LATE PLEISTOCENE CULTURAL AND TECHNOLOGICAL DIVERSITY OF BERINGIA: A VIEW FROM DOWN UNDER

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My fellow commentator Don Dumond asks the interesting question of whether the Beringian evidence provides any insight into the original peopling of America. I do not have an interesting answer. But from my vantage point down under (that is, from mid-latitude North America), I do have some thoughts, hypotheses, and even a few utter speculations to offer. I will explore these (without necessarily revealing which are which), beginning, as seems appropriate, in Siberia, and working my way east.

Vasil'ev and Slobodin's opening papers on Northeast Asia and western Beringia, although rather dissimilar in theoretical approach and geographic coverage (the latter due, of course, to editorial decree), provide up-to-date summaries of the archaeological record in areas that may have been departure points for groups that ultimately headed to the New World. Yet, while Vasil'ev assembles some 55 sites west of the Lena River and Verhoturie-Mountains, Slobodin reports that there are only half that number from far northeastern Siberia—western Beringia. That difference is significant, and surely limits what can be said about the sites and assemblages associated with the Asian groups who would become the first Americans.

Indeed, as is apparent from these papers, we know more about what the ancestral complexes did not look like, than what they did look like (not that we even have a good a priori idea of what an antecedent assemblage ought to look like, as Bever points out in this volume). The earliest human presence in the region does not appear, as Slobodin argues, to be a "pre-projectile" pebble tool industry (shades of Alex Krieger, 1964!). The early dates simply are not there, the complex is not well defined, and it appears to have little historical integrity or coherence (Slobodin's point, but one echoed by Vasil'ev in this volume). Indeed, these "complexes" may simply reflect the availability of lithic raw material and/or use patterns. It would be useful to tally the size (amount) and diversity of lithic raw material in these assemblages, and determine whether bipolar cores are present; such would help ascertain whether these pebble-dominated assemblages are merely the consequence of limited stone availability, and not historically meaningful archaeological complexes.

Slobodin is not much more enamored of the idea that non-microblade assemblages (said to follow the Pebble Tool complex and precede the microcore/microblade assemblages) are archaeologically representative of source populations for the Americas. While some of these (e.g., Ushki Lake Level VII), are reasonably well dated to the appropriate time period, they are distinctive more for what they lack (microblades), than for what they have in common with the earliest American assemblages (see also, Ackerman, Hoffecker, and Holmes, this volume). Stemmed points are occasionally found in these assemblages, but can be matched in too many places and times to be a useful link, and the fluted biface from Uptar (King and Slobodin 1996) is and remains a puzzle. That it has a purposeful, flute-like flake removal is not in doubt; what the presence of that single, particular feature means, and whether it has historical significance, is quite another matter.

The best dated and most abundant assemblages in far Northeast Asia are dominated by microcores and microblades (Slobodin, this volume)—rather in contrast to the archaeological assemblages to the west and south (in the Yenisei and
Altai regions), which may occasionally include microcores and microblades, but in varying frequencies, and sometimes not at all (Vasil’ev, this volume). In a recent paper, Brantingham et al. (2001) show that the maximum ages of this particular class of artifacts are virtually the same in both southern and northern Siberian sites, suggesting this class moved rapidly across space, from south to north. If (and it is a big “if”) that movement marks the dispersal of individuals (as opposed to the movement of a new and valuable technology across an extant population), then it suggests the tool makers moved rapidly into and across Beringia. Leaving aside for the moment the matter of how much weight should be allowed to rest on these as “indicator” taxa or diagnostic forms, this class of tools arrives in the far northeast very late in the Pleistocene sequence. Indeed, too late and too distinct, it would appear, to have any bearing on the initial peopling of the Americas south of the ice sheets, as Nels Nelson recognized long ago (Bever, this volume). At least not directly.

Yet, this material may bear on the matter indirectly—or at least on the way in which we think about the methodological question of how we know an early migration when we see one. Consider this: of the sites tallied by Vasil’ev, all but a handful are located south of 60° latitude, while virtually all the sites discussed by Slobodin are found north of that parallel. Moreover, as Slobodin shows, the majority of the sites north of 60° are dominated by wedge-shaped cores and microblades, which of course are almost all latest Pleistocene in age (i.e., younger than ~15,000 years B.P.)

While “non-microblade” sites and assemblages (and I use that term in the broadest sense, and not in the more specific sense used by Slobodin) are present in these northernmost areas, few are securely dated—let alone to earlier times.

On its face, this pattern reinforces the suggestion, hardly new, that human groups were late arrivals in this far corner of Asia. This is as one would expect, given the challenge of adapting to this uncompromising climate and landscape. Still, they also raise a cautionary flag since, again, these particular assemblages appear to have little, if anything, in common with the initial assemblages of the Americas south of the ice sheets (and that includes Clovis) (Bever, this volume). What may appear to be the earliest traces in an area (assemblages dominated by microcores and microblades) might only mark the first archaeologically visible populations on the landscape. The same might well be true of the Clovis and Clovis-like complexes of mid-latitude North America. Archaeological visibility is dependent on many factors, not least being the size of the population and the nature of its activities on the landscape. If human groups en route to the New World came through northeastern Asia at an earlier time—and there surely was a group that came through at an earlier time—and if they were traveling in relatively small numbers and moving rapidly (and not using the landscape in a redundant fashion), then evidence of their passing will likely be scarce, hard to see, and perhaps difficult to recognize (at least initially).

But just how much should we rely on apparent diagnostic classes, like microcores and microblades, or typological categories or attributes (stemmed points, bifacial points, etc.), to trace a migration, define a group or cultural tradition, infer a population dispersal, or even draw long-distance linkages between archaeological complexes and continents? Vasil’ev, Sellet, and Bever (this volume) all argue, rightly I think, that too much reliance is placed on too few traits or classes—such as the presence/absence of bifacial foliated points and knives in the Upper Paleolithic, or Paleoindian projectile point types—in the development of these historical or chronological schemes. It is hard to disagree. Indeed, while Vasil’ev politely suggests otherwise, candor compels the admission—as Sellet clearly demonstrates—that in North America we have placed inordinate weight on a very small part of the archaeological record. The search for Clovis origins and dispersal centers on just a very few traits—notably fluting (the vigorous search for which, as Bever notes, has worried Alaskan archaeology for a half century). And our Paleoindian sequences, implicitly taken to mark cultural and adaptive changes, are based on chronologically overlapping point typologies, which may have little bearing or relevance to on-the-ground groups, dispersals, or adaptive change. To be sure, at a sufficiently large scale (in time and space), such types or classes are historically meaningful, but as Sellet, Vasil’ev, Bever and others (e.g., Meltzer 2002) stress, that is often not the scale of interest.

The lesson here is that the past does not come to us in well-integrated, coherent boxes of material culture, possessing sharp space/time boundaries and well marked by changes that occur simultaneously across all elements of the assemblages or in all attributes of artifacts (i.e., stylistic, functional, and technological), so that looking at one element or attribute can reliably provide insight on changes in all the others. Moreover, aspects of material culture change at very different rates and times and may, in fact, be quite independent of on-the-ground changes in the population (e.g., social, linguistic, or genetic change). “Groups” or “dispersals” can be tracked archaeologically in many different ways, depending on which class(es) of material culture one selects as a marker, but that in turn can be biased by sample size and representativeness, the way in which that particular class moved or was distributed in space and time, the functional activities undertaken at a site or represented in an assemblage, and so on.
Vasil’ev and Ackerman point out, and Bever and Holmes explore in more detail, this volume). This is not to say that tracking movements of groups archaeologically is impossible; only that it is critical that one select attributes or objects which can reliably inform on the process.

Seltet and Bever are quite correct in pointing out that by themselves ill-defined point types are not up to the task; these are too often a concatenation of style, function, and technology, which is why there is often more variability within the types than between them. That “classic” types such as Agate Basin and Folsom prove chronologically problematic in the Late Pleistocene on the northern High Plains (Seltet, this volume), or the fact that we are still puzzling over the relationships (chronological, technological, historical) among forms like Goshen and Plainview, strongly suggests that we should pause before using those specific types to draw long distance affiliations between types from the northern High Plains (or the even more distant southern High Plains), and ones on the North Slope (e.g., Mesa points, see Mann et al., this volume) or in the interior of Alaska (“Plainview” or “Angostura”-like lanceolates, see Hoffecker, this volume).

Arguably, the optimal approach would be to use stylistic attributes, which can be diagnostic of units in space and characteristic of units of time (the basis of seriation), as presumably the shared forms (styles or homologous traits) were made by some coherent social lineage or group. Such an approach can work as long as the traits are solely stylistic, and not conditioned by selection or other evolutionary mechanisms (Lipo et al. 1997). Precisely what those stylistic units might represent in terms of an on-the-ground social or linguistic group, or deme (population terms), I am not willing to say, largely because such a conclusion depends on several variables such as: how styles are defined (a process that can be quite arbitrary), at what spatial/temporal scale, types of controls for effects of sample size, and so on (Lipo et al. 1997:326–327). Even with all these elements pinned down, it is difficult to say what such stylistic units might represent, as style in material culture can easily cross-cut ethnographically recognized units (see papers in Stark 1998).

When dealing with Late Pleistocene groups, whose tool kits have so little archaeologically visible stylistic signaling within them, the best we might hope for is evidence for some general time/space patterns in technology and function, along the lines discussed by Bever. Nevertheless, we still may not be able to link these to group lineages, migrations of people, bursts in population density, or the like. After all, the task is formidable even under conditions far more conducive to this sort of analysis (Lipo et al. 1997:327).

Yet, I must hedge a bit here, because I suspect that under certain spatial and temporal conditions, we may be able to argue that the patterns or trends we spot in the Late Pleistocene record—in stylistic and functional attributes of assemblages—may represent coherent groups or lineages, even if we cannot say who or what those might be. Vasil’ev observes that there appears to be considerable homogeneity in Siberian toolkits across a large expanse of space and time (though I cannot shake the feeling he is emphasizing homogeneity—lumping—a bit more than might be prudent). Ackerman and, especially, Bever suggest the same for the Mesa assemblages, though at a smaller geographic scale and while explicitly denying that this complex is merely part of a larger pan-Beringian tradition. While I have few quibbles about how homogeneity (and the flip side, diversity) is being defined or measured in regard to Vasil’ev’s Siberian assemblages, or how much cultural significance one should ascribe to differences in technologies (the latter is a concern of Ackerman and Bever as well), given the size of the areas and time periods under discussion, those misgivings are not of great consequence. In fact, I rather suspect (expect) that the material culture of Late Pleistocene populations moving across vast expanses of territory over long periods of time should produce such a pattern.

After all, aside from the need to maintain energetic return rates on an arctic landscape, groups living under those circumstances were faced with the challenge of reducing environmental uncertainty. Such groups had to minimize risk in space and time by exploring the landscape and mapping new resources, and by maintaining contact between dispersed groups, in order to maintain the flow of information and sustain demographic viability while living in relatively small numbers spread very thinly over large areas. Among the strategies for coping with such challenges were long-distance mobility, periodic aggregations, and large and more open social systems—precisely the kinds of adaptive strategies that would promote homogeneity in material culture at this scale (Meltzer 2002). There will be variability at finer spatial and temporal scales, and some measure of cultural “drift” (which also may be why, for example, there is such variability in fluted points across the Americas—even excluding Folsom). Overall, however, one would not expect highly distinct assemblages from such conditions of expansion or dispersal. That comes later in the sequence, when environmental uncertainty is reduced, populations expand, mobility drops off, and more regionally based adaptive strategies are developed. Or so I have argued (Meltzer 2002). But far more attention needs to be paid to the movement of raw materials, to site structure, detailed studies of stylistic variability, and the like, in order to test this (admittedly) accommodative argument.
Issues of assemblage variability and consistency are addressed in the papers on the Mesa complex sites, by Ackerman and Bever (this volume). Ackerman provides a detailed and useful overview of the Spein Mountain site complex, an apparent “cultural isolate,” which appears to be part of the same techno-complex as the Mesa material and sites 900 km distant on flanks of the North Slope. I find the spatial dispersion of this complex interesting, and wonder whether its apparent absence from central Alaska is real, or whether it could be an artifact of sampling strategies and site discovery. Ackerman and Bever certainly hold the latter open as possibility, and time will tell. But if the pattern holds, that might say some very interesting things about the distribution of other human populations and the emergence of style zones and possibly territoriality in Alaska around 10,000 B.P.

It is perhaps appropriate to add that, like Bever, I am more inclined to date the Mesa occupation to 10,000 B.P., and assume the two >11,000 B.P. dates from the type site are erroneous. Having been puzzled by those outlying dates since their initial publication (Kunz and Reanier 1994), I note with some interest the observation (Mann et al., this volume) that Pleistocene wood is found even today along the rivers of the North Slope. I respect their holding open the possibility there was an occupation at the Mesa site at 11,000 B.P., however, I have difficulty imagining why two purported Paleoindian occupations, separated by a millennium of harsh Younger Dryas climates, would be completely indistinguishable (of which, more below). In addition the evidence of old wood along the Ipiikpuk River, together with the absence (among the nearly 50 dates now available from the site), of any additional radiocarbon ages from Mesa older than 11,000 B.P., makes the possibility of an occupation at that date seem increasingly less likely.4

Bever shows that the various sites in the Mesa complex (and the various activity areas within the Mesa site itself) are highly homogeneous technologically, though he sees slightly different expressions of the organizational system at each, while the complexes as a group are distinct from contemporaneous Alaskan assemblages (a point on which Holmes [this volume] disagrees; while I am sympathetic to Holmes’ fight against undue splitting of the archaeological record, lumping Mesa in as a regional variant of Denali strikes me as going a bit too far). It is evident, too, that we are starting to discern variability in lithic technology and reduction strategies at these sites, despite the overall similarity in the activities that took place. It would be of considerable interest to see what an extended Mesa complex camp or habitation site might look like.

I would venture to speculate that the strong similarity among the Mesa assemblages (and here a closer look at the style-rich points might be beneficial), is likely a by-product of a wide-ranging, rapidly-moving group, whose activities across the far north were evidently geared toward hunting. What they were hunting, and under what environmental conditions is not so evident. Bever observes that the distribution of the sites coincides with the present tundra zone, which he readily admits is something of a moot point since, as Mann et al. show, tundra was not present or at least was not as widespread in this area at that time. The key for Bever, however, is the fact that the “forces which drive the modern geographical arrangement of tundra and forest were likely in place.”

However, it is evident from Mann et al. that it was not “modern forces,” per se, but instead a Late Pleistocene pattern without modern analogue: a period of rapid and repeated changes in climate, which were sufficiently dynamic, unstable, and harsh that in these open landscapes successional species—notably, grasses—would have been abundant, which in turn would have enabled bison to flourish at intervals. Certainly, the Mesa tool kit was sufficient to the task of hunting bison (Bever, this volume). And if these groups were, as has been suggested, a “backwash” from the south, perhaps they were tracking similar species—bison—northward up Guthrie’s (1990) Great Bison Belt. If, that is, it was a backwash—a matter still unresolved, and about which there remains disagreement (compare Holmes and Mann et al., this volume). It certainly would be helpful were sites with similar assemblages to be found in the “backwash” zone between the northern Plains and the North Slope (see also Wilson and Burns 1999:228–230).

Of course, there is the matter of timing. Given the rapid scale of the climatic changes envisioned by Mann et al. and the apparently brief periods when the landscape was covered by grassland, the question of whether bison were the primary prey resolves itself in a chronological correlation between one of these grassland “spikes” and a pulse of cultural activity at Mesa, Putu, Bedwell, etc. (direct evidence of bison remains in a Mesa-like site only occurring at Engistciak, 500 km distant and several centuries later than the Mesa sites flanking the Brooks Range). Unfortunately, they cannot readily make that correlation, at least not to the degree demanded by the hypothesis that Mesa groups were hunting bison. This is so largely because, as they expressly discuss, this may be the most complex segment of the Late Pleistocene radiocarbon curve, unresolvable chronologically and at a scale of resolution finer than what may have been centuries.5 Thus, while I find their hypothesis about the adaptive strategies of Mesa groups compelling, and especially the climatic and ecological reconstruction behind it, it lacks that critical linkage (chronological and/or archaeological) between human predator and bison prey. So far, anyway.
What already seems clear, however, is that there was little (if any) occupation by Mesa groups on the North Slope during the Younger Dryas, which is almost surely a consequence of harsh conditions during this climatic interval. And, with the onset of more stable and very different Holocene climate and environments (notably the rise of tussock tundra vegetation), Mesa groups effectively disappear from archaeological sight. Their disappearance does not necessarily imply they were evolutionarily unsuccessful, a possibility raised by Holmes; they may simply have abandoned this particular area.

Interestingly, a different pattern is described by Bigelow and Powers for interior Alaska (and suggested by Hoffecker, this volume). Even granting that they use slightly different chronologies for the Younger Dryas (compare, for example, Bigelow and Powers’ bracketing dates with the ones used by Mann et al.), Bigelow and Powers see different periods of human presence on the landscape of the interior. For them, there was no large scale population shift (abandonment) in response to the Younger Dryas, and human occupations continued further into the Holocene. One might well expect, of course, a difference in the Late Pleistocene and Early Holocene histories of human land use in the interior as opposed to the North Slope. These are very different habitats today, and were then too. The archaeological records of the area certainly bear this difference out (with cautions, as discussed below). But before concluding there was a significant contrast in the way in which humans responded to, say, the Younger Dryas, or other major climate shifts in these areas, a few cautions are in order.

I heartily agree with Bigelow and Powers that paleoecological data need not be so coarse as to be of no use to archaeologists. Yet, I wonder if, in their effort to prove the point, they undermine their own cause. Bigelow and Powers clearly control a great deal of valuable and relatively fine-scale paleoecological and archaeological data. But instead of teasing out the details of fine-scale climatic and ecological change and, in so far as possible, tying that to specific well-dated archaeological sites and activities, they instead merge their paleoecological data in 500 year blocks to create “time-averaged” slices of environment at 1000 year intervals. They then try to link those averages with human adaptive responses—over a comparably broad time period. To my mind, demonstrating that archaeological patterns were a response to particular climatic and ecological conditions, requires demonstrating that those environmental conditions existed locally, then directly linking their effects to specific human responses detected archaeologically (and not just the presence or absence of archaeological sites, or the appearance of particular fauna). That is no easy task, because of the scale mismatch between the archaeological and paleoecological records on the one hand, and the on-the-ground adaptive activities of hunter-gatherers on the other.

Hunter-gatherers do not respond to 500-year averages. They respond to the local weather—primarily on a daily and weekly basis, but also as it varies seasonally and inter-annually. They respond as well (but much more slowly) to changes that may occur over the course of a lifetime. And how or whether they respond depends on whether those changes triggered fluctuations in prey populations, reduced surface water, or otherwise caused detectable changes on the landscape that directly impacted foraging activities. Long-term patterns of very low frequency climatic variation may not be detectable at all as they play out over many generations and hundreds, if not thousands, of years.

Even when more rapid environmental changes were evident to foragers, we might not have the archaeological resolution to see their responses. Archaeological sites are only rarely snapshots of real-time events; more often, they are palimpsests of separate events, perhaps accumulating over weeks, months, decades, or even centuries. Worse, the radiocarbon resolution of the age of a site is usually no better than a century (and may be worse if one uses two standard deviations for its age range). Thus, humans might not have seen or experienced those “average” environments, and often we do not know just how much of their environment they noticed, or is represented in a site record.

Merely noting that the environment—writ large in space and time—looked a certain way (warming with increased effective moisture, as per one of their examples), and that the archaeological sites within that long period have certain patterns (e.g. sites are present), does not a causal relationship make. After all, one can always find patterns in the archaeological record, whether it be an increase or decrease in the number of sites, a higher (or lower) incidence of a certain prey taxa, the appearance of a particular tool class or classes, and so on. And one can always derive an accommodative argument to link those archaeological patterns with environmental change.

But such patterns can be a consequence of many other factors that may have little to do with responses to climatic or ecological change: increased human population on the landscape, for instance, can prompt changes in settlement patterns, and perhaps resource depletion that might trigger changes in subsistence systems. Nor can one discount the possibility that those patterns result from factors as mundane as biases in the geomorphic record or archaeological search strategies that make it more likely to find certain kinds of sites or activities and not find others. One must always be wary of mistaking correlation with causation, particularly where the archaeological record is such that the absence of evidence may not be indicative of the evidence of
absence. It is not enough to merely assert a relationship between environmental and cultural patterns or changes, especially not after having taken suites of archaeological and paleoenvironmental data that are at very different scales and can be (and often are) temporally coarse to begin with, and making them even more so. Despite these concerns, I still like this ambitious attempt, precisely because such efforts can be a valuable first step toward suggesting hypotheses that can then be tested against archaeological and paleoecological records on a finer scale.

At the moment, however, there may well be limits to how far a resolution is yet possible in terms of the archaeological data. Hoffecker and Holmes clearly show the Nenana and Tanana River-valley sequences are complex and ambiguous, at least in regard to the relative sequence of tool complexes and kinds of activities. The Nenana and Denali complexes were once neatly separated on apparent technological grounds, but as Hoffecker and Holmes (also Bever) emphasize, that technological distinction is hardly so clear cut anymore. Microblades are absent when they should be present (Panguingue Creek) and present when they ought to be absent (Healy Lake at >10,900 B.P. and Swan Point at >11,500 B.P.); they occur above levels with bifacial projectile points (Dy Creek), except where they occur beside them (Swan Point).

Yet, Hoffecker questions the pre-10,900 B.P. age for microblades at Healy Lake, and dismisses the pre-11,500 B.P. microblades from Swan Point as being part of a "small and impoverished" lithic industry that is not part of any recognized assemblage, and may represent "a doomed attempt at the colonization of eastern Beringia" (Hoffecker, this volume). Doing so, of course, would seem to clear the Beringian landscape of any microblade/microcore technologies from 11,300 and 10,800 B.P., and thus enable Hoffecker to re-assert (see Hoffecker et al., 1993) the claim that Nenana groups bearing a non-microblade technology colonized mid-latitude North America and became Clovis, on the assumption those two assemblages are 'plausibly' similar and chronologically sequential.

But is this a reasonable assertion, and are the two assemblages similar and sequential? I agree that the precise antiquity of the Healy Lake microblades is uncertain (but see Holmes, who also agrees, but nonetheless is convinced the Healy Lake microblades are associated with Nenana complex Chindadn points) and, I have long argued that colonization pulses must have occasionally failed (Meltzer 1989). Thus I am certainly not opposed to the idea in principle, but I do not think either of these arguments—even if correct and I am not convinced they are (nor, for that matter, is Holmes)—can salvage the Nenana-Clovis connection. For as Hoffecker also admits, one cannot preclude the possibility that Nenana is simply a functional variant within a larger microblade industry—that larger industry, of course, bearing little resemblance to anything south of the continental ice sheets in latest Pleistocene times. And, even if it is not such a variant but a stand-alone complex, it is still difficult to see Nenana and Clovis as similar technologically, functionally, or stylistically (also Holmes, this volume). Indeed, given the near-chronological overlap between the two, they ought to be highly similar stylistically, yet they are not. By comparison, there is no mistaking the strong stylistic similarity between classic Clovis forms on the High Plains, with fluted points in eastern North America—fluted points that may be as much as 500 years younger than those on the Plains. Finally, with the recent re-assessment of the Nenana chronology (Hamilton and Goebel 1999) this northern complex may be no older than 11,300 B.P., making it a poor candidate for an historical antecedent of a complex that dates back to 11,500 B.P.

An obvious alternative hypothesis to account for these early (pre-11,500 B.P.) microblade assemblages is that instead of representing a failed migratory pulse, they mark the initial, but still poorly visible, appearance of the Nenana complex. Clearly, groups were present on the Beringian landscape before they appear on archaeological radar; we know that on sampling grounds (archaeological records are a product of many things, not least of which is a straight-up sampling phenomena. The odds of having sites preserved and then found are much better, when the population of sites you have to draw from is larger). We know that because these groups, by the time we see them, are quite familiar with their landscapes—as attested to by the use at Broken Mammoth and Swan Point of obsidian from Batza Tená and the Wrangell Mountains (Holmes, this volume).

While Hoffecker maintains that two or possibly three archaeological complexes are present in the Nenana and Tanana River valleys (Denali and Nenana of which he is certain, and a third, possibly failed pulse about which he speculates), he nonetheless admits that merging the sequences of the two areas leaves a residue of inconsistency and ambiguity. Holmes attempts to clear some of the clutter, by relegating the Nenana complex to local, rather than pan-regional status; and by stressing (quite rightly, I think) the importance of differences in site function, seasonality, sampling etc. Thus, the fact that Denali and Nenana complexes have technofunctional tools like microblades in common may only suggest that functional items like this were used for long periods of prehistory (here and in Siberia as well), and are by themselves unreliable diagnostic features of temporally-specific lithic complexes (with the caveats as noted above). Their presence or absence in a particular assemblage, of course, does have potential for revealing something
of the activities at a particular locality, or perhaps differences in seasonality or habitat (as Holmes suggests).

But perhaps most provocatively, Holmes proposes a set of larger groupings which lump together complexes which might—or might not—be historically related (such as the Eastern Beringian and the rather broad Transitional period, the former to include the earliest Chinhadn points and microblades, the latter Chinhadn variants and Mesa). I will happily leave the debate over these proposed groups to those more versed in the archaeology of the region than I. However, I would echo Holmes’ own caution that we are dealing with very small samples. Under the circumstances, it might be a bit too early to start lumping sites and complexes.

So what does all this say about the initial peopling of the Americas—or at least the Americas south of the ice sheets? In the end, perhaps not very much at all. The Siberian materials are distant and distinct from anything we see in the early Paleoindian sequence. The Alaskan materials are more similar to Paleoindian sequences south of the ice sheets, but we remain unable to draw any specific historical or archeological connections between this region and mid-latitude North America. Indeed, one sees far more similarity in the Late Pleistocene archeological records on the two sides of the Bering Strait, than between either and the area to the south. This is to be expected: Beringia was trafficking in human beings for thousands of years, and what we see there archeologically will no doubt speak more to the movement and interchange of populations between Siberia and Alaska and populations residing in Alaska (perhaps including groups that may have disappeared without issue), than the footprints of colonizing pulses, original or otherwise, down to mid-latitude North America. Depending on how large and how quickly and by what route those initial colonizers were moving, we may not see their footprints at all.

I am as keen as anyone to see Clovis origins in Beringia, or even the origins of the pulse that ended up in South America at 12,500 B.P. (if that was indeed separate from Clovis, as I suspect it might have been, see Meltzer 2002). Nevertheless, at the moment it appears we have in Beringia a record of settlement that in the earliest periods is more Siberian than mid-latitude North American (again given all the caveats about tracing historical affinities, noted above). Except, perhaps, for the Mesa complex: in the end, that does appear to be rather unusual, at least in the context of terminal Pleistocene Alaskan prehistory. If the Mesa complex does indeed represent a different group than those who were in interior Alaska at that same time, what might that mean in terms of population-level processes of interaction, exchange, and territoriality between these two areas and (dare we say) different peoples? It is an interesting question, and one that, in time, may yield an interesting answer.

End Notes

1. Citations without dates are to papers in this volume.

2. All ages are in radiocarbon years.

3. Particular styles are random (you can paint any design you please on a pot), but they are not arbitrary; they derive, in a process involving drift and innovation, from prior forms (producing a stochastic or Markovian structure, see Lipo et al. 1997:310; Nieman 1995:31). Hence, stylistic similarity between two or more objects or assemblages represents proximity in time or space. Stylistic similarity is therefore homologous similarity, and in a historical context marks divergence from a common source, and only divergence, on the assumption that the likelihood of convergence or even chance similarity in truly random traits will be close to nil.

4. For that matter, having two such widely separated sets of ages for the Mesa site surely complicates efforts to link this occupation with Paleoindian groups on the northern Great Plains. At the very least, the 11 ka dates—which well predate the Agate Basin occupation—eliminate the possibility that the Mesa complex originated on the Plains and moved northward.

5. Of course, the same processes that set the radiocarbon record to skipping, ultimately are also responsible for making this one of the most complex periods of Late Pleistocene climate change.

6. Holmes questions Hoffecker’s proposed 500-year gap in the microblade sequence on both empirical and theoretical grounds. I am not as certain as Holmes that we should expect continuity in material culture, and that therefore the gap is not a genuine absence of evidence. One must bear in mind that Beringia may have trafficked in different groups with different toolkits at different times. The gap could be real. The issue is therefore primarily an empirical one—are microblades present during this interval or not?—and here I find Holmes’ skepticism quite reasonable. Of course, using Holmes’ same logic I find it difficult to agree with his suggestion (which is no more than a suggestion), that Nelana represents an unsuccessful experiment.

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