults leaving home bands could have made exploratory trips into new ranges hundreds of kilometers from home without taxing their abilities to recognize animal sign or follow animal trails to water, or without exhausting curated toolkits. New ranges could have been settled into for long periods, if they were promising enough, or occupied temporarily while dispersing family members explored farther territory. If the curated tool-stone eventually was used up, bone or wooden tools could have been substituted for some of the stone forms, until new stone sources were identified.

Much of the evidence about pre-Clovis-age sites in the Americas is open to question or alternate interpretation. I would point out that the chronometric record of unambiguous human occupation in the Amazon basin (Lynch 1990, Schmitz 1987) is still being argued over (see Roosevelt *et al.* 1996 and the responses that followed in *Science*). Whatever the first settlement interval was, it seems to have been characterized by a material culture that is as different from the ethnographically recorded rainforest culture as it is from Clovis. The modern hunter-gatherers use blowguns,

shotguns, and the bow and arrow, often sleep in hammocks, cultivate gardens for food, and exhibit other patterns (such as in storage, mobility, scheduling, and overall technology) as distinct from Clovis as they are from the first Amazonian patterns. So what does this prove about human adaptiveness? It certainly does not prove conclusively that the people who gave rise to Clovis culture in North America could not have dispersed into South American forests and changed their technology in just a few centuries; what it does suggest, on the other hand, is that there is a variety of different ways to adapt to the forests.

In this regard, we as Americanist archeologists should remember that populations of archaic *Homo sapiens*, while generally regarded as behaviorally different from modern populations, nevertheless colonized an extremely wide range of different environments in Eurasia, including glacial steppes and interglacial forests (Roebroeks *et al.* 1992), and an equally wide range of habitats in subtropical and tropical Africa (Klein 1994), and these distinct environments were pioneered using very similar toolkits. Stiner & Kuhn (1992: 330) discuss the independence of hunting success ('as measured by prey nutritional returns, age classes taken, etc.') and the 'increasing sophistication of formal weapons used by modern human hunters' in the context of contrasts between archaic and modern *H. sapiens* populations. Stiner & Kuhn (1992) argue that human behavior can be adjusted to overcome what archeologists see as shortcomings of technology, and that technology is not the perfect reflection of habitat-specific adaptations. In other words, hominids have succeeded in many different environments and under many different conditions, sometimes using identical technologies that appear simple, unspecialized, or even inadequate.

Over and above the arguments about the real or fervently hoped-for existence of neatly delineated colonies of distinctive cultures in the Americas around 11-12,000 yBP, another

argument about the nature of Pleistocene mosaic environments can be raised to address the issue of ecological barriers. This is the argument that the Americas were influenced by glacial climates to the extent that ecological zones were fully restructured, and that today's distinctly different ecozones did not exist at the time of Clovis.

Graham (1979) has surveyed the evidence about the 'nonanalogue' late Pleistocene environments of North America. He found that the migration of mammalian genera from Asia to North America was at its highest level during the Rancholabrean, when more than twice as many genera made the journey as during the Irvingtonian. These colonizations occurred mainly during the interval of grassland dominance, unlike earlier ones. Yet all types of habitat-limited genera crossed over in the Rancholabrean, from forest types to others, including a high percentage of carnivores; the implications of these facts are

- (1) that habitat type did not fully control late Pleistocene mammalian dispersals (suggesting that human cultures were not specifically pressured to be inflexibly adapted to grassland or steppe foraging); and
- (2) different habitat types existed in mosaics or successional stages throughout the late Pleistocene, increasing the foraging options available to humans in Beringia and beyond.

Rancholabrean environments were most conducive to open habitat-preferring mammals, but they also seemed to attract other types as well. If Clovis or Clovis predecessors had been openland oriented and big herd-animal oriented, they still would have encountered mosaic habitats, discouraging any obligate foraging patterns to develop. Social carnivores are always capable of adjusting to different prey and habitat conditions; technology helps humans cope with the tasks of hunting and gathering, but it does not dictate what can be hunted or gathered. Cold is also no barrier to social carnivores like humans; heat is no barrier, either.

The population centers and boundaries of each mammalian species in the late Pleistocene were not isomorphic. These species were not scattered along a gradient, except a very general one (extreme North, extreme South, extreme West, extreme East), but within the huge bulk of interior North America the species boundaries flexed and bulged and were permeable - individuals were continuously dispersing into new ranges through the boundaries. The zonal limits on species distributions were set only at the end of the Pleistocene. Guthrie (1990) makes this point eloquently: A 'recurrent faunal melody' can be heard all over the 'vast conglomeration of arid, grass-dominated communities' which he calls the Mammoth Steppe. During western Europe's cold periods (and even in the warm periods, to a lesser extent) can be found 'almost exactly the same species we find across eastern Europe', across Asia, and northern China in the south, all the way to the polar areas of the north (Guthrie 1990: 269). For the most part they also occur in Alaska and Yukon, with local or regional variants of course, due to differences in altitude, drainage, latitude, history, or stages of succession, but overall the communities are similar over an 'enormous expanse' of the world. South and east of this mammoth steppe mosaic in North America were biotic communities that differed in terms of average and extreme temperatures, and in species membership, but even these communities were not all that different from those of the mammoth steppe.

Harris (1985) examined the evidence about late Pleistocene paleoecology in the American West, and found some 'zonal' differences in vegetative communities, but for the most part these were segregated much more by altitude than by latitude. As an experiment I suggest envisioning three imaginary north to south transects through the United States, at longitudes 117, 112, and 107° West, and examining each of the paleontological assemblages that Harris has described from near the transects. Stratify the sample by

high, medium, and low altitude, using Harris' 'equator-equivalent' ratings. The results will show that no dramatic differences existed among locales at any longitude in terms of medium- and big-game species: in some locales *Camelops* dominates, and in other locales *Bison* or *Mammuthus* dominate the big-game finds, while deer, elk, antelope, and sheep are common medium-game species. The differences are predictable according to elevation; in other words, the differences correlate with altitude, not with longitude or latitude. I would propose that a late Pleistocene hunter in any longitude and latitude of Beringia and the American West would be competently prepared to be a successful hunter in any other latitude and longitude, a proposition also made by Kelly & Todd (1988) but without the discussion of paleontological facts to warrant it.

Moving eastward, the situation is not quite so similar over huge areas of land. Graham *et al.* (1987) provide descriptions of prairie and plains paleoecology, and once again you can construct several imaginary transects through this central American region to examine differences and similarities in large-mammal availability. Wendlund *et al.* (1987) provide information about Illinois and Missouri climates and vegetation: the mammalian associations at all times were mixtures of wide-spread taxa, steppe and boreal elements, and deciduous types. The deciduous taxa outnumber the others during one interval, 16-13,000 yBP. Boreal and steppe elements were always a minority, but also always present throughout the later Pleistocene. In Iowa boreal and deciduous elements were in the majority most of the time, but all other element types were present at all times after the Late Glacial. Your Southwest Plains transect will have similar observations, as will the Central Plains transect, and the Northern Plains transect. One may conclude that while central-continent habitats were certainly not identical with those of the west and Beringia, they nevertheless shared important characteristics.

They supported big-game species such as mammoth, as well as medium-sized game such as deer and elk. They supported plant communities that were mosaics.

If the game-animals were not so different as to present barriers to a rapid Clovis movement, then what is different about the biotic communities on the transects? The proportions of different plant elements in the mosaics differ, hence the amount of woody cover in any region would be expected to differ. The average temperature and seasonality of separated regions possibly differed to a nonnegligible degree, as did stream densities or numbers of streams and watersources expected to be encountered per kilometer traveled by a migrating human population. Temporary shelters require poles and brush to build, and wood is needed for making spear shafts, tool handles, digging sticks, bowls, spear throwers, and so forth, so a scarcity of woody vegetation in any locality may be considered a drawback, but certainly not a barrier, since mosaic habitats did contain woody taxa in varying degrees. Woody plants also serve to provide chemical compounds for leather-tanning, but leather does not wear out quickly, and I expect that foraging people would have been able to find new tannin sources, new pole sources, and new sources of wood for other uses long before the people suffered any consequences that slowed them down in their exploration, foraging, or reproduction. Mosaic habitats also would be expected to provide medicinal plants, nuts and berries, roots and fruits, and dye sources, although not in the same proportions or with the same ease of extraction from one region to another. Water availability would have been impacted by seasonal temperatures, but frozen water is not inaccessible to people who can create fire. And if there are any game animals living in any region where foragers hope to succeed, the people would know how to trail the animals to water sources, and they would know that the scarcity of game sign means scarcity of food or water. People would not have entered deep-

ly into inhospitable country without mapping out escape routes, or without first evaluating the country's seasonal and long-term potential.

It seems that late Pleistocene North America offered few to no barriers to a mobile foraging culture that was determined to explore or inclined to disperse into uninhabited country. Archeologists cannot proclaim that foragers would never wish to explore or disperse so actively, because there are no ethnographic examples from which to draw analogical models and there are no general anthropological/biological theories denying human beings the ability to irrupt, disperse, or migrate as 'fugitive' populations.

What sets Clovis apart from ethnographic foragers is that it seems to have been unusually mobile. D. Anderson (1990: 172) points out that there are Clovis concentration centers, and areas around them relatively empty of the Clovis presence in the American east. He also points out that raw materials in the Clovis assemblages came from up to 250 km away, or more, both in the western and eastern American sites. The large sites are found around some of the major stone quarries, perhaps having been aggregation loci, as were other sites situated on prominent landscape features. Maybe these sites were rendez-vous loci, where mobile foragers returned cyclically. Maybe they were central camps from which groups fissioned to search scattered resources; the search costs were high, but the exploratory returns would have been higher. Major and minor drainages have large sites, so Clovis people made decisions based on more than just resource volume, scale of landscape features, or other strictly environmentally-determined variables. Post-Clovis shows clear packing into the emptier areas, but along with the filling-in of geographic space comes an indisputable continuity in flintknapping technology (but with an increasing regionalism developing). Anderson (1990) co-predicts scattered patchiness of resources and logistical resource use in the

Clovis interval. I do not fully agree with this, because I see Clovis foragers as preferring heterogeneous environments where they were residentially highly mobile, but their mobility was of the circulating kind, where certain points in the landscape were returned to after long periods of time, and the overall sizes of lifetime territories were enormous.

Binford (1990) examined mobility, transport technology, house construction, environment, and a number of other variables among hunter-gatherers, and his predictions may be applicable to Clovis foragers. Clovis did not seem to possess transport technology, yet it was a fully nomadic culture possessing no substantial houses, and a preference for moving consumers to resources (such as mammoth carcasses and stone quarries) instead of logistically collecting resources to transport back to consumers. According to Binford's (1990) schema, Clovis may have been making 10-30 moves a year (with or without infants carried by women and older children). The people spent little effort in plant exploitation (whether or not this is a preservational problem is unsolved); and Clovis cultures may have felt comfortable in ranges that were up to 2-5,000 km² in area. Unique to Clovis may have been its high dependency on terrestrial mammals in regions where biomes were not floristically productive; in these regions the search times and energy expense were high, but all resources would have been searched at the same time, so exploratory returns were worth the price. There would have been positive feedback on high mobility, which encouraged farther exploration and dispersal. Meat could have been stored as an overwintering buffer in times when travel and exploration were riskiest. Facilities for caching meat, such as racks, pits, or protected areas, should be associated with Clovis sites, although meat can also be well-preserved by merely allowing it to freeze (Frison & Todd 1986) or by drying it. Perhaps the underutilized mammoth skeletons at many American sites (see discussion

in G. Haynes 1991) were simply storage as well as processing features.

PART 2: MAMMOTH BEHAVIOR, HUMAN STRATEGIES, AND ARCHEOLOGICAL SIGNATURES

WHAT WERE MAMMOTH DIE-OFFS LIKE?

Most authorities prefer to take an Olympian view of Clovis dispersal. But abstract ideas about mammoth die-offs must be placed into the more earthbound contexts of rapid Clovis dispersal and high group mobility. In this section I discuss several 'principles' of human and proboscidean behavior and biology. I hope to show that some of the characteristics of Clovis settlement, technology, subsistence, and social organization may have been conditioned by the behavior of mammoths, and that Clovis foraging decisions can indeed be seen as 'optimal' when viewed as reactions to mammoth die-off events.

Five principles are relevant here. First, following Winterhalder (1981) and other foraging theorists, I propose that foragers with a large hunting component in their energy procurement systems will not violate the Marginal Value Theorem of evolutionary ecology; that is, they will decide to skim animal populations for meat, but will deliberately leave a reservoir or remnant breeding stock. The animals will not be hunted to extinction. Or rather, the animals will not be hunted to extinction unless the cost of stopping their hunting outweighs the benefits from it. Second, hunters who are dispersing into unoccupied or unfamiliar ranges will (through trial and error processes) settle into 'best-return' habitats first (O'Connell & Hawkes 1981).

These are not controversial propositions, but I raise them first because they may seem irreconcilable with my earlier arguments that Clovis hunters dispersed widely and participated in the rapid killing-off of mammoths (and other megafauna). I propose to recon-

cile my suggestions about Clovis exploitations of mammoth die-offs (and its result, overkill) with these principles by referring to three other generalizations (which may qualify as principles, as well). The third principle of interest is that during climate-caused proboscidean die-offs, when elephants seek resource refuges, the resultant aggregating of large numbers of animals into patches of limited forage, water, and cover creates an ecologically stressed situation. In die-off refuges of modern African elephants, there is a serious decline in local biotic diversity, starting with the destruction of many soil microinvertebrates, and also including the removal of specific plant taxa and perhaps whole communities. Mammoths, like elephants, would have trampled to death worms and insects; they also would have compacted soils around their resource refuges, and decreased the biomass of large tracts of land. Runoff would have increased at the expense of soil-soaking during rain or snow, and erosion would have greatly intensified, severely shrinking groundcover biomass. These processes can be seen today in overcrowded African elephant ranges. Furthermore, the mammoths would have hammered vegetation in the refuges, killed grasses by their destructive feeding methods, which is tearing the grass plants out of the ground, and pushed over trees or broke them down to reach top leaves.

While these processes were occurring, the mammoths would have been creating a network of accentuated signs of their presence, density, and relative health, readable by any group of foragers with experience in sharing ranges with proboscideans. Elephant trails are abundant in their ranges in Africa, but in stress seasons some of the trails become much more heavily used. The trails are unmistakable that lead to water or forage refuges such as patches of evergreen trees in the dry season or patches of trees with edible seed pods in the winter. Heavy trail use results in a much thicker trail-covering by trampled

dung; the dung's freshness and contents can be understood by hunters to reflect seasonal diet and seasonal animal densities. The trails are not uniformly distributed, of course. They lead to and from preferred feeding patches and relict water sources; they also lead to and from refuges. But their presence and distribution would be detectable to competent human foragers or dispersers, who would then understand where to go to find mammoths tethered to resource points such as water sources or vegetational patches, and how easy the mammoths may be to kill. Human foragers would know that increased returns from mammoth-hunting await them in the refuges. Human foragers who share ranges with mammoths would also know that die-offs in refuges were predictable, although nonuniformly distributed in time and space. Hence, human foragers would not specialize in exploiting mammoth refuges during stress periods, but they would be ready to exploit die-offs whenever the signs were recognized.

A fourth principle is that hunters who utilize patchy environments will decide to proceed directly to the patches where they expect to find higher-return resources. Marks (1994) has documented this in modern hunting by rural Africans, and it is congruent with foraging theory and ethnographic observations (see Winterhalder 1981 for examples). Therefore, I expect that Clovis hunter-gatherers would have foraged for a variety of resources in a variety of patches wherever they happened to be in North America, but that at the moment they detected the physical signs of mammoth stress they would have proceeded to follow the game trails to the promising sites where die-offs were expected. A fifth principle is that hunters normally expect their success rates will be low with big game animals, based on prior experience and a cautious appraisal of the effort needed to chase and kill the largest game species. They will thus decide to first target the high-vulnerability animals out of a population (Marks 1994). In an elephant die-off situ-

ation, not all animals are weakened to the same degree; young animals and older females are the most vulnerable. Adult males may be vulnerable due to their being alone, but generally they travel farther between water and food sources than females with young, and hence are more difficult to locate and ambush, and harder to kill.

Under these conditions, with Clovis foragers exploiting seasonal or cyclic mammoth die-offs, it is possible that the Marginal Value Theorem would no longer be operable in the minds of the foragers. As Clovis people hunted and killed mammoths in their refuges (or scavenged the dead), and as mammoth densities diminished during the stress period, there still would have been no increase in the foragers' search time for mammoths (since the surviving animals continued to stay in the refuges where animal densities were high and stayed higher than outside the refuge even after die-offs occurred), and there would have been no decline in the return rates from mammoth-hunting. The patch full of mammoths would not have been abandoned (see Kelly 1995: 143). Perhaps when the last mammoths died (probably the larger bulls), Clovis foragers were already planning their next dispersal move to other patches, or may have finally abandoned the refuge patches by then.

WHY THE DIFFERENCES BETWEEN CLOVIS MAMMOTH SITES AND THOSE IN EURASIA?

The physical differences between the Clovis mass-mammoth sites and the large Eurasian sites were mentioned above. Here I am concerned with proposing some sociocultural differences that may help to explain the physical differences. Ethnographic analogy isn't going to work well when dealing with mammoth-hunting, because the only big-game specialists who left sites full of prey bones are marine-mammal hunters. Whales do not appear to die-off in refuges, so Eskimo foraging decisions about where to exploit whales

or how opportunistic to be may not say much about possible mammoth-hunting-cultures' decisions.

Since there are few or no analogies to call upon that deal with megamammal hunting, instead I will call upon a recent model of hunter-gatherer social behavior and its relationship with technology, mobility, and site structure. One very pertinent model was formulated by Lyn Wadley (1988) to explain artifact assemblages in southern African Late Stone Age sites, and is based on 'bushman' behavior. I emphasize that I do not mean to apply this model to late Pleistocene Clovis and Eurasian assemblages as a convenient blanket explanation; rather, I hope to point out some organizational ideas demonstrated among hunter-gatherers that may also apply to the Clovis and Eurasian cases.

Certain foragers display aggregation-phases and dispersal-phases in their activities. That is, at some times (of the year, perhaps, or over longer time cycles such as decades or lifetimes), foragers aggregate into larger band-sizes. The cause of the aggregation may be to allow for participation in ceremonies and rituals, or in response to seasonal changes in resource distribution and density. At other times, the foragers disperse into smaller bands. Site sizes differ between phases, as do assemblages and features. Aggregation sites are expected to have a wider range of tool types, a greater proportion of 'formal' tools (reflecting more formal behavior by people in the sites, resulting from the social articulation of different intra-band and inter-band groups), more variability in tool-types across space in the sites (reflecting more rationalization of space and an emphasis on formality), more ornamental artifacts and 'craftwork' (reflecting more gift manufacture and exchange, or increased ritual), and sometimes a relatively lower artifact density per sediment unit volume (as a result of the separation of activities into different spatial areas). Often the people within aggregation sites decide to for-

ge for food resources by using strategies very different from those employed when in dispersal phases, motivated by the desire either to increase the provisioning needed by a larger group, or to provide resources that could not be successfully procured by smaller foraging units. In other words, different hunting techniques, different patch choices, and different quantities of game meat and body parts brought back to camps may characterize the aggregation-phase gatherings.

I suggest that mass-hunting of mammoths, repeated perhaps several times while people were camped in the same aggregation-phase sites, characterized the late Pleistocene cultures adapted to Glacial Maximum conditions in Eurasia; the same range of basic adaptations (foraging in a megafauna-rich steppe environment) also inspired New World dispersers to explore widely and seek mammoths to hunt en masse. But Clovis mass-hunts differed from those in Eurasia because they were done by smaller human populations; in effect, the Clovis cultural groups were in a semi-permanent dispersal phase, and their sites are smaller, less formal in layout and feature contents, and contain few (or no) ornamental artifacts.

Wadley's (1988) systematic generalizations refer to the aggregation and dispersal phases of groups and subgroups that are members of one and the same sociocultural unit. I realize that stretching the culture concept to define Clovis and Eurasian foragers as members of the same sociocultural unit is not very satisfactory. I am not trying to define all mammoth-hunting cultural groups as one; I am trying to explore the possibility that mammoth-hunting people who existed during late Glacial times shared some significant trademarks and made decisions about foraging based on similar ideas about success rates, survival, and opportunism. As shown above, the scores that could be assigned to the subsample of Clovis and Eurasian sites based on weathering, gnaw-damage, age-profiles, and bone representation are different in important respects. The sites of

Mezin and Mezhirich may be aggregation camps of people who cyclically hunted mammoths in large and small groups; the site inventories contain both ornaments and workaday tools. Some sites such as Krakow-Spadzista Street have several small camps situated around large mammoth-bone deposits, a heterogeneity that seems more formal than the 'typical' Clovis mass-mammoth sites.

The existence of Clovis aggregation sites is not ruled out here; in fact, there are big clustered locales where Clovis assemblages are abundant in the eastern and western United States (D. Anderson 1990). These sites may not contain evidence of different hunting strategies simply because of unfavorable preservational factors; they also do not afford much evidence about formal behavior or ritual activities, perhaps also because of poor preservational factors. Group size for Clovis aggregation sites may have been exceptionally large, as it may have been for the Eurasian mammoth-hunters - especially if the post-sharing return rates among all the foragers of the group were highly correlated (that is, everyone did as well as everyone else; see the discussion in Kelly 1995: 212-214).

I propose that evenly distributed mammoth populations and a stable availability of them (or perhaps other large game taxa) would have led late Pleistocene foragers (who preferred to specialize in big game) to live mainly in small, dispersed settlements; but once in a while the existence of clumped and predictable concentrations of mammoths under stress and seeking refuges may have led late Pleistocene humans to aggregate in large, longer-term central settlements. The Eurasian aggregation sites exceed the Clovis ones in size and complexity, simply because Clovis 'aggregations' were relatively smaller-scale and shorter-term.

CONCLUSION

Prehistorians will be arguing about the very first people who colonized the Americas long after we are dead and buried. Unfortunately,

opinions may carry more weight these days than facts, since the facts can be misunderstood, or they are scarce. There are three forms of belief that figure in these arguments: (1) hypotheses are the preferred form because they can be tested; (2) paradigms, rarely outlined clearly, are scarcely 'provable' or 'falsifiable'; (3) ideology, which motivates us to continue arguing, is not testable because of its highly abstracted nature. Too often an argument deteriorates into ideological and doctrinaire confrontations when we argue only by assertion, speculate too wildly, or make propositions that transform themselves into gospel truths. We also may see our own ideas reinforced by others' arguments, even when the ideas clash. This problem was called 'introspectosis' by the humorist Stephen Leacock (1926), and it seems to afflict writers when they make claims that a consensus of acceptance exists about pre-Clovis migrations (see examples in Lewin 1989, Morell 1989). The more adroit archeologists can reason in print in ways that apply intellectual brakes to the speed of the first migrations, or they can find one new 'barrier' after another to a rapid spread. Rhetorical wars sputter and flare as one faction militantly publicizes yet another of the earlier and earlier sites, and the other side insults the opponents as a rhetorical device. There is no knockdown argument for either side that will settle the question. Rapid migration, according to one author, was 'quite impossible' (N. Guidon, as quoted in D. Meltzer 1993: 76), while another group of authors term it 'extraordinarily implausible' (Rogers *et al.* 1992: 284). Yet rapid Clovis migration, in my view, is a 'fait accompli'.

To the rest of the world's prehistorians, American archeologists probably seem inordinately boastful about their 15 (or maybe more) sites that contain mammoth/mastodont bones and evidence of human killing or butchering behavior. But these are important sites, and their use as evidence in the never-ending war of words over the tempo of American colonization by stone-age humans

demands that their meanings must be made clearer and less readily misapprehended, as they seem to have been so far. I have examined some arguments about the proposed speed of human migration and the possibility of a consequential process of megamammal 'overkill', specifically of mammoths. It seems very logical to conclude that human migration could have been surprisingly rapid, and that humans did have a hand in the extinctions.

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