

The 12.9-ka ET Impact Hypothesis and North American Paleoindians

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A hypothesized extraterrestrial impact in North America at ~12,900 calendar years BP (12.9 ka) has been proposed as the cause of Younger Dryas climate changes, terminal Pleistocene mammalian extinctions, and a supposed “termination” of the Clovis archaeological culture. In regard to the latter, however, an examination of archaeological, geochronological, and stratigraphic evidence fails to provide evidence of a demographic collapse of post-Clovis human populations, especially where the Clovis and post-Clovis site records are reasonably well constrained chronologically. Although few Clovis sites contain evidence of an immediate post-Clovis occupation, interpreting that absence as population collapse is problematic because the great majority of Paleoindian sites also lack immediately succeeding occupations. Where multiple occupations do occur, stratigraphic hiatuses between them are readily explained by geomorphic processes. Furthermore, calibrated radiocarbon ages demonstrate continuous occupation across the time of the purported “Younger Dryas event.” And, finally, the relatively few sites purported to provide direct evidence of the 12.9-ka impact are not well constrained to that time. Whether or not the proposed extraterrestrial impact occurred is matter for empirical testing in the geological record. Insofar as concerns the archaeological record, an extraterrestrial impact is an unnecessary solution for an archaeological problem that does not exist.

The book *The Cycle of Cosmic Catastrophes: Flood, Fire, and Famine in the History of Civilization* (Firestone, West, and Warwick-Smith 2006) and a series of articles (e.g., Firestone et al. 2007; Kennett et al. 2009a) propose a hypothesis for an extraterrestrial (ET) impact over the Great Lakes region of North America ~12,900 calendar years before present (hereafter, 12.9 ka). This impact ostensibly sent out intense winds, fireballs, and a sudden radiation flux that purportedly ignited “many thousands of square kilometers” and in turn “decimated forests and grasslands, destroying the food supplies of herbivores and producing charcoal, soot, toxic fumes, and ash” (Firestone et al. 2007:16,020).

This hypothesized ET impact is much debated (e.g., Pinter and Ishman 2008 and comments and replies in *GSA Today* 2008). Whether or not it occurred requires careful documentation and analysis of the geological indicators and evi-

dence of impact. Although some geological evidence is offered on its behalf (e.g., Firestone et al. 2007; Kennett et al. 2009a), strong counterevidence also recently appeared (e.g., Marlon et al. 2009; Paquay et al. 2009; Surovell et al. 2009b). As a result, the empirical demonstration of an impact has yet to be made.

Under the circumstances, discussions of the consequences of an event that is not known to have actually occurred are premature. Nonetheless, proponents of the impact hypothesis have claimed that the “catastrophic effects of this ET event and associated biomass burning led to abrupt YD [Younger Dryas] cooling, contributed to the late Pleistocene megafaunal extinction, promoted human cultural changes, and led to immediate decline in some post-Clovis human populations” (Firestone et al. 2007:16,017).

We leave to others the assessment of whether an ET impact had a causal role in Younger Dryas climate change, Pleistocene mammalian extinction, or more globally had “serious, widespread consequences for anatomically modern humans” (Firestone et al. 2007:16,021). Our purpose in this paper is to examine the assertion that an ET impact caused a “major adaptive shift” and “population decline” among North American Paleoindians, leading to the “termination” of the Clovis culture at 12.9 ka (Firestone et al. 2007:16,021). We focus on

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the archaeological evidence for North American Paleoindians for the same reason as the proponents of an ET YD impact do: if an impact occurred and was as severe as has been suggested, its consequences would have been most devastating for the Paleoindians who occupied the continent where and when it happened.

Changes in Terminal Pleistocene Human Adaptations

Firestone et al. (2007:16,021) suggest “major adaptive shifts are evident at 12.9 ka . . . as subsistence strategies changed because of dramatic ecological change and the extinction, reduction, and displacement of key prey species.” Thirty-five genera of mammals were extinct by the end of the Pleistocene, though as yet the timing of the extinctions remains uncertain, and it cannot be demonstrated that all 35 genera survived until the time of the supposed ET impact or even to the terminal Pleistocene (Grayson 2007). Those mammals were long assumed to be primary prey targets of Clovis “big game” hunters (and still are by some; e.g., Fiedel 2005:99). By that assumption, mammalian extinction would have forced an adaptive change on the part of post-Clovis hunters. However, a critical review of the Clovis archaeological record reveals that a case for predation can be made for only two of those now-extinct genera: mammoth (*Mammuthus* sp.) and mastodon (*Mammut americanum*), at just 14 archaeological sites (Grayson and Meltzer 2003). Accordingly, there is no compelling evidence to claim the animals’ demise had a fatal impact on Clovis groups. In fact, it appears that instead of being dependent on those large mammals, Clovis foraging strategies varied in prey choice and diet breadth, variation that in part reflects differences in habitat and resource structure across late Pleistocene North America (Cannon and Meltzer 2008).

In eastern North America, for example, differences in subsistence strategies between Clovis and post-Clovis groups are poorly known but appear to be relatively inconsequential (e.g., Cannon and Meltzer 2008; papers in Walker and Driskell 2007). Goodyear, in fact, argued that large Pleistocene mammals likely played no role in diets of eastern Clovis groups, as these taxa were locally extinct by the time those hunter-gatherers entered the region (Goodyear 1999:443). In other areas and habitats, post-Clovis adaptive changes are seemingly more pronounced: on the Great Plains, for example, bison become an increasingly more important prey species in post-Clovis times than they had been earlier. Yet even there, bison are but one prey species among a wide variety of prey types, marking a broader diet breadth in keeping with that of earlier Clovis times (Hill 2007; Kornfeld and Larson 2008).

The most obvious archaeological change from Clovis to post-Clovis times is in the appearance of the projectile points and particularly in the means by which these were hafted—mostly obviously, whether fluted or not and how they were fluted (all of these points seem to have been mounted on

thrust or thrown spears). These alterations might reflect changes in the environment or prey species, weaponry design, or stone procurement and mobility patterns. However, in many regions across the Clovis/post Clovis temporal border, those variables remain relatively constant: bison, for example, were common prey on the Great Plains and in the Southwest; many of the same stone sources were used; and virtually all of these groups were highly mobile, though they could have varied in the nature of their mobility. And certainly there were changes in the environment over this multcentury span, though these varied by region and degree (Meltzer and Holliday 2010), and in only a few instances is there evidence to tie those to changes in weaponry. Newby et al. (2005), for example, believe the transition in northeastern North America from largely open tundra to closed mixed pine forests led to a change in large mammal species—the replacement of caribou by deer and moose—and suggest that prompted a shift from fluted to nonfluted points. However, the chronological correlation between the disappearance of fluting and the shift to hunting moose/deer is not secure; it may be coincidental that this change in hafting technology landed on this moment of ecological change. Regardless, that change in projectile points occurred at the end of the YD.

Moreover, if the changing form of Paleoindian projectile points is dominantly stylistic in nature and reflects design elements popular at particular moments in time and space—much like the appearance and disappearance of tail fins on American cars in the late 1950s and early 1960s—then it need not have any bearing on the functional utility of the points or inform on changes in adaptation. Projectile point styles changed regularly and abruptly throughout Paleoindian and later times. The end of the Clovis point style, which actually postdates the purported 12.9-ka “event” (fig. 1; Waters and Stafford 2007) is not a problem that requires an ET impact to solve.

Of course, projectile points are but one element of the Paleoindian tool assemblage. Were there significant adaptive changes in response to novel circumstances or challenges—such as sudden and severe changes in the structure of environments or resources—that should be evident in the appearance of new or modified tools or technologies (Bird and O’Connell 2006). Yet the broader Paleoindian tool kit and technology stayed more or less the same from Clovis into post-Clovis times (Stanford 1999). Even blade technology, thought to be limited to Clovis, has been observed in some Folsom age assemblages (M. Stiger, personal communication, 2009).

Population Continuity or Discontinuity: The Paleoindian Archaeological Record

Conceivably, adaptive continuity in the Clovis to post-Clovis archaeological record masks population decline or discontinuity over that same span. However, the evidence for a post-Clovis demographic collapse is problematic. The claim was originally based on changes in projectile point frequencies in

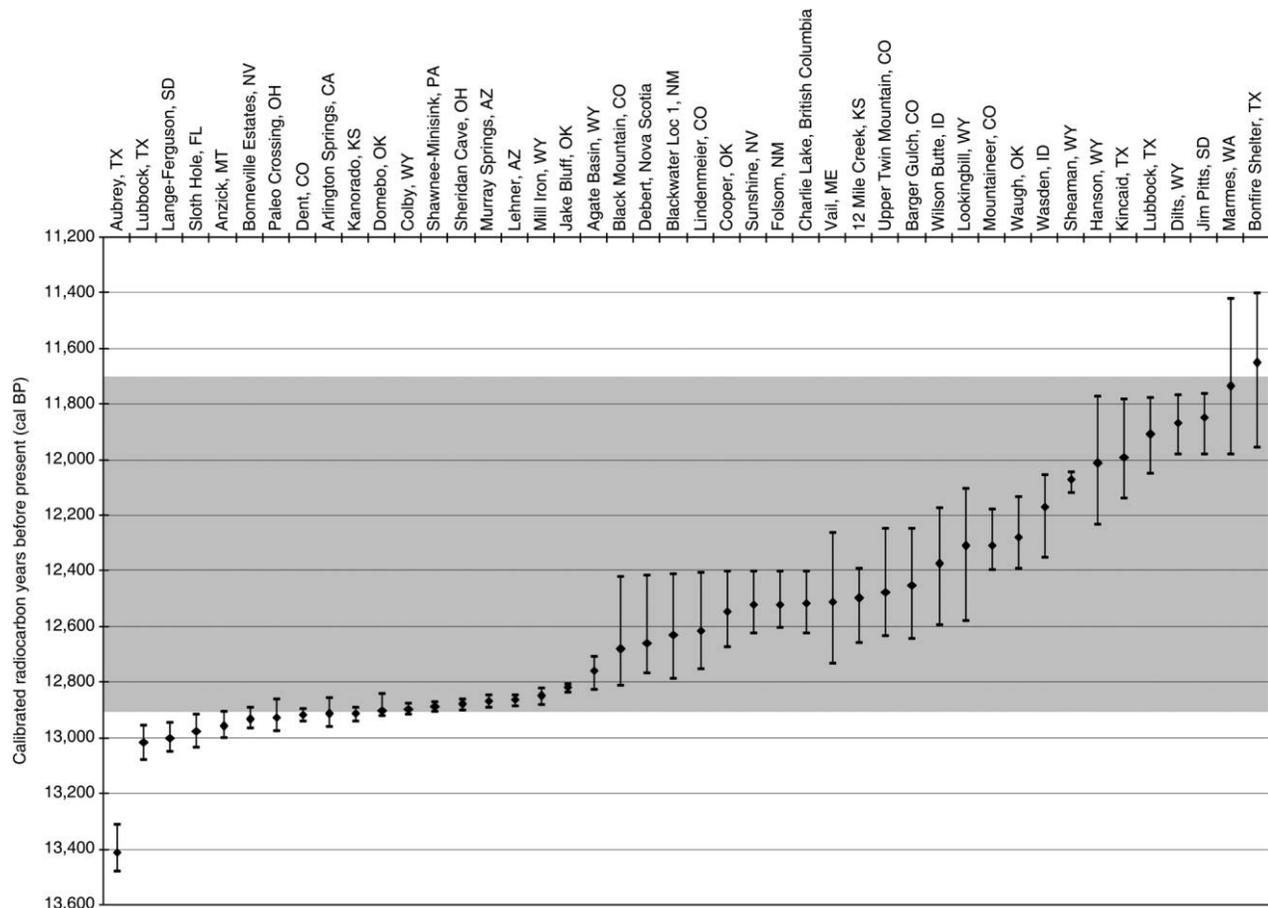


Figure 1. Box plot showing calibrated ages (*diamond*) and 1 standard deviation (SD; *vertical bars*) for Paleoinidian sites, ordered by decreasing calendar age (data from Meltzer and Holliday 2010). The shaded area represents the Younger Dryas Chronozone (YDC) at 1 SD; virtually all sites intersect the YDC. Note the increasing span of years represented by 1 SD for sites younger than ~12,700 cal BP (i.e., Black Mountain and sites to the right of it). As described in the text, although the span of calibrated years increases significantly for those sites, the SDs of their original radiocarbon dates are not significantly different from those sites older than ~12,700 cal BP.

eastern North America, specifically, the relatively larger number of Clovis points and the relatively smaller number of post-Clovis forms, such as Redstone points (Firestone, West, and Warwick-Smith 2006:113–114; Firestone et al. 2007:16,017; Goodyear 2006b).

Yet as Anderson et al. (2008b) note, counting projectile points is “fraught with potential error due to many possible kinds of collection sampling bias and the potential misidentification or inaccurate dating of diagnostic forms.” This is especially the case here: Redstone points, and for that matter all other eastern North American fluted point forms, are assumed to postdate Clovis. Yet with the exception of Dalton projectile points, none of those are securely dated by radio-

carbon, and the stratigraphic relationships of the forms are largely unknown. Redstone points, which were the basis for supposed population decline (Firestone et al. 2007; Goodyear 2006b), are inferred to be post Clovis in age based on fluting style and technology (Goodyear 1999; D. G. Anderson, personal communication, 2009). Although that broader temporal relationship may be correct, no data are available to confirm which point form(s) were the immediate successors of Clovis and, thus, the indicator of the supposed population decline.

Further biasing the issue, Clovis points are readily recognized and for some 60 years have routinely and often systematically been counted in published statewide surveys (e.g., the Paleoindian Database of the Americas [PIDBA], accessible

at <http://pidba.utk.edu/main.htm>; Anderson et al. 2005). In fact, Clovis point tallies may be inflated for some areas, notably eastern North America, given variation in the type and ambiguity in its definition: indeed, points recorded as eastern Clovis may be Redstone points (D. Anderson, personal communication, 2009).

In contrast, post-Clovis fluted point types in eastern North America are almost certainly undercounted (Goodyear 1999: 439). Although Redstone points were first recognized in the 1960s (Cambron and Hulse 1969), they have been systematically tallied only recently (e.g., Goodyear 2006*b*). These and other post-Clovis forms in eastern North America (e.g., Cumberland points) likely also exist in large numbers in private collections but have not been systematically documented or published. Even more problematic, nonfluted, probable post-Clovis lanceolate points—Beaver Lake, Dalton, Quad, Simpson, and Suwanee points, for example—have not been systematically recorded (D. Anderson, personal communication, 2009). Hence, the number of Clovis fluted points in this region is inevitably larger than the numbers of fluted and unfluted post-Clovis forms. Their relative published frequencies thus primarily inform on collection and recording practices and not demographic collapse.

That bias is highlighted when the record from eastern North America is compared with that of the Great Plains, where the post-Clovis archaeological sequence is better constrained chronologically and post-Clovis projectile point forms are well defined and widely recognized. Folsom projectile points, which follow Clovis in time, are sought by collectors, documented by archaeologists, and reported in the literature. Folsom points significantly outnumber Clovis artifacts where they co-occur (e.g., Jodry 1999; Judge 1973; Hill and Holliday, forthcoming; Holliday 1997). A systematic tally from the central Great Plains recorded more than four times as many Folsom points as Clovis points (LaBelle 2005), types that in this region have comparable time spans (~600 calendar years). Collard, Buchanan, and Edinborough (2008) used the PIDBA data set to calculate the number of Clovis and Folsom points in those regions and show almost twice as many Folsom as Clovis points.

Of course, no matter how reliable the numbers, projectile points may not be a valid proxy for past human populations. Given all the biases attendant with their production in prehistory (point numbers varied, e.g., depending on availability of stone), as well as collection and recording practices in the present, numbers of points do not equal numbers of people (nor would we know if they did).

A somewhat better measure of relative population change, though still a very blunt instrument, is the number of archaeological sites from different time periods. These tallies are likewise not without bias, because the numbers are driven by multiple factors, including erosional and depositional processes that help erase or bury sites and make them less visible, the antiquity of the period (time takes its toll), the search techniques used to find sites, the chronological controls on site age, and the

number of sites relative to the size of the area being searched by archaeologists (Meltzer 2009:133). Again, caution is required. For example, Jones (2008:E109) suggests that there are 51 fluted point locations, but “no archaeological sites in California that have reliable radiocarbon dates between 12,900 and 12,200 calibrated years BP.” Yet as Jones and colleagues elsewhere admit, those 51 fluted point locations are dominantly undated surface finds of isolated fluted point specimens (Rondeau, Cassidy, and Jones 2007:68). In the absence of radiocarbon control, it is unclear when or even whether gaps occurred in the California Paleoindian record.

In contrast, in a region like the Central Great Plains, where geomorphic factors can be held somewhat constant, recognition biases are reduced and archaeological sequences are reasonably well constrained chronologically, the relative (not absolute) frequency of Clovis and post-Clovis sites is instructive: there are 3.4 times as many Folsom sites as Clovis sites (LaBelle 2005). Whether coincidence or not, it is noteworthy that the ratio of Folsom : Clovis archaeological sites is virtually the same as that of Folsom : Clovis projectile points.

Thus, in regions that are better sampled and more securely dated and where biases are at least minimized, there was no post-Clovis population collapse, as measured by the numbers of sites and projectile points. The archaeological record, in fact, suggests the opposite: an increase in population in the post-Clovis centuries.

Population Continuity or Discontinuity: The Radiocarbon Record

Other arguments both for and against population discontinuity in post-Clovis times rely on radiocarbon dates, using either the trend in the overall frequency of radiocarbon dates over time as a direct demographic proxy (Buchanan et al. 2008), the number of radiocarbon dated sites to indicate continuity or lack thereof, or perceived gaps in the radiocarbon and stratigraphic record at specific sites (Firestone et al. 2007).

The first of these approaches is problematic since there are many factors that drive the frequency of available radiocarbon dates, such as whether one obtains suitable samples to date, whether the samples are analyzed (which could reflect little more than budget issues), whether the results are reliable, published, and so on. The second approach—using the number of radiocarbon dated sites—is subject to the same biases that impact site discovery and recognition (noted above). But if the frequency of radiocarbon ages or radiocarbon-dated sites over time is not a reliable or a valid approach to gauging population changes in Clovis and post-Clovis times, perhaps the presence or absence of radiocarbon ages provides a somewhat more reliable measure.

The scenario presented by Firestone et al. (2007) assumes a relatively large population in Clovis times (immediately before 12.9 ka) followed by a massive demographic collapse due to continent-wide ecological disruption and wildfires in the centuries after the ET impact. The period of population nadir—

suggested to have spanned half a millennium or more—was then followed by centuries of increasingly larger populations as human numbers rebounded from the catastrophe.

If this scenario took place, there ought to have been corresponding changes in the numbers of camp and kill sites produced by human groups over the same period of time, and the population of sites surviving to the present ought to fluctuate in a corresponding manner (more Clovis sites, fewer immediately post-Clovis sites, increasing numbers of later Paleoindian sites). Here we assume that the likelihood of site preservation and discovery from 12.9 to 11.7 ka (the span of the Younger Dryas Chronozone [YDC]) was the same. No geological or archaeological biases differentially impacted sites of different ages.

In a random radiocarbon sample of that population of sites, particularly the small sample we possess of Paleoindian sites, the presumed “postcatastrophe” period—with its relatively fewer sites—should appear as a noticeable gap in the radiocarbon chronology of Paleoindian sites (whether it would or not depends on the luck of the sample). In fact, without a very large sample, the odds are against archaeologists finding any dated or datable sites within that post-Clovis period. If, in contrast, the record of radiocarbon ages is essentially continuous across the span of the supposed catastrophe, it would suggest we are sampling the same size population of sites and, accordingly, not one that was generated by a human population that had suffered massive demographic collapse.

We compiled and calibrated available radiocarbon dates from Paleoindian sites that fall within the span of the YDC, and for which the association between the radiocarbon age and the archaeological occupation is reasonably secure (data from Buchanan et al. 2008; Holliday 2000; Waters and Stafford 2007). We include Clovis and Clovis-age sites that predate the YDC as well as later Paleoindian sites falling within at least one standard deviation of its end at 11,700 cal BP. Following the suggestion of Kennett, Stafford, and Southon (2008b), this sample of 44 sites includes only those with standard deviations equal to or less than 100 radiocarbon years; sites with standard deviations >100 radiocarbon years were excluded, so as to remove any effects of poor radiocarbon resolution (Kennett, Stafford, and Southon 2008b). Ideally, of course, we would use only sites with radiocarbon ages with standard deviations of 50 years or less and ones based on short-lived organic materials (though some of these short-lived materials are not always preferable for radiocarbon dating, as Geib [2008] has recently demonstrated). However, it is an unfortunate fact that few Paleoindian sites are that precisely dated, and many sites, especially in eastern North America, are not radiocarbon dated at all. Calibration was done using CALIB 5.0.1.

The box plot (fig. 1) of these ages, in which the error bars represent one standard deviation, shows no significant gap in the radiocarbon record at the onset of the YDC, the supposed geological moment of impact. With the exception of the Au-

brey site (the oldest of the sites in the sample), each site overlaps at one standard deviation with the site(s) to its left (older) and right (younger). Chronological gaps appear in the sequence only if one ignores standard deviations (a statistically inappropriate procedure), and doing so creates gaps not just around 12.9 ka but also at many later points in time.

However, it is conceivable that this conclusion is analytically flawed. There could be gaps in the radiocarbon record that are invisible because of ambiguity in the calibration curve during the YDC (Muscheler et al. 2008) or because a continent-wide compilation of ages—combining sites from areas not severely devastated by an ET impact with those from areas that were (perhaps regions closer to the point of impact)—gives the overall impression of continuity. The former concern seem less likely, since the confounding effects of changes in atmospheric ^{14}C (and hence calibration complications) seem to be most pronounced after 12.7 ka (Bartlein et al 1995; Meltzer and Holliday 2010). But the latter seems a reasonable possibility. Accordingly, detection of a post-12.9-ka period of population nadir might require examining the stratigraphy and radiocarbon ages of the sites themselves.

The Paleoindian Stratigraphic and Radiocarbon Records Considered

Kennett and West (2008), for example, observe that “Archaeological sites containing both Clovis and immediately post-Clovis material are rare. . . . Of the 11 well-dated (Waters and Stafford 2007) credible Clovis sites, none has post-Clovis materials immediately above, suggesting a potential disruption in settlement or landscape use” (Kennett and West 2008: E110).

Sites containing both Clovis and immediately post-Clovis material certainly are rare. There are reasons why that might be so, not least that a significant number of Paleoindian localities are kill sites, where circumstances permitting the kill (e.g., the temporary aggregation of prey) were contingent and rarely repeated. But leaving possible explanations aside, only the Clovis type site (Blackwater Draw Locality No. 1, NM) and Jake Bluff (OK) have a documented in situ Clovis occupation below an in situ Folsom occupation (Bement and Carter 2005; Sellards 1952; Indian Creek, MT, may have a similar sequence, but the data are mostly unpublished and equivocal [Davis and Baumler 2000]).

Yet it is equally true that any evidence of multiple, successive Paleoindian occupations is rare—even over the entire several-thousand-year span of the Paleoindian period. The very few examples found after nearly a century of looking—the two sites just noted plus Lubbock Lake, Texas (Johnson 1987), and Hell Gap and Agate Basin, Wyoming (Frison and Stanford 1982; Larson, Kornfeld, and Frison 2009)—are the exceptions (fig. 2; for data see CA+ online supplement A). Unlike post-Paleoindian times, when favored localities were returned to on multiple occasions, Paleoindian groups had a relatively empty landscape and unfettered mobility, and they

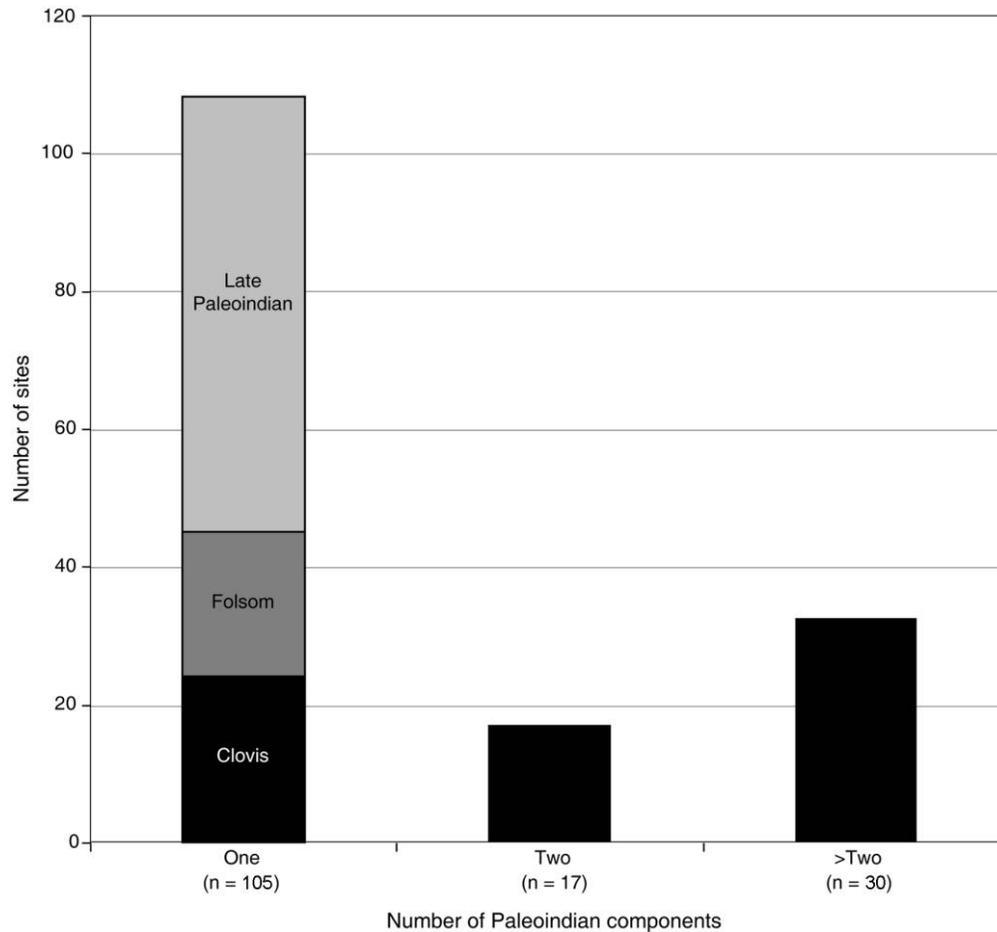


Figure 2. Frequency of Paleoindian features in archaeological sites (see CA+ online supplement A).

rarely used the same spot twice, save in the case of fixed places on the landscape that provided important but rare resources, such as outcrops of high-quality stone for tool making or freshwater springs (like Blackwater Draw Locality No. 1). These places were often used repeatedly and not just in Paleoindian times. However, because of the palimpsests that accrue at these locations, teasing apart separate occupations is exceedingly difficult (Larson, Kornfeld, and Frison 2009; Sellet 2001).

To further emphasize the lack of redundant use of space even in later Paleoindian times, we observe that of those “11 well-dated sites” (Kennett and West 2008), only four have both a Clovis and a later post-Paleoindian (Archaic or Late Prehistoric) component, while the other seven have only a Clovis component. In fact, in a much larger sample ($n = 156$; supplement A) of sites from Clovis through late Paleoindian times, including the 26 tallied by Waters and Stafford (2007), over two-thirds (105) are single-occupation sites. Measured by this criterion, there apparently was considerable “disruption in settlement and landscape use” throughout the

Paleoindian period, but again, unless there were multiple ET impacts, this characteristic can be readily explained by Paleoindian land use patterns. In that regard, comparing the number of Clovis and Folsom sites with and without reoccupation shows that the incidence of reoccupation between the two is statistically indistinguishable (table 1).

Kennett and West (2008:E110) offer as additional evidence for a post-Clovis human population decline the observation that at two stratified sites (Blackwater Draw Locality No. 1 and Shawnee-Minisink), the “Clovis and immediately post-Clovis material . . . are nearly always separated by culturally sterile sediments.” There are ready geomorphic explanations for that pattern, however.

At Blackwater Draw Locality No. 1, Kennett and West (2008:E110) argue Clovis-age material “is poorly dated but is capped by a black sedimentary layer indicating a terminal age of 12.9 ka as summarized by Haynes [2008 and references therein]. Folsom-age materials occur above the Clovis materials but a hiatus of ~500 years is suggested by an intervening sterile deposit (10–35 cm) and radiocarbon ages of 12.4–11.8

Table 1. Contingency table analysis of Clovis and Folsom sites with and without reoccupation

	Clovis	Folsom	Totals
Single occupation	25	22	47
Two or more occupations	12	20	32
Totals	37	42	79

Note. $G = 1.896$, $df = 1$, $P = .168$ (difference is not significant). Data from CA+ online supplement A, table A1.

ka.” Understanding the geomorphology and stratigraphy of the site is critical: it sits in a basin that extends over half a square kilometer, and drained into Blackwater Draw. The first archaeological investigations began there in 1933 and continued intermittently to the present, but most of the site was destroyed by commercial gravel mining (Hester 1972). What is known of the archaeology and geology varies considerably, owing to the standards of recovery and recording by different research teams and the varying and often difficult conditions under which some of the fieldwork was conducted (e.g., in and around quarrying machinery). Despite this, there is still evidence from the site for a minimum of 27 Paleoindian components, including bone beds and habitation areas (table 3.4 in Holliday 1997). Many more were assuredly once present (Hester 1972).

The section at Blackwater Draw Locality No. 1 sampled for evidence of an impact and illustrated by Firestone et al. (2007, fig. 1 and supporting information) is on the South Bank of the original basin, in a section described and discussed by Haynes (1995). The South Bank is the most complete stratigraphic section remaining at the site. It approximates the general site stratigraphy reported from the basin but probably not the geological or archaeological microstratigraphy. Further, the South Bank cannot be precisely correlated to the original basin fill because the latter has long since been removed.

More complicating still, in situ Clovis or Folsom archaeology is unknown in or around the immediate area of the South Bank section sampled by Firestone et al. (2007). A hand-dug pit of probable late Clovis age was found ~56 m east of the section sampled by Firestone et al. (Haynes et al. 1999; G. Crawford, personal communication, 2009), but stratigraphic discontinuities prevent direct correlation between the two areas. Excavations at the site in 1936 and 1937 produced Clovis and Folsom material in situ ~20 m northeast of the sampled section, but direct stratigraphic correlations between the two areas are also impossible because their traces were long ago destroyed by quarrying. Arguments for Clovis-Folsom continuity or lack thereof cannot be based on the section reported by Firestone et al. 2007.

The “sterile” character of the deposits separating purported Folsom and Clovis material is considered an indicator of depopulation due to an ET catastrophe. But these stratigraphic breaks are typical of Paleoindian features and occupation

zones known at the site. Stratified Clovis, Folsom, and other post-Clovis artifacts and features are known from several other areas of the site (e.g., the 1936 and 1937 excavations noted above). These features and associated microstratigraphy were minimally described and reported, but based on some published descriptions and the vertical distribution of artifacts (e.g., figs. 41, 45 in Hester 1972), all seem to be separated by sterile sediments. This is not surprising given the complex interplay of lacustrine, spring, eolian, and slope-wash sedimentation that varied across this basin over the last 13,000 years (Haynes 1995).

Shawnee-Minisink, overlooking the Delaware River in Pennsylvania, contains an extensive Clovis occupation, radiocarbon dated to 12.88 ka (Dent 2007). Above that level is ~1.7 m of sterile alluvium and then an early Archaic occupation layer. The sterile alluvium is likewise offered as evidence of “a post-Clovis decline in human populations” (Kennett and West 2008:E110). Yet in the same section, >50 cm of sterile, fluviially derived sediment separates the Early Archaic from a still higher Late Archaic occupation (fig. 1.3 in McNett 1985). As was the case at Blackwater Draw Locality No. 1, geomorphic processes rather than an ET-triggered population collapse is the most parsimonious explanation for the observed stratigraphic pattern.

In fact, the presence of thick, archaeologically sterile sediments in lacustrine and fluvial site settings is common, as is well illustrated by work on the Paleoindian levels at the Allen (Bamforth 2007), Wilson-Leonard (Collins 1998), Lubbock Lake (Johnson 1987), and Richard Beene sites (Thoms and Mandel 2007). As with Blackwater Draw Locality No. 1 and Shawnee-Minisink, geomorphic processes readily explain these stratigraphic breaks throughout Paleoindian times. At Lubbock Lake, stratigraphically similar to and 150 km southeast of Blackwater Draw Locality No. 1, Folsom and post-Folsom occupations are found interstratified in otherwise archaeologically sterile diatomite, sandy slope-wash, and sandy eolian deposits (Holliday 1997). The other sites are all in alluvial settings where sterile deposits are expected. In fact, the Gault site (Texas) is reported to contain multiple and sequential Clovis occupations, and these too are “separated by nearly sterile zones” (Collins 2007:62).

Locations where multiple Paleoindian occupations are not separated by thick sterile zones—for instance, the Jim Pitts (Sellet, Donahue, and Hill 2009), Big Eddy (Hajic et al. 2007), and Pavo Real (Collins, Hudler, and Black 2003) sites—are settings where the locality was on a surface that was stable or quasi-stable for relatively longer periods of time (although it perhaps goes without saying, we observe there was no uniform or “universal” stratigraphic response, as one might expect from a continent-wide YD impact). However, even in these settings different occupations can often be analytically separated (by examining the artifact layers), assuming those occupations were not otherwise mixed before or during soil formation.

On the Age of the Supposed ET Markers at Archaeological Sites

Firestone et al. (2007:16,016) begin their paper stating that “a carbon-rich black layer, dating to ~12.9 ka (12,900 cal years BP), has been identified by C. V. Haynes . . . at >50 sites across North America as “black mats,” carbonaceous silts or dark organic clays. . . . The age of the base of this black layer coincides with the abrupt onset of the Younger Dryas (YD) cooling.” Haynes (2008:6520), however, states that black mats include deposits that range from black to gray to white in color, and include mollisols (grassland soils with thick, dark, carbonaceous surface horizons), wet-meadow soils, algal mats, or pond sediments. Thus, they are not all high in carbon. Moreover, Haynes’s table 2 clearly shows that many of the “black mat sites” have base dates younger than the onset of the YD, and others have base dates older than the onset of the YD. In several key regions of the Great Plains, carbonaceous soils and sediments in both lowland and upland settings clearly have time-transgressive lower and upper boundaries (Holliday 1995; Mandel 2008; Mason et al. 2008) resulting from a variety of geomorphic processes (Meltzer and Holliday 2010). Even the Murray Springs black mat, which Firestone et al. (2007:16,017) rely on heavily as the chronological anchor for their ET event, has several layers, is time-transgressive and postdates 12.9 ka (Jull et al. 1999; A. J. T. Jull, personal communication 2009).

To further illustrate the general pattern, figure 3 shows basal or oldest dates from black mats that formed between 18,000 and 9000 cal BP. The dates were selected and calibrated following the criteria used for Clovis sites, noted above. They are grouped by state or region and arranged roughly geographically. The plot clearly shows that (1) black mat formation was a time-transgressive process in much of North America; (2) organic-rich “mats” similar to those documented by Haynes in the San Pedro Valley of Arizona (Haynes and Huckell 2007) are known from late Pleistocene sections older and younger than the YD—in fact, Quade et al. (1998) document their formation throughout the Holocene in the Great Basin; and, (3) black mats are relatively scarce in many areas, including eastern North America and the upper Great Lakes (the latter being the region of the supposed impact). The claim for a sudden, synchronous, continent-wide stratigraphic “event” is very weak.

Finally, we observe that five of the seven archaeological sites for which ET markers are reported and said to have “calibrated YDB [Younger Dryas Boundary] ages” at 12.9 ka (Firestone et al. 2007, supporting information, fig. 9) are either not well constrained to that time or are not dated at all.

Firestone et al. (2007) put the age of the Clovis occupation and the onset of the YD at Blackwater Draw Locality No. 1 at 12.9 ka and further assert that there is a 500-year post-Clovis gap at the site. But they do so by selecting two radio-

carbon ages from among a very large corpus of dates available from this extensive and stratigraphically complex site (Haynes 1995). More problematic, those two ages ($11,040 \pm 500$ ^{14}C years BP [A-490; 12.90 cal ka] for the Clovis level and $10,170 \pm 260$ ^{14}C years BP [A-488; 11.86 cal ka] for the Folsom level) come from the North Bank area of the site. Yet their ET samples and illustrated section come from 360 m to the southwest in the South Bank area of the site (A. West, personal communication, 2009). The South Bank has its own detailed suite of ages, which do not always match up with those on the North Bank. Moreover, and as noted above, there are no known Clovis or Folsom features in their immediate sampling area, and the nearest Clovis and Folsom occupation surfaces, from the 1936 and 1937 work, were never dated.

Chobot (Alberta) lacks any radiometric control. Firestone et al. (2007) assigned it an age of 12.9 ka on the assumption that the presence of Clovis points limits its age “to an interval of ~200 yr ending at 12,925 cal B.P.” (Firestone et al. 2007:SI). However, Clovis sites postdate (or well predate) 12.9 ka, by at least two to three times that span (fig. 1; Holliday 2000; Waters and Stafford 2007). The occurrence of a Clovis point is not the equivalent of a radiocarbon age, and given the variation in the type—variation that has not been radiocarbon dated with precision—using its presence to “date” a site is, at best, a rough estimate and, at worst, circular reasoning.

There are two thermoluminescence (TL) ages on burned chert from the Gainey (MI) site: $12,360 \pm 1224$ years BP and $11,420 \pm 400$ BP (Lepper 1999:370; Simons, Shott, and Wright 1987). Firestone et al. (2007:SI) cite only the ~12,400-year age (curiously, 6 years earlier Firestone put the age of Gainey at 39,000 years BP on the assumption that a supernova-generated “nuclear bombardment” at 12,900 cal BP had contaminated the site’s radiocarbon dating [Firestone and Topping 2001:12]). However, the Gainey site excavators consider the 12.4-ka age “earlier than expected” and believe the site was occupied closer to the younger end of the standard error for that age (Simons, Shott, and Wright 1987:28). But even 12.4 ka puts the occupation at Gainey and the ET evidence 500 years later than at other sites. Yet the Gainey site is located near the point of the hypothesized ET impact, and that evidence ought to date closer to 12.9 ka.

There are also luminescence ages for the Clovis artifact level at the Topper (South Carolina) site (in this case, optically stimulated luminescence rather than TL), with the samples closest to the Clovis artifacts dating to $13,500 \pm 1,000$ BP (Goodyear 2006a). That puts the Clovis horizon at Topper in the approximate time of the 12.9-ka event, but again with poor chronological resolution.

Four radiocarbon ages are available for faunal remains from Wally’s Beach (Kooyman et al. 2001), which fall into two statistically distinct age populations: one dating to $13,180 \pm 45$ cal BP, the other to $12,930 \pm 80$ cal BP. Yet at

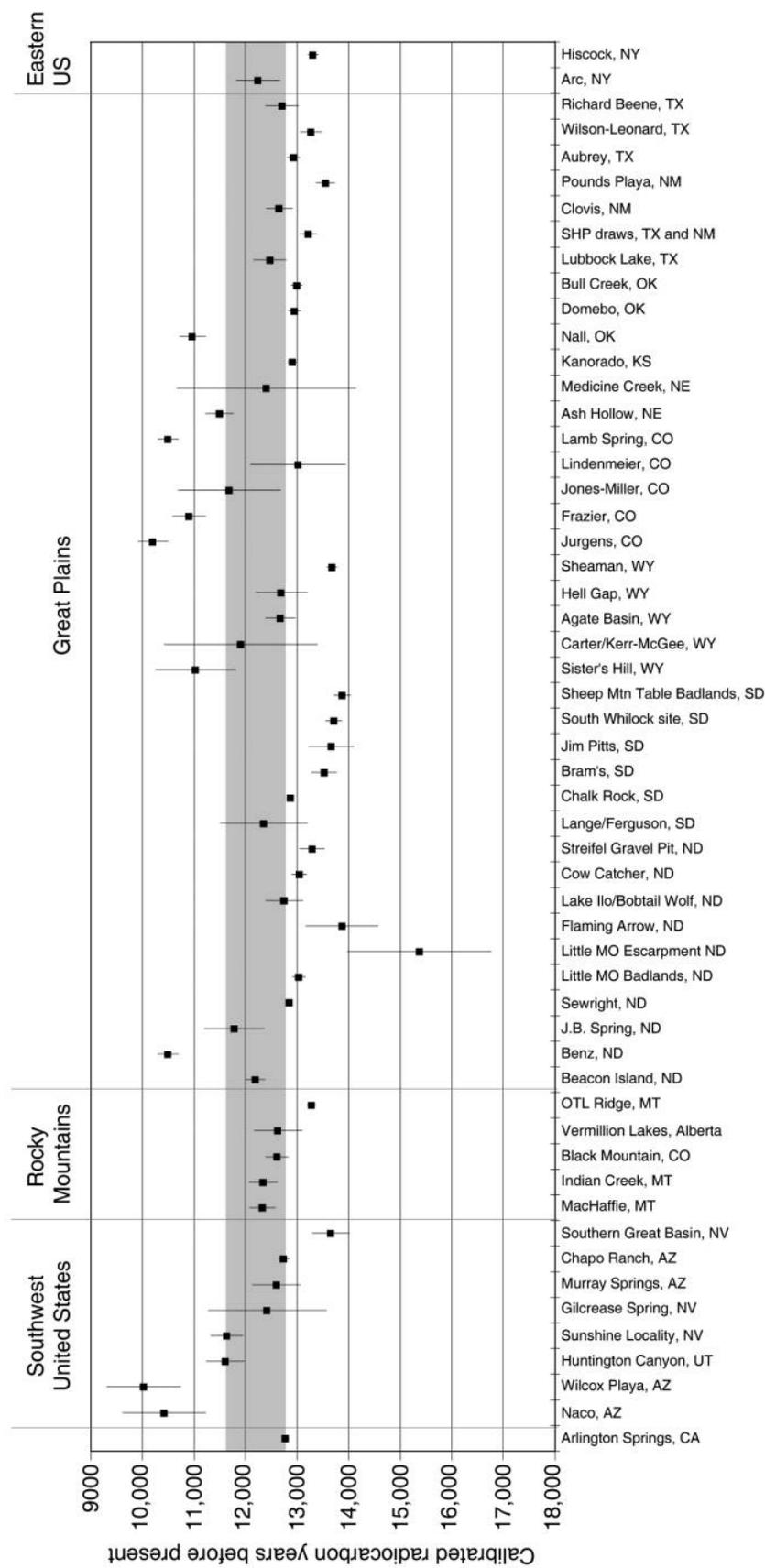


Figure 3. Box plot showing calibrated ages (square) and 1 standard deviation (vertical bars) for lowest or oldest (or only) "black mats" from sites largely in the central and western United States. The shaded area represents the Younger Dryas chronozone (data from table 2 in Haynes 2008; plus additional dates compiled from Holliday 1995, May 2007, Hill et al. 2008b, and May et al. 2007).

Wally's Beach, "none of the Paleoindian points recovered was in situ and therefore it is not possible to directly link the points with the faunal remains" (Kooyman et al. 2001:687).

Discussion and Conclusions

The North American landscape underwent a wide variety of changes in climate, biota, and geomorphology at the end of the Pleistocene, and there is little question that those changes were significant, at least on a timescale of centuries and millennia (Cannon 2004; Shuman, Bartlein, and Webb 2005). However, there is no evidence of changes from Clovis and post-Clovis times that were so abrupt as to require major or rapid shifts in adaptation or even that such shifts occurred (also Meltzer and Holliday 2010).

That post-Clovis groups across North America display a wide variety of projectile point styles (Meltzer 2009) and yet otherwise maintained a tool kit and adaptive strategies broadly similar to Clovis, even if the details varied, likely indicates that later groups were more restricted in their geographic extent and had less interaction with distant kin (Goodyear 1999). This is to be expected following the settling-in of peoples into different areas and/or habitats across North America and a reduction or "closing" of the social systems, as population increases in post-Clovis times reduced the need—present in Clovis times—to maintain the large and open social systems and alliances critical to insuring access to resources, information, and mates across a vast and only thinly populated landscape (Meltzer 2004).

What is also evident in the archaeological record is that the long-term consequences of terminal Pleistocene climatic and ecological changes brought about cumulative changes in adaptation, such as the increasing role over time of bison in Paleoindian diets on the Great Plains. The relatively greater importance of bison hunting in this region has been attributed to increasing hunting proficiency, to bison becoming more abundant on the late Pleistocene and early Holocene landscape (Hill 2007; MacDonald 1981), or to some combination of the two. It is not necessary to invoke an ET impact to explain the increase in bison numbers, for that phenomenon is likely a result of competitive release, owing to the extinction of other large grazers (e.g., mammoth, horse, and camel), and changes in the nature and structure of the Great Plains grassland. This also begs the question why, if the extinctions of those other grazers was the result of an ET event, bison survived while those other genera did not, and why there is no evidence of a 12.9-ka bottleneck in bison populations as can be seen at, say, 37,000 BP (Shapiro et al. 2004)?

Evidence of a demographic collapse of post-Clovis human populations called for by the ET impact hypothesis is not apparent in areas where the Clovis and post-Clovis site records are reasonably well constrained chronologically and where the biases attendant with these records can be minimized (as we

note above, they can never be fully eliminated). To be sure, there are gaps in site occupations and site stratigraphy: very few Clovis sites contain evidence of an immediate post-Clovis occupation. Yet it is also the case that the great majority of Paleoindian sites contain but a single occupation. Among stratified sites with multiple Paleoindian occupations, none exhibit evidence for a hiatus between Clovis and post-Clovis occupations any more significant than gaps among or between Folsom or the other post-Clovis occupations. Blackwater Draw Locality No. 1, offered as key evidence in support of a significant gap between Clovis and Folsom occupations, is perhaps the least informative among sites with multiple Paleoindian occupations because of the varied and often chaotic nature of field recovery and because the site was largely destroyed.

Radiocarbon dating provides further evidence supporting our contention of no obvious gap or disruption in the Clovis-to-post-Clovis occupation record of North America, at least not in areas with well-documented and dated sites (largely on the Great Plains and adjacent areas). Granting the caveats noted earlier for these data, the plot of calibrated radiocarbon ages (fig. 1) seemingly illustrates a trend of continuous occupation across the time of the hypothesized "YD event." Likewise, radiocarbon dating also shows no synchronous, abrupt change in depositional environments across the continent coincident with the lower boundary of the Younger Dryas Chronozone (fig. 2).

As we expressed at the outset, resolving the empirical question of whether there was some sort of extraterrestrial impact at 12.9 ka has to be done with evidence from the geological record. Only after that evidence has been amassed and critically vetted and the hypothesis is shown to withstand falsification will it be appropriate to explore what effects such an impact might have had on contemporary human populations.

Until then, and based on current archaeological evidence, it seems that any effects must have been subtle indeed. There is no compelling data to indicate that North American Paleoindians had to cope with or were affected by a catastrophe, extraterrestrial or otherwise, in the terminal Pleistocene.

Acknowledgments

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Comments

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Here and in a related paper, Holliday and Meltzer argue that there is no compelling archaeological evidence for appreciable environmental change associated with the onset of the Younger Dryas (YD) that would have affected North American Paleoindian populations (Meltzer and Holliday 2010). The evidence suggests a more ambiguous picture. The eastern Paleoindian record is quite different a few centuries into the YD than at the onset. Distinctive projectile point forms occur in many areas, likely marking the emergence of subregional scale cultures; group ranges apparently decreased markedly; only modern fauna were exploited; and interaction patterns changed (Anderson 1990, 1995, 2004; Anderson et al. 2009, 2010; Meltzer 2004, 2009). Unquestionably, eastern Paleoindian assemblages need to be better documented stratigraphically and chronologically. But to claim that “no data are available to confirm which point form(s) were the immediate successors of Clovis” is not completely accurate. The northeast and midwestern projectile point sequence is fairly well documented (Bradley et al. 2008; Ellis and Deller 1988, 1990, 1997; Shott 2004), and while in the southeast full-fluted forms are not well dated, some later unfluted forms are, and full-fluted forms are absent among them, suggesting they fall somewhere in between chronologically (Anderson 2004; Anderson and Meeks 2008; Anderson and Sassaman 1996; Anderson et al. 2009). Since in the west Folsom points are immediately post Clovis in age, it is not unreasonable to assume that forms similar in manufacture and appearance are roughly contemporaneous.

The author’s use of PIDBA (Paleoindian Database of the Americas; <http://pidba.utk.edu/>) needs minor clarification. The following statements are not entirely correct: “post-Clovis fluted point types in eastern North America are almost certainly undercounted” and “[Redstone] and other post-Clovis forms in eastern North America (e.g., Cumberland points) likely also exist in large numbers in private collections but have not been systematically documented or published.” Fluted points have been consistently reported regardless of type in every eastern Paleoindian point survey of which I am aware; indeed, they are typically called “fluted point surveys” for that reason. Holliday and Meltzer probably meant that we are missing information and undercounting all fluted points, including Clovis,

in those states or provinces where no active recording projects are underway. Nonfluted points in most surveys, furthermore, are unquestionably underreported.

If full-fluted forms are immediately post Clovis in age, then the southeastern data show a marked decline in numbers compared with Clovis data (Anderson et al. 2008a, 2008b, 2010). Full-fluted forms, which are consistently reported in the Southeast, occur far less commonly than do presumed Clovis point types. Whether this reflects how long these types were in use, population reorganization or collapse, or other factors remains unknown. Holliday and Meltzer’s radiocarbon data also suggest a decline. In the early centuries of the YD, from 12,800 to 12,600 cal BP, only five sites are reported, while the two centuries before and after have 16 and eight sites, respectively. If the initial YD decline is not due to calibration issues, their data reinforce the southeastern projectile point pattern. A similar decline in dated sites during the early YD has been observed throughout much of the northern hemisphere (Anderson et al. 2009).

The authors’ statement that “a systematic tally from the central Great Plains recorded more than four times as many Folsom points as Clovis points (LaBelle 2005)” and that “Folsom points significantly outnumber Clovis artifacts where they co-occur” may be accurate for the areas referenced. But roughly equal numbers of fluted (presumed Clovis) and Folsom points have been recorded in the PIDBA sample from west of the Mississippi, although some of the fluted points probably date later than Clovis, as the authors note. The continental sample may trump more localized samples, but PIDBA is a work in progress and has many data gaps and sources of bias. I urge archaeologists to contribute to its development. We need more recording projects like those in Texas and Virginia, which through regular publication of locational and attribute data exemplify such efforts (Anderson et al. 2010; Bever and Meltzer 2007; Hranicky 2008; McCary 1984; Meltzer 1986, Meltzer and Bever 1995).

Claiming “the end of the Clovis point style . . . actually postdates the purported 12.9-ka ‘event’” or that Folsom and Clovis both last ~600 years in the Central Plains, while citing Waters and Stafford (2007) for the temporal range for Clovis is curious, since they, and I, and a number of other Paleoindian researchers authored a letter noting that Waters and Stafford had “not definitively established the temporal span of this [Clovis] cultural complex in the Americas” (Haynes et al. 2007:320b). Waters and Stafford, furthermore, argue for a much shorter duration for Clovis than ~600 years. Finally, if “differences in subsistence strategies between Clovis and post-Clovis are poorly known” in eastern North America, how does it follow that they “appear to be relatively inconsequential?” While the initial centuries of the YD may not have been catastrophic, changes in Paleoindian culture did occur. Much more research is needed to fill the gaps in our knowledge, and Holliday and Meltzer deserve our thanks for highlighting the ambiguity that exists.

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The extraterrestrial-impact (ET) hypothesis contains testable expectations, including predictions that markers (iridium, nanodiamonds, carbon spherules, etc.) and widespread burning (soot and charcoal) will be found in above-background levels in deposits dating to the start of the Younger Dryas (YD). Confirmation of the presence of these markers is needed before the ET hypothesis is generally accepted. However, discussions about the effect of the ET event on other aspects of the YD have invigorated YD investigations.

Holliday and Meltzer address the prediction that the ET event disrupted Paleoindian populations, leading to the demise of Clovis culture. In opposition to this prediction they present, among other data, a record of radiocarbon-dated sites and a comparison of Clovis to Folsom site numbers and projectile point numbers.

Their figure 1 contains 44 radiocarbon-dated sites from North America offered as evidence for a continuous human presence across the time of the purported ET event. Holliday and Meltzer admit the inclusion of sites from such vast areas could potentially mask evidence of a hiatus by populations less affected by an ET event.

Within the dated Clovis and Folsom sequence on the southern Plains, Jake Bluff holds a pivotal position. Jake Bluff is not a mammoth kill but an arroyo trap bison kill that was successfully employed by Clovis hunters to procure a minimum of 22 bison (Bement and Carter 2010). The development of this hunting strategy, usually attributed to Folsom hunters, suggests Clovis hunters were adapting to changing prey availability. While this situation seems to support the position that the Clovis adaptation did not come to an abrupt halt, there are no Clovis sites dating after Jake Bluff. The Clovis development of the arroyo trap technique may be the first in a long line of independent inventions of the technique derived from an understanding of bison behavior and the availability of suitable landforms to contain the animals. Such a situation precludes a cultural link between Clovis and Folsom hunting adaptations.

Jake Bluff contains both Clovis and Folsom components in stratified context (Bement and Carter 2010). The Clovis component's age has been refined by additional bone radiocarbon dates that provide an average 1σ calibrated range of 12,825–12,850 cal BP (Calib 5.1; Reimer et al. 2004), bringing it closer in age to the latest Clovis mammoth kills. The Folsom component is found substantially higher (1.3 m) in the profile, suggesting the passage of an unknown but potentially significant period of time between these two components. At the nearby Cooper site, the Folsom-age kills occupy a similar stratigraphic position in the gully fill, and the lowest kill event dates to a split range of 12,424–12,432 and 12,635–12,768 cal

BP (Bement 1999; Calib 5.1; Johnson and Bement 2009). Herein is the problem with using radiocarbon dates to address the issue. The range of separation between the oldest kill at Cooper and the Jake Bluff Clovis component can be as short as 57 years or as long as 426 years. Based on this situation, you can argue either for or against a hiatus between these cultures at these sites. Adding in more dated sites from the area potentially will help, but as long as the calibration plateau exists for mid-YD dates, the results will remain inconclusive. Only tightly stratified components where geoarchaeology, taphonomy, and perhaps other nonradiometric dating techniques can suggest a minimal passage of time will be able to address this ET prediction.

Considering population crash, Holliday and Meltzer cite the greater number of Folsom sites and Folsom points compared with Clovis sites and points as evidence that post-Clovis population actually increased in the Great Plains. These data, with the caveats provided by Holliday and Meltzer, are valid if Folsom technology derives from Clovis technology. The presence of nonfluted point cultures during the early Paleoindian period exacerbates the situation and may provide evidence that could support the ET prediction. The Goshen culture, with its attendant intermediate age between Clovis and Folsom at the Mill Iron site (see Holliday and Meltzer 2010, fig. 1), as well as the stratigraphic position of a Goshen level below Folsom at the Hell Gap site and the possibility that Folsom technology derived from Goshen, not Clovis, deserves consideration (Bradley and Frison 1996; Larson, Kornfeld, and Frison 2009; Sellet, Donahue, and Hill 2009). Although no counts are available, the number of Goshen sites and Goshen points is undoubtedly lower than the number of Clovis sites and points, thereby lending support for a possible post-Clovis population crash. Admittedly, more work is needed on the chronological placement of Goshen sites and components.

Better dating of cultural stratigraphy and increased site numbers are needed before discussions about Paleoindian demographics and temporal bounds are fruitful. Regardless of the final disposition of the ET hypothesis, it has propelled the YD and Paleoindian research community into new research arenas.

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The discussion concerns the possible archaeological effects of an extraterrestrial impact on North America that could be important for our understanding of the peopling of the Americas. If there was a catastrophic impact in North America at 12.9 ka, the concept that it was a trigger for the dispersion

of human populations toward South America can be entertained. But this is dependent on the acceptance of the classic Clovis-First model, against which there is mounting evidence of populations living south of the Grande river at ca. 12.9 ka (Borrero 2008; Dillehay 2000; Steele and Politis 2009).

According to Firestone and colleagues, a decline “in some post-Clovis human populations” was predicted (2007:16,017). However, Holliday and Meltzer using the temporal distribution of projectile points, radiocarbon dates, and radiocarbon dated sites have shown this not to be the case. Also, alternative explanations for the 12.9-ka carbon-rich black layer exist (Gill et al. 2009; Haynes 2008; Marlon et al. 2009). Moreover, recent paleodemographic reconstructions using a large sample of radiocarbon dates as a proxy confirmed these patterns, showing that “The Native American population of North America underwent a slow but gradual increase from the Paleo-Indian to the Archaic periods” (Peros et al. 2010: 663).

I agree then that there is no evidence of post-Clovis population collapse. On the contrary, the evidence indicates an increase in human populations at that time (Collard, Buchanan, and Edinborough 2009). The so-called “termination” of the Clovis culture can be simply seen as a cultural transformation.

As happens with most explanations for the extinction, this one has weaknesses. Unless the impact explanation applies to South America, it is very difficult to accept it. In South America there is nothing like a carbon-rich black layer dated ca. 12.9 ka in South American sites, but the Pleistocene megamammals disappeared. It is indeed asking too much of the concept of extinction by concurrent factors—an interesting concept—to make it say that in some places the Pleistocene megamammals were wiped by an extraterrestrial impact and in others by climate or humans. The core of the “concurrent factors theories” is that different combinations of the same factors organize themselves in different ways in different places.

In the end, an event that is not known to have actually occurred was considered as the probable cause of a decline that is not documented. Anyway, as many similar theories have done in the past, this one spurred debate and research—as exemplified by Holliday and Meltzer’s paper—and for that I am thankful.

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We welcome Holliday and Meltzer’s contribution. We have published several analyses that call into question the part of Firestone et al.’s (2007) hypothesis that deals with the Pa-

leoindians (Buchanan et al. 2008; Collard, Buchanan, and Edinborough 2008) but to little effect. Firestone and colleagues continue to present their hypothesis as if it is consistent with the archaeological record (Kennett et al. 2009*b*). Hopefully, now that two senior Paleoindian archaeologists have also criticized their claims about the Paleoindians in print, Firestone and colleagues will realize they can no longer pretend everything is okay.

Regarding the specific points Holliday and Meltzer make, most of them are unproblematic in our view. Indeed, we have made several of them ourselves (Buchanan et al. 2008; Collard, Buchanan, and Edinborough 2008). However, Holliday and Meltzer are mistaken about one thing. In the section titled “Population Continuity or Discontinuity: The Radiocarbon Record,” they plot the medians and one standard deviation around the medians for a sample of radiocarbon dates and then look to see whether the dates are consistent with occupation being continuous or discontinuous across the putative extraterrestrial impact event. They argue that this approach provides a better picture of Paleoindian demography than the summed probability distribution approach we have used in our papers on Firestone and colleagues’ hypothesis (Buchanan et al. 2008; Collard, Buchanan, and Edinborough 2008). However, this is incorrect. Their approach is not only subject to the same biases as the summed probability distribution approach, it is also less accurate than the summed probability distribution approach. The reason for this is that Holliday and Meltzer’s approach assumes all years within a calibrated date’s range are equally probable when in fact different probabilities are associated with each year within that range. In contrast, the summed probability distribution approach takes into account the differences in probability among years. Thus, contrary to what Holliday and Meltzer contend, their approach can be expected to provide a poorer picture of Paleoindian demography than the summed probability distribution approach. Given that applications of the latter also do not support Firestone and colleagues’ claim that the Paleoindians were decimated at 12.9 cal BP (Buchanan et al. 2008; Collard, Buchanan, and Edinborough 2008), recognizing Holliday and Meltzer’s error for what it is does not change the basic point that the radiocarbon evidence is inconsistent with Firestone and colleagues’ hypothesis. But as we (hopefully) move into a new phase of the debate, it is important to be clear about the pros and cons of the methods used to test the hypothesis.

Our only other criticism of Holliday and Meltzer’s piece is that they overlook an additional line of evidence—the geographical distribution of radiocarbon-dated site phases across the putative extraterrestrial impact event. As Holliday and Meltzer explain, Firestone and colleagues argue that the impact occurred above the Great Lakes. A corollary of this part of the hypothesis is that the effects of the impact can be expected to have been more pronounced in the northern half of North America than in the southern half. In our 2008 paper we tested this prediction by assigning dates to blocks

defined by latitude and longitude, and evaluating whether their distribution changed before, during, and after the putative impact event (Buchanan et al. 2008). Our results were not consistent with the prediction. We found no evidence that the distribution of dated site phases changes before, during, and after 12.9 cal BP. We concluded at the time that this was another reason to be skeptical about Firestone and colleagues' hypothesis, and we are still of that opinion. In fact, we consider the lack of a geographic "signature" of the putative impact in the radiocarbon record to be particularly important, because it is inconsistent not only with the strong version of Firestone and colleagues' hypothesis in which the Paleoindians were decimated but also with a weaker version of the hypothesis in which the Paleoindians experienced only a population decline rather than being wiped out. Thus, we think the situation is even more problematic for the Paleoindian part of Firestone and colleagues' hypothesis than Holliday and Meltzer's piece suggests.

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The Kennett group's recent publication (Firestone et al. 2007) about an extraterrestrial impact leading to the Younger Dryas cooling with related megafaunal extinction and "termination" of the Clovis culture has generated a burst of refutations (Haynes 2008; Marlon et al. 2009; Paquay et al. 2009). As skeptical impact experts, paleoclimatologists have insisted on the inadequacy of the grand impact solution in elucidating the complex processes that triggered the Younger Dryas cooling. Paleontologists have consolidated a view that there was a long decline of the megafaunal populations through North America long before the Younger Dryas cooling. On the basis of refined charcoal records during deglaciation, paleoecologists have denied the occurrence of impact-initiated continental-scale wildfires at 12.9 ka. Fifteen hundred radiocarbon dates from North American sites have led anthropologists to recognize fluctuations in the growth of the Paleoindian populations. In this context, Holliday and Meltzer gave the death shot to the hypothetical demographic crisis of North American Paleoindians supposed to have resulted from the 12.9-ka impact. Their long-established knowledge of the context of Paleoindian sites throughout North America leads them to refute what are basically three major assumptions used to support the supposed termination of the Clovis archaeological culture. These authors remind how any correlation between weaponry and changes in the environment is not acceptable

in most cases in archaeology. Holliday and Meltzer insist on the necessity of a reliable chronostratigraphical frame before extrapolating the demographic trend of the Paleoindian populations based on occupational patterns. They cleverly describe how the unfettered mobility of Paleoindian groups in a relatively empty landscape has created a delicate archaeological situation, with the rare occurrence of multiple occupation sites and the near absence of long-term ones. These authors warn against using the frequency of radiocarbon ages or radiocarbon-dated sites over time for gauging population, suggesting that it is preferable to use the presence/absence of radiocarbon ages. This systematic demonstration shows there is not the slightest evidence that Paleoindians suffered a massive demographic collapse at 12.9 ka. However, Holliday and Meltzer seem to leave between the lines of the last paragraphs a sort of second chance for the impact hypothesis in case solid geological data are found; relying on the current archaeological evidence means accepting that a continent-scale catastrophe had, at the most, subtle effects on human populations. No doubt that Firestone and collaborators will strongly disagree with this perspective and continue to defend their alternative of a giant impact at 12.9 kyr BP by identifying more North American sites with the intriguing billions of nanodiamonds. But why not consider the little breccia offered by Holliday and Meltzer in their final remarks and open a clear constructive debate on a scientific basis? Retrieving impact-linked mineralogical markers from various high-resolution records of the last deglaciation is obviously the research priority. Considering the global dimension of the Younger Dryas event, exploration should be conducted far beyond the frontiers of North America. The naive view of a uniform or "universal" stratigraphic response that is probably borrowed by Holliday and Meltzer to explain the simplified perception of impact based on the K-T (Cretaceous-Tertiary boundary) event at ~65 Ma should be replaced by taking empirical approach to analyzing impact processes. Why not assume that imprints of these phenomena on ancient surfaces remain to be identified? Consider that the only known recent impact airburst, which blasted the Siberian forest in 1908 in the Tunguska region, was never investigated within this perspective. This small-scale event is certainly nothing like a continent-wide catastrophe, although the explosion was the equivalent of 3,000 Hiroshima-type atomic bombs and terrified the local population for decades. If it had hit a short time later the mysterious bolide would have destroyed Saint Petersburg and most probably changed the course of the nineteenth-century world history inducing giant fireworks and a sociopolitical shock. This cosmic explosion most probably would have been responsible for the most serious impact threat on humanity to be studied by geologists and sociologists, so why not archaeologists too? The repeated occurrence of Tunguska-class impacts is now well recognized to have occurred during human history and to have caused severe devastation. Effects on the human mind of the threat initiated by these exceptional events cannot be neglected unless we

deny the unique irrational perception of natural forces on our species in comparison to the animal world. Finding the traces of these memorable events in the most subtle expressions of the material culture should soon become the new frontier of archaeology and social anthropology. Why not perform experimental excavations at the Tunguska site in order to understand what we have missed for so many years in all kinds of contexts, including the Paleoindian sites? Searching for these impact markers in high-resolution occupation sequences will help to view these exceptional events at human-relevant scales. Having geologists work alongside archaeologists to decipher the geomorphic effects of impact events on ancient surfaces is central to expressing the memory of our ancestors, especially the Paleoindians.

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The extraterrestrial impact (ET) model (Firestone et al. 2007; Kennett et al. 2009a) is now part of an array of explanatory theories for events that occurred towards the close of the Pleistocene in North America. The model neatly explains the (abrupt) onset of the Younger Dryas, the Late Pleistocene faunal extinctions, and the collapse of post-Clovis human populations. Holliday and Meltzer have used archaeological data, namely, occupation chronologies and artifact data, to test the third tenet of this model. In testing the prediction of post-Clovis population collapse, they have added to the growing body of literature that has systematically refuted an ET impact that would have had catastrophic consequences for North America and set in train reverberations that would have been felt on a global scale (e.g., Marlon et al. 2009; Paquay et al. 2009).

The ET model is built on circumstantial evidence. The absence of an impact crater was neatly explained away by the suggestion that it was an aerial burst (Firestone et al. 2007: 16,020). Such an event would have been catastrophic for all animal and plant life across the continent (Wasson 2003). It would have left an indelible mark on the fossil record (e.g., French and Koeberl 2010). However, the archaeological records examined by Holliday and Meltzer do not show sudden or abrupt changes in assemblage composition or occupation sequences. When combined with other paleoenvironmental data, the case for an ET impact becomes borderline.

While the evidence from North America appears to be weak, would the ET impact have been visible in records such as those from Australia? Researchers have not found any unequivocal evidence for significant climatic oscillations that

were synchronous with the Younger Dryas of the Northern Hemisphere (see Turney et al. 2006; Williams et al. 2009). Similarly, there is no evidence for an increase in fire frequency or distinct changes in the archeological record that would correlate to these events. Late Pleistocene faunal extinctions in Australia were mostly complete by the onset of the Last Glacial Maximum around 30 ka (Wroe and Field 2006).

Debates concerning human arrival and impacts and megafaunal extinctions are broadly parallel for both continents. The primacy of any model seems linked to the traction it gets in the popular media. The ET impact theory conjures spectacular imagery of a cataclysmic meteor explosion and translates easily for to a general audience. Another catastrophic model—the blitzkrieg—is also simple but spectacular: humans as specialized big-game hunters obliterating a suite of big (now extinct) hairy beasts. Tim Flannery adapted Paul Martin's blitzkrieg model for the megafaunal extinctions in Australia (Flannery 1990). Flannery asserted that no evidence of humans hunting megafauna would necessarily be found because the process occurred quickly and no sites were preserved. Wroe et al. (2002:58) neatly summarized the blitzkrieg model as “spear-wielding hordes hacking their way through startled megafauna.” However, the fantastic imagery and associated rhetoric are not science. It is unfortunate that many debates concerning significant events in the past are often characterized by assertions rather than good science. We must maintain a clear perspective on what is, and what is not, supported by strong empirical evidence.

Academic agendas can sometimes fundamentally impact community views of indigenous peoples (see Head 1996; Wroe and Field 2006). As recently as March this year, the assertions of the blitzkrieg model were replayed in one of Australia's foremost newspapers—*The Australian* (March 18)—by Gary Johns, a former government minister who wrote, “Aborigines did their best to alter the environment by hunting macropods to death and burning much of Australia's forests, altering for all time the Garden of Eden.”

The patchy nature of the fossil record has hindered a comprehensive exploration of the role of people and climate in the events of the Late Pleistocene in Australia and North America. In Australia, sites documenting systematic hunting of megafauna have never have been found. Only two sites with overlaps are known (Cuddie Springs, southeastern Australia, and Nombe Rockshelter, New Guinea Highlands; Field et al. 2008). This in itself probably tells us something about the abundance of megafauna across the landscape.

The debate in Australia has been hijacked by the popular notion that sites with fossil fauna should be accepted only if they contain articulated skeletal remains (see Wroe et al. 2004), allowing some geochronologists and ecologists to place the extinctions within a window at ca. 40–50 ka BP. If the same criterion was applied to the North American record, we might well be looking at an extinction process that precedes the arrival of humans by millennia and makes irrelevant the notion of extinction caused by an ET impact, Younger Dryas, or human

activity. With only two of the numerous Late Pleistocene extinct species found in archeological sites in North America, the blitzkrieg model also faces an extinction of its own (Grayson and Meltzer 2003). The ET impact can now be added to those models struggling to find a place in discussions of the events that occurred at the close of the Pleistocene.

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The hypothesis that an extraterrestrial (ET) impact decimated huge swaths of the forests and grasslands of North America, ultimately leading to the extinction of the Pleistocene megafauna and the collapse of the Clovis culture (Firestone, West, and Warwick-Smith 2006; Firestone et al. 2007), has created a firestorm of interest among geologists and archaeologists as well as the popular media. The widespread appeal of this “otherworldly” explanation likely reflects the limitations of our established hypotheses (such as human overkill and environmental change) for explaining extinctions and culture change at the end of the Pleistocene.

I have never placed much confidence in the ET impact hypothesis, given the strength of the counterarguments: lack of evidence for continental-scale landscape burning, no evidence of temporal synchronicity in megafaunal extinction, and a limited record for a decline in post-Clovis populations. My own work (Hill et al. 2008*a*; also Guthrie 2003; Shapiro et al. 2004) suggests that the demographic and physiological shifts that likely contributed to the extinction of Ice Age megafauna represent trends that took possibly millennia to occur and certainly predate any 12.9-ka ET impact. Holliday and Meltzer successfully expose significant weaknesses in the ET impact hypothesis in a variety of theoretical, methodological, archaeological, and geomorphic issues. I strongly agree that there is little need to look beyond the Earth to explain the demise of the Clovis culture or the disappearance of large-bodied Pleistocene mammals.

Holliday and Meltzer consider and critique the archaeological methods used to study human demographic patterns for the early peopling of the Americas. In addition, their concise summary of the adaptive, geomorphological, technological, and subsistence changes (and continuities) associated with the transition from Clovis to later periods illuminates the nature of Paleoindian human-environment interactions at the Pleistocene/Holocene transition. If forced to point out a weakness, I would note only that the focus of the paper was perhaps too limited. A very nice response paper like this one could have been an exceptional paper had the authors chosen to move beyond solely criticizing a particularly problematic hypothesis and instead discussed in broader detail

issues related to larger concerns over Paleoindian demographics and land use strategies.

For example, Holliday and Meltzer correctly point out that instead of a post-Clovis decline in populations, there are hints that post-Clovis populations—especially Folsom—may actually have been larger than Clovis populations rather than much smaller as predicted by the ET impact hypothesis (at least in regions with an abundance of well-dated sites). While this raises serious doubt about the validity of the ET impact hypothesis, it leaves some important issues unanswered. Why would a population increase occur? Is it an expected outcome where populations have become regionally established and social systems become more closed? Or is it a result of some change in the Paleoindian economy associated with the increasing importance of bison hunting? The authors acknowledge that loss of megafauna such as mammoth seems to have had a limited impact on the overall diet of later Paleoindians (Hill 2007, 2008). Hunters apparently substituted bison in the dietary position formerly held by mammoths. However, such a prey selection change alone does not seem to explain why there would be a population increase. We still need to understand the mechanism of that change.

Much of the authors’ criticism of the proposed demographic trend highlighted the difficulties archaeologists face when trying to reconstruct paleodemography. They clearly show that the traditional methods used by archaeologists (all of which involve some form of counting projectile points, sites, or radiocarbon dates) have methodological or theoretical limitations. Even Holliday and Meltzer’s own analysis, based on a select sample of highly reliable radiocarbon dates from 44 sites dating to the Young Dryas Chronozone, was fraught with methodological problems, especially ambiguity in the radiocarbon calibration curve during this period and possible geographic variability in human population density. Unfortunately, readers are left feeling quite pessimistic about archaeologists’ ability to measure and understand changes in population size. I wished the discussion had been less of a criticism of the ET impact and more of a guide, based on the great experience and knowledge of these authors, to how archaeologists could more productively examine these issues.

To be clear, I very much like this paper and I am in agreement with almost all the points Holliday and Meltzer make. The ET impact hypothesis, although an innovative and creative hypothesis, currently lacks widespread supporting evidence. Holliday and Meltzer are the preeminent researchers on the Paleoindian period, and as such, their systematic evaluation of key assumptions of the ET impact hypothesis is extremely valuable. Now, surely we can put this interesting but as yet incompletely formed hypothesis to rest. Ultimately I hope that this discussion will be continued along other lines, and that the important methodological and theoretical issues raised in his paper will be used to shed more light on the complex nature of the North American landscape and its human inhabitants.

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The dramatic changes that occurred to North American climate, biota, and human cultures at the close of the Pleistocene have long fascinated scientists, and research has given rise to a number of theories seeking to explain those changes. Often such explanations are specific to one or another discipline, or at most explore the ramifications of, say, human hunting for megafaunal extinction. When the 12.9-ka extraterrestrial (ET) impact hypothesis was first presented (Firestone et al. 2007), it had immediate appeal because it offered a single causal explanation for a host of environmental and cultural phenomena that occurred at about the same time. It rapidly caught the attention of the media, while physicists, paleoecologists, geologists, and archaeologists began the slower process of examining the empirical foundations and claims of the still-evolving hypothesis. This article is one of a growing number urging caution in its uncritical acceptance.

Holliday and Meltzer begin by decoupling elements of the hypothesis, separating whether there was an ET impact from what its effects may have been on humans and the environment. As they note, others have found it difficult to replicate the physical evidence supporting the ET event, and they focus their contribution on the geoarchaeological side of the hypothesis. They convincingly demonstrate that claims the ET impact “terminated” the Clovis culture through biomass burning, megafaunal extinction, and attendant human population decline are ultimately predicated on shaky archaeological ground. In much of western North America, Folsom is demonstrably the uncontroversial successor to Clovis. Holliday and Meltzer show that in west-central North America Folsom appears to follow Clovis with little apparent temporal separation and that there are significantly larger numbers of Folsom points and Folsom sites than Clovis ones. They argue that site numbers, radiocarbon dates, and stratigraphic evidence show no clear evidence of catastrophic biomass burning and population crash but, instead, steady human population growth.

I would like to explore this matter a bit further—the archaeological situation in eastern North America—from the perspective of one whose work is in the West. The greatest concentrations, and most morphological/technological variants, of fluted points clearly lie in eastern North America. However, the typological and chronological placement of fluted point forms remains controversial, and the reliability of recognizing a fluted point as Clovis or post Clovis is challenging in the absence of stratigraphic superposition and the rarity of radiocarbon dates. It is generally assumed that these fluted forms overlap chronometrically with Clovis-Folsom in the West. Differences in blade and base shape and shaping/fluting technology have been used to define at least eight or 10

named types inferred to be Clovis or post Clovis. Parenthetically, it is worth observing that the points from the Colby site in Wyoming (Frison and Todd 1986) would almost certainly not be considered Clovis on morphological grounds had they not been found in context. This may be a valuable cautionary example in the use of morphological typology to infer age.

The eastern North American fluted points most morphologically and technologically similar to western Clovis points tend to have associated radiocarbon dates younger than Clovis points in the West. Common northeastern examples include Kings Road–Whipple, Vail–Debert, and Bull Brook–West Athens Hill, the latter two of which are dated to the 10,500–10,700 radiocarbon years BP (RCYBP; approximately 12,600–12,700 cal BP) range; Kings Road–Whipple has no reliable dates but is inferred to be slightly earlier (Bradley et al. 2008). In the Great Lakes–Midwest area, the Gainey point is thought to date to ca. 10,700 RCYBP or slightly later (Morrow and Morrow 2002*a*, 2002*b*). These ages would place these types a scant 200–300 years after the ET event, making them coeval with Folsom, and their morphology and manufacturing technology strongly implies technological continuity with Clovis.

Firestone and colleagues propose that the supposed rarity of another eastern fluted point type, Redstone (Justice 1987; Broster and Norton 1996), which they take as post Clovis, is evidence of a population crash after 12,900 cal BP. Holliday and Meltzer note that Redstone points are not radiocarbon dated, and so their chronological position is uncertain. It is reasonable to ask whether Redstone is demonstrably different either typologically or chronologically from points called Gainey, Kings Road–Whipple, Vail–Debert, and Bull Brook–West Athens Hill. A morphometric analysis by Buchanan and Hamilton (2009) of Early Paleoindian (ca. 11,500–10,500 RCYBP) fluted points (not including Redstone) from sites of known or inferred age across North America concludes that while regional morphological variation is present, it is most probably explained as the product of stochastic stylistic drift over a short temporal span. They interpret such essential similarity as the product of rapid demic diffusion. If these points are morphological/technological variants of, or slightly younger (Folsom-age equivalent) successors to Clovis, their relative abundance and broad geographic distribution suggest that the hypothesized population crash after 12,900 cal BP may be illusory in eastern North America as well.

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Some of the most interesting and important scientific questions are addressed with interdisciplinary research. Archaeologists are increasingly leading the way in this regard, ad-

addressing a wide range of historical and environmentally relevant questions from an interdisciplinary perspective (e.g., van der Leeuw and Redman 2002; Zeder, Buikstra, and van der Leeuw 2010). It is therefore surprising that Holliday and Meltzer argue that archaeology has no role to play in testing the predictions of the Younger Dryas (YD) cosmic impact hypothesis. I disagree and argue that new archaeological research will provide an essential test of the hypothesis. The YD cosmic impact hypothesis predicts a major biotic disturbance in North America at the beginning of the YD (ca. 12,900 ± 100 cal BP; Firestone et al. 2007). Reduced primary productivity, loss of many animal genera, and climate change would then be implicated in human cultural changes at this time, including resource procurement strategies, mobility patterns, and population levels, as Paleoamericans adapted to substantially altered environments. Following from this prediction, Firestone et al. (2007) hypothesized a population decline or even a hiatus in human settlement in some regions. Large numbers of archaeological sites in the early YD (ca. 12,800 and 12,400 cal BP) would be inconsistent with this element of the YD cosmic impact hypothesis and could suggest that either (1) such an event did not occur, (2) it had little effect on the biota of North America, or (3) human populations were largely unaffected by these biotic changes. Evidence for demographic continuity would force elements of the original hypothesis to be revised. If a cosmic impact event in the early YD is eventually confirmed through geological, geochemical, and other lines of empirical data, the anthropological relevance of that continuity, if established, is important. Understanding the elasticity of human response to rapid environmental change underpins the most pressing problems we currently face as a species.

So the question becomes, have Holliday and Meltzer made a case for continuity of Paleoamerican populations during the late Pleistocene? This is not an easy problem to resolve and determining whether there are archaeological sites dating to the early YD will be difficult because of radiocarbon reversals and plateaus during this critical interval (Reimer et al. 2004; but see Reimer et al. 2009 and Hua et al. 2009 for a substantial revision to the calibration curve that will force a reevaluation of the boundary between Clovis and Folsom and the age of the hypothesized cosmic impact event). The result is that calibrated distributions of existing radiocarbon dates after ca. 12,800 cal BP span up to 400 years (see figs. 1, 3). Point estimates in these distributions are invalid because they are discontinuous, and the probability of any one age within these ranges is small. This leaves open the possibility of a significant gap between Clovis and Folsom from ca. 12,800 to 12,400 cal BP, where Holliday and Meltzer place five sites with large and/or discontinuous calibrated age ranges. The uncertainties associated with calibration are compounded by major shortcomings in the post-Clovis radiocarbon record. Waters and Stafford (2007) critically assessed and redated many of the Clovis age sites presented by Holliday and Meltzer. This has not been done with the Folsom age data set, and the only

apparent selection criterion used by Holliday and Meltzer for the post-Clovis sample was analytical errors lower than 100 years. It is unclear from the paper and the data table kindly provided by the authors what type of material was dated in each case, but potential sources of error can come from old and reworked wood charcoal (Kennett et al. 2002) or problems associated with dating soil humates (Grimm, Maher, and Nelson 2009). These sources of error can be reduced by directly dating short-lived materials (e.g., carbonized seeds, twigs, animal bone) that are directly associated with archaeological assemblages. Waters and Stafford (2007) primarily redated animal bones from Clovis assemblages (using appropriate chemical preparation; see Kennett et al. 2008*a*), and this approach was used successfully by Meltzer (2006) at the Folsom type site to obtain an accurate and defensible age of 12,598–12,393 cal BP and not the 12,800 ± 80 cal BP proposed by Buchanan et al. (2008) based on outdated radiocarbon dates on wood charcoal.

I suspect that when strict criteria for accepting radiocarbon ages are applied to Holliday and Meltzer's data set, even fewer sites will exist in the early YD, and this will amplify the significant gaps already evident at the regional level in figure 3. On California's northern Channel Islands, early YD sites are absent when strict criteria are applied, and not a single site of this age exists in the state of California during this interval (Kennett et al. 2008*a*, 2009*b*). This is consistent with recent genetic evidence for a significant disruption in human populations in North America sometime in the early YD (Schroeder et al. 2009). More work is clearly needed in California and elsewhere to characterize Paleoindian population dynamics in the late Pleistocene, and testing the YD cosmic impact hypothesis provides focus for future archaeological research. In my mind this represents a productive research agenda regardless of the result.

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Through a series of publications claiming evidence for an extraterrestrial (ET) impact at the end of the Pleistocene raining calamity on central North America (centering on the Great Lakes region), Firestone and colleagues have spawned a cottage industry of refutations. Holliday and Meltzer have joined this guild. However, unlike as in most refutations, Holliday and Meltzer do not address the evidence for the ET event in this paper. Rather their goal is to assess Firestone and colleague's claim that there is evidence for Clovis demise and population decline purportedly caused by this cataclysm. Consequently, Holliday and Meltzer focus on what we know about cultural processes in the temporal vicinity before and

after the purported ET impact. There could hardly be two more qualified scientists to evaluate this argument—Holliday, a geoarcheologist, and Meltzer, a prehistorian, both with a focus on studies of the First Americans.

In general, Holliday and Meltzer are correct: there was no abrupt end to Clovis, Clovis diet breadth was broad, and there is no evidence of post-Clovis population collapse. In fact, there appears to be a population increase, there is no disruption in landscape use, and the age of the black mats (Haynes 2008) spans a great time range from much before to much after the supposed ET event as well as the Clovis period. This latter point seems to be more in the realm of evaluating the evidence of the ET event than for cultural process which Holliday and Meltzer claim they leave to others. I only have minor quibbles with Holliday and Meltzer's arguments, quibbles that go more toward archeological methodology and equifinality or the difficulty of overcoming such problems in historical sciences.

In evaluating the change to post-Clovis adaptation, Holliday and Meltzer state that in the Great Plains "bison become an increasingly more important prey species in post-Clovis times than they had been earlier." They buffer this statement with recent interpretations of bison being one of a wide variety of prey species for Paleoindians. More important, however, are two recent publications that close the Clovis/post Clovis gap with respect to bison, Murray Springs, and Jake Bluff (Bement and Carter 2005; Haynes and Huckell 2007). Although Murray Springs is not on the Plains, these sites show major Clovis emphasis on bison. When our sample of animal procurement sites is so infinitesimally small, as is the case for all Clovis sites, one wonders whether in the future we will not discover many more Clovis bison procurement sites relative to mammoth procurement sites. There are good reasons to think this may happen, not the least of which is the greater visibility of the mammoth sites and, hence, their greater chance of already having been discovered and reported.

The second quibble involves Holliday and Meltzer's attempt to discount Firestone and colleague's emphasis on discontinuity of site occupation from Clovis to post Clovis. To accomplish this, Holliday and Meltzer contrast apparent settlement strategies of Clovis and post Clovis as well as Paleoindian and post-Paleoindian periods. According to Holliday and Meltzer, "Unlike post-Paleoindian times, when favored localities were returned to on multiple occasions, Paleoindian groups had a relatively empty landscape and unfettered mobility, and rarely used the same spot twice." Although they clearly acknowledge a few repeatedly occupied Paleoindian sites (Blackwater Draw, Jake Bluff, and maybe Indian Creek with Clovis components; and Lubbock Lake, Hell Gap, and Agate Basin for post-Clovis, multiple-Paleoindian-occupation locales), there are many more sites they could have mentioned. A few of these include Indian Creek, Barton Gulch, Lindenmeier, Carter/Kerr-McGee, Jim Pits, Sewright, Allen, and Mummy Cave, and even Finley, Jurgens, and Horner bison kills apparently have multiple occupations

(Bamforth 2007; Davis, Aaberg, and Greiser 1988; Davis and Greiser 1992; Davis et al. 1989; Donahue, Carter, and Albanese 2007; Frison 1984; Frison and Todd 1987; Husted and Edgar 2002; Satterthwhite 1957; Sellet, Donahue, and Hill 2009; Wheat 1979; Wilmsen and Roberts 1978). Admittedly these are mostly northwestern Plains and Rocky Mountain localities, but other regions are not much different in this regard (e.g., Collins 1998; Collins, Hudler, and Black 2003; Driskell 1996). In other words, there may not be a great difference in reoccupation patterns of Paleoindian and later aboriginal North American sites, or at least the jury is still out on this question.

In conclusion, Firestone and colleagues arguments demonstrate the dangers of entering an unknown field without adequate background. Just as Jared Diamond (e.g., 1997, 2005), Joseph Campbell (e.g., 1959, 1983), and others who have ventured into anthropological or archeological data have done, Firestone and colleagues "cherry pick" factoids to demonstrate their points. Unfortunately, they are both unfamiliar with the data as well as with the methods and nuances necessary to interpret such data or to interpret archaeological facts. In this regard Holliday and Meltzer are another in a series of course corrections in an ever-growing field of runaway, misguided misuses of anthropology and archeology.

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Toward Multiple Working Hypotheses

The hypothesis that an extraterrestrial (ET) comet impact and its consequences triggered the onset of the Younger Dryas climate reversal (YD) at 12.9 ka, contributed to the extinction of late Pleistocene megafauna and major shifts in human adaptation, and caused the collapse of Paleoindian populations (Firestone et al. 2007; Kennett et al. 2009a) is an extraordinary claim. The idea has prompted multiple investigations into the evidence for such an impact at 12.9 ka and its potential signature or consequences, such as high concentrations of iridium (Paquay et al. 2009) or microspherules (Surovell et al. 2009b), stratigraphic sequences at geoarchaeological sites (Haynes 2008), Paleoindian demographic changes (Buchanan et al. 2008), megafaunal extinctions (Faith and Surovell 2009; Gill et al. 2009), and continent-wide fires (Marlon et al. 2009). Two of these studies produced evidence indicating widespread, catastrophic changes around the time of the YD (Faith and Surovell 2009; Haynes 2008). If such changes were found to be truly synchronous, these data would be consistent with the ET hypothesis. However, none of the other studies found support for the hypothesis.

Holliday and Meltzer now analyze the archaeological record from the terminal Pleistocene to examine the issue and again

find no support for the catastrophic consequences hypothesized by comet impact proponents. The negative results from Holliday and Meltzer's analyses thus add to the growing list of challenges to the comet impact theory. And yet, there remain some phenomena that require explanation (e.g., the origin of the "black mats"; Haynes 2008) and that invite the question, "What did actually happen at the beginning of (and during) the Younger Dryas interval?" as well as more general questions about the nature of abrupt environmental changes during the transition between glacial and interglacial conditions in North America. To address these questions, it may be helpful to remember the climatic and environmental context of the YD interval, which has been intensively studied for years before the ET impact hypothesis was advanced.

Generally speaking, deglaciation was a time of dramatic and frequent environmental changes, including abrupt shifts in climate (e.g., Alley 2000) and its controls, rapid migrations of human populations (e.g., Hamilton and Buchanan 2007), megafaunal extinctions (Haynes 2009), and massive reorganizations of vegetation and associated ecosystem processes (e.g., Williams, Shuman, and Webb 2001; Williams et al. 2004). The multiple large and sometimes abrupt changes in baseline climate conditions at this time are evidenced by hundreds of paleoenvironmental records that track changes in vegetation and fire regimes across the continent. Vegetation changes in eastern North America, for example, were significantly more rapid and widespread both at the beginning and end of the YD than at any time before or after the interval (Shuman et al. 2002; Peros et al. 2008). Relatively fast and widespread declines in large herbivore populations (Faith and Surovell 2009) contributed to dramatic shifts in both vegetation and fire regimes (Gill et al. 2009; Johnson 2009). Analyses of charcoal abundances in lake deposits indicate that fires repeatedly increased during deglaciation when climate changes were large and rapid, regardless of the direction of change (Marlon et al. 2009). The strongest evidence for synchrony in large fires occurred during the abrupt warming at the end of the interval (the same time at which projectile point technology changed, as noted by Holliday and Meltzer). All of these studies indicate climatological and ecological changes that were diachronous or time transgressive but often with evidence for particular episodes of abrupt change centered on both 12.9 ka and 11.7 ka.

While such generalizations about environmental changes do not bring us closer to explaining specific observations of abrupt changes, they do highlight the fact that the effects of the environmental changes that occurred throughout deglaciation were undoubtedly enormous and far beyond anything we have seen in the historic record. Thus, the potential impacts from these known climatic changes should at least be considered when looking for explanations of phenomena at this time (see Haynes 2008). Holliday and Meltzer make a similar point—that alternative explanations for the evidence should be considered before moving on to interpretation. For example, they note that more than one potential explanation exists for observed changes in projectile point styles, including

cultural adaptations to changing environmental conditions, a shift in style preferences, or simple coincidence. If a strong argument is to be made that point style changes were a response to environmental changes, then there should be evidence not only to support that particular claim but also to reject the potential alternative explanations. Furthermore, the experimental design of any study testing the ET impact hypothesis should consider not just a single instance of rapid environmental change during deglaciation but also multiple instances of such changes. Daniau, Harrison, and Bartlein (forthcoming), for example, found a consistent increase in biomass burning in response to multiple instances of abrupt warming over the interval between 80 and 15 ka, recasting the abrupt increases in fire at 13.2 and 11.7 ka identified by Marlon et al. (2009) as expected responses instead of exceptional events. The process of identifying and testing (or at least considering) all the potential alternative explanations for a phenomenon is what Chamberlin (1897) first described as the application of "multiple working hypotheses." Such an approach would be particularly valuable for investigating the ET impact hypothesis, especially considering the high potential for multiple interacting controls and complex interrelationships driving human-environment interactions at the time.

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The extraterrestrial (ET) impact hypothesis has generated intense scholarly debate and public interest, and it has stimulated renewed work on the more general issue of the demographic and cultural impact of the onset of the Younger Dryas (apart from whatever triggered it). This brief comment addresses the use of frequencies and presence/absences of radiocarbon-dated events as a demographic proxy in relation to that more general issue. The approach has already been used to explore the demographic impact of the Younger Dryas (YD) in Europe (Gamble et al. 2005) and seems a useful part of the archaeologist's demographic toolkit, provided we also remember the cautionary notes sounded in this target article. I therefore calibrated the 44 dates cited in the target article in OxCal (Bronk Ramsey 2009) using both the INTCAL04 (Reimer et al. 2004) and the INTCAL09 (Reimer et al. 2009) calibration curves, and estimated the average number of events that occurred in each 100-year interval across the period of interest (for methodology, see Steele 2010). I also repeated this exercise separately for the sites tagged as Clovis ($n = 10$) and as Folsom ($n = 15$) in table 2 of Meltzer and Holliday (2010), treating the dates for both Sheridan Cave and Jake Bluff as Clovis. I was unable to replicate the averages

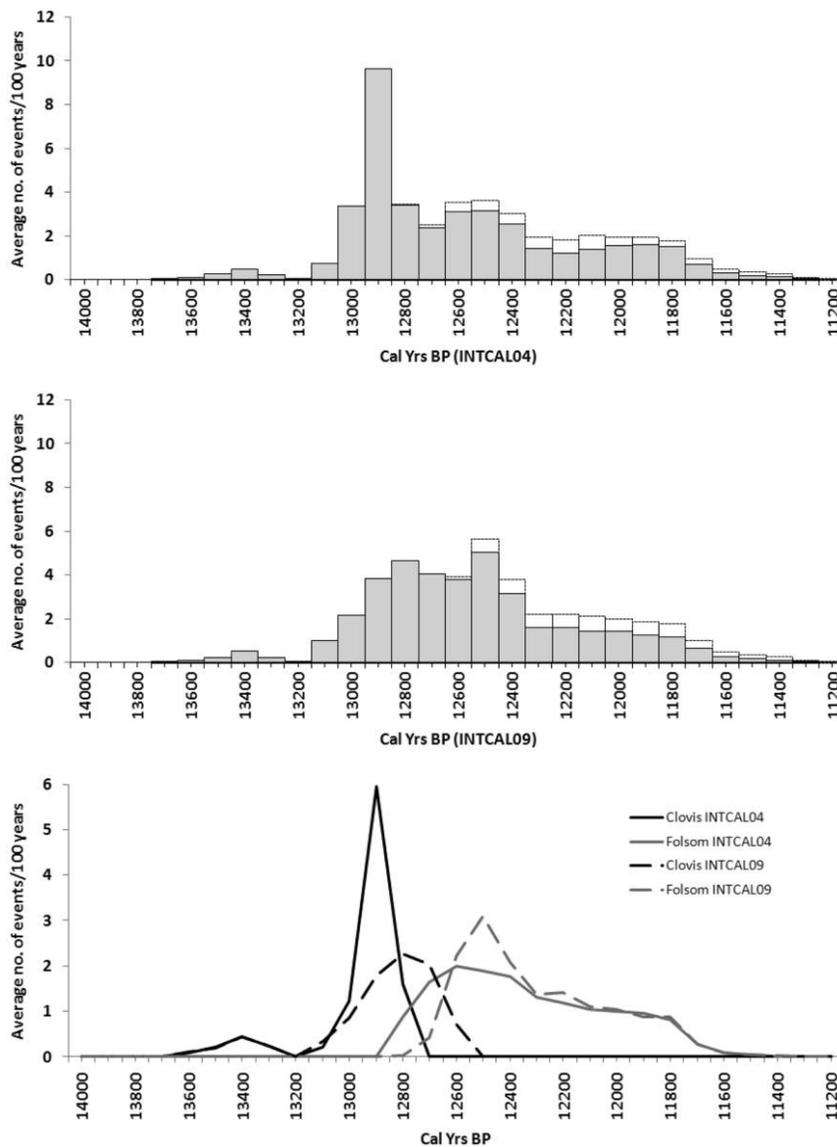


Figure 4. Effects of calibration on average numbers of events occurring in successive 100-calendar-year bins, based on 1,000 sets of random draws of single-year values for each event. Upper graph shows dates calibrated with INTCAL04; middle graph shows the same dates calibrated with INTCAL09. Dotted outlines show bar heights when the five individual sites mentioned in the commentary are retained. Lower graph shows results of a similar analysis of just the Clovis and Folsom sites using both calibration curves but plotted as a line graph for ease of overlay.

from individual dates for five sites (Kincaid, Marmes, Sunshine, Wasden, and Wilson Butte) without violating Ward and Wilson’s (1978) statistical criterion, so I have graphed results both with and without these five sites when analyzing the whole sample (but have included Kincaid and Wasden in the specifically cultural part of the analysis, in order not to allow any possible bias against high counts of Folsom dates). The results are shown in figure 4.

Some of the more obvious implications are as follows. First, and at odds with the thrust of the target article, the INTCAL04 results do indicate a marked peak in events in the century of the Younger Dryas boundary (YDB), followed by a contraction. This is no more than congruent with the data given in Meltzer and Holliday’s (2010) table 2, where 17 of the mean calibrated ages of the same 44 sites are shown to fall between 12,800 and 13,000 cal BP but only five are between 12,600

and 12,800 cal BP, albeit with large uncertainties in many individual cases. However, this result is not replicated when calibrating with INTCAL09; the peak becomes flattened and shifted forwards into the early Younger Dryas itself. Second, the temporal overlap between Clovis and Folsom remains quite small after calibrating using INTCAL09, but it is shifted forward in time by at least a century compared with the INTCAL04 results. Third, in both calibrations the set of Folsom sites extends over a larger time range than the set of Clovis sites (which themselves are spread over a somewhat longer time span in the INTCAL09 results), suggesting that greater numbers of undated Folsom artifacts could simply reflect greater longevity for that culture. Fourth, in terms of chronology Clovis still looks like a broadly peri-YDB cultural horizon, albeit now extending further into the early Younger Dryas, whereas Folsom seems to span the remainder of the Younger Dryas and to come to an end with that cold period itself. If there is a functional dimension to the Clovis/Folsom cultural transition, then a purely chronological argument can still therefore be made relating it to adaptations to changing climatic or environmental conditions.

The implications of INTCAL09 specifically for the ET impact hypothesis will need to be examined much more carefully elsewhere, as it may be that the ages of some of the potentially impact-related archaeological layers will also be affected. It is also worth noting that INTCAL09 uses only marine data for periods before 12,550 cal BP. The Huon Pine (HP-40) YD tree-ring sequence anchors the previously floating Late Glacial Pine (LGP) ^{14}C sequence (Hua et al. 2009) and supports the reduction in calendar age of radiocarbon determinations (12,900–12,550 cal BP) found using INTCAL09 (Reimer et al. 2009). However, a plot of the anchored LGP tree-ring ^{14}C sequence (Hua et al. 2009:2986) also suggests that a Pacific coral-based calibration may overestimate both that reduction in age at the younger end of the range (12,700–12,550 cal BP), and the associated uncertainty due to trends in atmospheric ^{14}C concentration. Future revisions of the calibration curve incorporating the anchored LGP tree ring ^{14}C series are therefore likely to change the picture again and may yet restore some of the amplitude of the fluctuations in early YD event densities.

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My initial reaction to this paper was that much of it is so obvious that it is rendered somewhat unnecessary. However, given not only the scientific importance of a hypothesized extraterrestrial (ET) impact at 12,900 BP in North America but also its visibility both in the academic and popular literature, assertions made by Firestone et al. (Firestone, West,

and Warwick-Smith 2006; Firestone et al. 2007) and others about the terminal Pleistocene prehistory of North America cannot go uncontested, and for this I commend Holliday and Meltzer. That said, I have doubts that the truth or fiction of Younger Dryas (YD) ET impact can be resolved in the archaeological record; its testing is much better suited to the geological record.

The Firestone et al. (2007) and Kennett and West (2008) claims of a post-Clovis population decline are troubling. While a major human population collapse at the onset of the YD would be convenient for the ET impact hypothesis, there is no compelling evidence that such a collapse ever happened. Although Martin (1973) has argued that human population should have declined following megafaunal extinction even without ET impact (see also Alroy 2001), there have been no published archaeological studies that have found serious empirical support for this claim (e.g., Buchanan et al. 2008; Peros et al. 2010). Yet over the past several months, I have been asked by several reporters about the so-called disappearance of the Clovis people, an idea about as compelling as the long-discredited notion of the disappearance of the Maya.

The primary argument made by comet proponents regarding human population decline, as reiterated by Holliday and Meltzer, is that archaeologically sterile zones often immediately overlie Clovis occupations (e.g., Kennett and West 2008). I agree with Holliday and Meltzer that there are reasonable geomorphic explanations for this phenomenon and that this observation is hardly unique to the archaeology of Clovis. Somewhat lost in the discussion is the question of how often we would expect occupations to immediately follow Clovis. Due to low human population density and the effects of taphonomic bias (Surovell and Brantingham 2007; Surovell et al. 2009a), early Paleoindian sites are extremely rare, and it would not be difficult to demonstrate that the likelihood of discovering closely timed and spatially overlapping early Paleoindian occupations is extremely low (e.g., Surovell 2009: 107–109). Thus, the lack of immediately overlying occupations at Clovis sites should be expected and not seen as somehow anomalous.

Likewise, as Holliday and Meltzer note, the notion of “major adaptive shifts” (Firestone et al. 2007:16,021) receives little empirical support. Changes in material culture at 12,900 BP do occur across the continent, but they are not striking, particularly in comparison to other shifts in the archaeological record, such as those of the Upper Paleolithic to Mesolithic or Paleoindian to Archaic periods. Given the speed and severity of the transition to YD climate coupled with the contemporaneous extinction of some 35 genera of megafauna, I would suggest that the shifts we see in human adaptation at this time are remarkably unremarkable.

One point of disagreement regards the timing of North American faunal extinctions. Holliday and Meltzer note that “it cannot be demonstrated that all 35 genera survived until the time of the supposed ET impact or even to the terminal Pleistocene.” It is true that we do not have terminal Pleis-

tocene radiocarbon dates for many genera of extinct Pleistocene megafauna, but this is easily explained by sampling phenomena. Those species that are not known from the latest Pleistocene are invariably those that are rare in the fossil record. Obtaining accurate extinction dates with small samples is very improbable, and the record as we know it is perfectly compatible with a synchronous and catastrophic mass extinction event (Faith and Surovell 2009). While sudden mass extinction could be viewed as supportive of ET impact, it is also consistent with overkill, hyperdisease, and ecological/climatic hypotheses invoking the severe and sudden climate change at the onset of the YD.

Since the YD ET impact hypothesis flashed onto the scene a few years ago, much research has followed. Holliday and Meltzer have effectively dismantled the archaeological evidence marshaled in its favor, and to date, no published studies independent of the original Firestone et al. (2007) work have found support for ET impact in the geological record. Marlon et al. (2009) found no evidence for widespread wildfires at the onset of the YD (see also van der Hammen and van Geel 2008). With colleagues, I was unable to replicate magnetic grain and microspherule results (Surovell et al. 2009b), and Paquay et al. (2009) found no evidence of an iridium anomaly. Although the YD ET impact hypothesis is far from refuted, my impression is that it could be like a meteor in the night sky. It has produced a bright flash but will disappear as quickly as it appeared.

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Considerable discussion has revolved around a possible comet impact at ca. 12,900 cal yr BP and its effect on climate, fauna, and humans (Firestone et al. 2007). Holliday and Meltzer discuss the proposition that a population decline followed the proposed 12,900 extraterrestrial event, and they conclude that no such demographic decline occurred. Arguments for and against a population increase or decrease at the onset of the Younger Dryas cannot be substantiated by the current data sets and will require reevaluation once unequivocal data are available.

Holliday and Meltzer feel that post-Clovis sites dating to the Younger Dryas are “reasonably well constrained chronologically” to allow an analysis of post-Clovis demographics. However, this is not the case. Discussion of the post-Clovis time period is hampered by an absence of good chronological control (accurate dates with high precision). Further, discussions of the age of Clovis and the immediate post-Clovis time periods are hampered by equivocal calendar-age control.

All radiocarbon dates used in Holliday and Meltzer were calibrated using CALIB 5.0.1. which is based on the IntCal04 data set (Reimer et al. 2004). Radiocarbon dates ranging from 0 to 12,400 cal yr BP are calibrated using tree-ring-derived corrections yielding true calendar dates. Radiocarbon dates that are calibrated in the range from 12,400 to 26,000 cal yr BP are calibrated using reservoir-corrected marine data sets from U-Th-dated corals and Cariaco Basin sediments (Reimer et al. 2004), yielding age estimates. The IntCal04 dataset was recently updated to IntCal09 which allows tree-ring-based calibrations to ca. 12,600 cal yr BP (Reimer et al. 2009). In the Holliday and Meltzer paper, all Clovis and the early Younger Dryas dates from archaeological sites are calibrated using the marine-based calibrations, which in this interval are dominated by Cariaco Basin data. The dates from the latter half of the Younger Dryas are calibrated using tree-ring-based calibrations. Recent papers have shown that the Cariaco Basin marine calibrations for part of the time period of interest, the early Younger Dryas, are not accurate (Hua et al. 2009; Muscheler et al. 2008). The only accurate calibrations for the early Younger Dryas time period are derived from tree-ring-based calibrations. Kromer et al. (2004) developed a tree-ring-based calibration for the late Allerød and part of the early Younger Dryas based on a floating dendrochronological sequence that they tentatively anchored to the Cariaco Basin record. The chronological position of this floating tree-ring sequence was recently modified by Hua et al. (2009). They suggest that the floating record of Kromer et al. (2004) was improperly placed and anchors the floating tree-ring record to the fixed tree-ring record using tree rings from Huon pine found in Tasmania. This link shifts the floating record 240 years younger than that proposed by Kromer et al. (2004). If this correlation is correct, a tree-ring-based calibration is now available for the Younger Dryas Chronozone.

Waters and Stafford (2007) redated the datable Clovis sites to produce accurate ages on reliable materials with high precision. A similar study is required for all post-Clovis sites dating in the Younger Dryas Chronozone. What is needed are new radiocarbon dates on reliable materials from secure contexts, in association with post-Clovis artifacts and with small standard deviations. This is especially important because of the radiocarbon plateaus in the radiocarbon calibration record of the Younger Dryas. It is critical to obtain high-precision ages if we have any hope of determining which post-Clovis age site is older than another post-Clovis age site during the Younger Dryas.

Further, we must acknowledge the limitations of the dates that are obtained from the Younger Dryas Chronozone. Many calibrated dates during this time period will have wide ranges, and the true age of a particular site could fall anywhere within this range. In other words, we cannot with any certainty determine where in the time continuum a certain site dates. Holliday and Meltzer present a box-plot of Clovis and post-Clovis sites in their figure 1, in which the error bars represent one standard deviation. This box plot is used to show that

there is no significant gap in the radiocarbon record in the post-Clovis time period. However, 16 of the earliest post-Clovis sites (60% of the sample) have age ranges such that the site could date over a 250–400-year time span. The calibrated date mean is not the age of the site. That said, it is possible that all 16 sites could date to the same time period, ca. 12,400 cal yr BP. If true, this would create a chronological gap between Clovis and the immediate post-Clovis archaeological record. I am not saying that this is true, only that this is a possibility given the limitations of age calibration. In short, we do not know where in time these sites fall within the calibrated age range, and thus we do not know the true ages of these sites and if a gap does or does not exist between Clovis and post-Clovis complexes. Until the post-Clovis Paleoindian chronological record is improved with quality data, we will be left talking in circles.

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The late-glacial/Holocene transition has captivated the imagination of geologists, paleoclimatologists, paleoecologists, and archeologists for centuries, inasmuch as its rich record describes a time of melting ice sheets, rising sea level, rapid vegetation change, megafaunal extinctions, and human migrations. It has long been known that the climate transition was neither gradual nor linear, and the most dramatic event by far was the Younger Dryas Chronozone (YDC; 12.9–12.8 to 11.6 ka), named for the arctic plant *Dryas octopetala*, whose remains were first discovered in late-glacial sediments in Sweden in 1870 (Iversen 1973). The YDC is widely recorded throughout the northern high latitudes, especially in regions bordering the North Atlantic, and it is associated with an abrupt drop in temperature of 5°C or more. Registration of a Younger Dryas cold event in the Southern Hemisphere is less certain and still much debated (see Lowell and Kelly 2008).

The hypothesis that an extraterrestrial (ET) impact over North America triggered the onset of the YDC was proposed a few years ago (Firestone, West, and Warwick-Smith 2006; Firestone et al. 2007), and ever since there has been a fireball of debate among a variety of disciplines. This is the stuff of lively symposia and great reading. Proponents claim that the impact was responsible for many of the late-glacial events—rapid collapse of the Laurentide ice sheet, a reorganization of ocean circulation, a period of widespread biomass burning, the extinction of the megafauna, declines in human population, and shifts in tool technology. The hypothesis has prompted a careful reexamination of the environmental record to see whether evidence of a catastrophe might have been

overlooked previously. The result thus far has been a better understanding of the last glacial/Holocene transition overall but little support for an ET impact (e.g., Paquay et al. 2009; Surovell et al. 2009b).

Holliday and Meltzer offer a nice addition to the growing number of papers that, while not refuting the geological claims of the hypothesis, cast doubt on its archaeological consequences. Aspects of their argument seem particularly compelling. An examination of radiocarbon dates from 44 well-dated archeological sites reveals no gap in occupation or technology during YD time, arguing against a population collapse at 12.9 ka for any reason. Neither was there widespread megafaunal extinction at the YD onset, even at the hands of climate change. And of interest to me as a nonspecialist were concerns about stratigraphic interpretations at key archeological sites. Taken together, these lines of evidence dispute the notion that an ET impact caused major upheaval among Paleoindian groups.

Two papers from the paleoecological side also cast doubt on the occurrence of an ET impact or at least its consequences. Marlon et al. (2009) examined 35 high-resolution charcoal and pollen records from North America, looking specifically for changes in fire activity and vegetation from 15–10 ka. In particular, higher-resolution charcoal records from lakes and wetlands offer information on levels of biomass burning as well as register individual fire episodes (Whitlock et al. 2008). The network of sites indicates that biomass burning increased as conditions warmed before the YDC. Fire activity leveled in the cold event and then increased again as warming resumed at 11.7 ka. Increased fires were noted at 13.9, 13.2, and 11.7 ka, in association with rapid changes in climate but not with changes in human population density or the timing of megafaunal extinction. Contrary to the ET impact hypothesis, the YDC was associated with less fire, not more. Although there was considerable variability in the charcoal record among sites, six times as many sites showed fire episodes at the end of the YDC than at the beginning (Marlon et al. 2009).

Gill et al. (2009) compared the timing of ecological change as recorded in lake sediments from Indiana and New York. The presence of a dung spore *Sporormiella* in the sediments was used to infer past presence of large herbivores in the watersheds. The absence of such spores after 13.7 ka suggested a decline in megafauna populations before their final extinction. The decline occurred during the Bølling-Allerød warm period and preceded the onset of YDC and ET event by several centuries. The authors also suggest that the decline of large herbivores may have triggered the shifts in vegetation and fire regime, not the reverse as has been often proposed.

The YDC has engendered much discussion about its cause, abruptness, global expression, and biotic consequences. Although the test of the ET impact hypothesis ultimately rests on the geologic record, much of the paleoenvironmental data from North America argue against widespread ecosystem disruption at 12.9 ka. This disparagement, however, is not meant

to detract from the importance of the late-glacial/Holocene transition as a case study for understanding rapid climate change and its attendant cultural, ecological, and environmental consequences.

Reply

We are pleased that our paper generated so many comments, all the more so that nearly all of the commentators agreed with our analysis, discussion, and conclusions, with many offering additional evidence and arguments amplifying our points. Even more welcome, criticisms raised by some of the commentators were refuted by others. Accordingly, our task in reply is made relatively easier. And since many responses hit on the same issues, rather than undertake a commentator-by-commentator reply, we'll discuss the issues raised and along the way address a number of more specific points/comments.

First and foremost, a matter of logic. As we explicitly stated in our paper, the archaeological data does not and cannot falsify the hypothesis of an extraterrestrial (ET) impact at the onset of the Younger Dryas Chronozone (YDC): only geological evidence can. This is a point on which a few of the commentators seem confused (e.g., Courty, Hill), or with which they explicitly disagree (Kennett). Kennett, in fact, finds it "surprising that Holliday and Meltzer argue that archaeology has no role to play in testing the predictions of the Younger Dryas (YD) cosmic impact hypothesis." He argues instead that the archaeological record provides "an essential test" of the ET impact hypothesis, if it can be shown that there was a "population decline or even a hiatus in human settlement in some regions" or the "termination" of Clovis culture (Firestone et al. 2007). But his arguing along these lines is an instance of the Fallacy of Affirming the Consequent, for it reasons thus:¹

If an ET impact occurred, then there would be a hiatus in Clovis settlement in some regions.

There was a hiatus in Clovis settlement in some regions.
Therefore, an ET impact occurred.

Of course, there are many earthbound reasons why changes in Clovis settlement (including a regional hiatus) might have occurred quite independent of an ET impact. In the face of such equifinality (a point echoed by Marlon and Bartlein), merely demonstrating that such changes occurred—or that there was a "termination" of Clovis culture or that subsistence patterns changed in post-Clovis times (Firestone et al. 2007: 16,021)—is not tantamount to demonstrating the earth was hit by "one or more large, low-density ET objects" (Firestone et al. 2007:16,016). Reasoning that this archaeological evi-

dence provides a "crucial test" of the ET impact hypothesis could be correct but only under very restricted circumstances, namely, that changes in Clovis settlement patterns and culture would have occurred if and only if the earth experienced an ET impact. That can hardly be the case.

Rather, as we argued, the test of an ET impact must come from direct empirical evidence of the event: that is, geological evidence of an impact such as a crater and its associated extraterrestrial debris, as is routinely required of such claims (detailed in French and Koeberl 2010). And it strikes us—and several of our commentators (Borrero, Field, Marlon and Bartlein, Whitlock)—that evidence of this sort has not been forthcoming. Indeed, our paper cited several independent studies that failed to support the ET impact hypothesis, even when analyzing evidence from some of the sites where the original data used to support the hypothesis were collected (e.g., Paquay et al. 2009; Pinter and Ishman 2008; Surovell et al. 2009b).

In the interest of fairness and to call attention to a publication not easily noticed, we note that several of the original authors have recently elaborated on their argument and evidence in the *Journal of Siberian Federal University: Engineering and Technologies* (Firestone et al. 2010). For example, they had earlier speculated that "if multiple 2-km objects struck the 2-km-thick Laurentide Ice Sheet at <30°, they may have left negligible traces after deglaciation . . . [perhaps] limited to enigmatic depressions or disturbances in the Canadian Shield (e.g., under the Great Lakes or Hudson Bay)" (Firestone et al. 2007:16,020). An obvious flaw with that speculation is that by 12,900 years ago only the Lake Superior basin was still under glacial ice (Dyke, Moore, and Robertson 2003). They now suggest "deep holes" beneath four of the Great Lakes could represent impact craters (Firestone et al. 2010: 57–58). They dismiss the possibility these holes were the result of glacial erosion, citing the latest edition of Dawson's *Acadian Geology*, a book published more than a century ago (Dawson 1891). Evidently, they believe subsequent generations of glacial and Quaternary geologists working in the Great Lakes failed to notice the holes' extraterrestrial origin. Yet if these holes were caused by an impact 12,900 years ago (and they provide no evidence the holes are that old), it is curious that the impacts produced elongated craters at different orientations, yet each one is parallel to local ice flow in the up-ice end of its lake basin. There are other attempts to bolster the impact hypothesis, but absent is any effort to respond to criticisms of the hypothesis.² But perhaps this paper was submitted before those criticisms appeared.

And more continue to appear. It is striking that in just the year since our paper was sent off for CA comment, additional

2. Apparently more impact claims are in the offing. In that same journal several of the proponents of a 12.9-ka impact separately report evidence of another meteor strike at $32,000 \pm 1,800$ years ago, which they suggest could explain the reduction in bison populations several thousand years earlier at 37,000 years ago, reported by Shapiro et al. (2004).

1. Usually synthesized: If *p*, then *q*; *q*; Therefore *p*.

independent investigations have been published calling into question the geological evidence for the supposed ET impact or reporting an inability to reproduce critical geological impact markers (French and Koeberl 2010; Haynes et al. 2010; Morrison 2010; Scott et al. 2010). French and Koeberl (2010: 153), for example, state that “none of the materials so far identified in the Younger Dryas sediments by Firestone et al. [2007] and Kennett et al. [2009a, 2009b] can be regarded as diagnostic and unarguable evidence of meteorite impact.”

When a hypothesis gains no support from and cannot have its empirical claims replicated by scientists who were not members of the original team, it seems likely, as Morrison (2010: 17) put it, that “something is amiss.” He adds the telling observation that three decades earlier the equally sensational claim that there had been an impact at the K-T boundary (~65 million years ago) was followed—literally within weeks—by independent tests that provided additional evidence and support that an impact had actually occurred (Morrison 2010:17).

Although the archaeological evidence cannot provide an essential test of the hypothesis for the reasons given, as we stated (contra Kennett), it does have a role to play if the empirical reality of an impact is demonstrated. Then it will obviously be interesting to assess what effect (if any) such an event had on human populations at the time. But as Borrero wryly observes, at the moment “an event that is not known to have actually occurred [is] considered as the probable cause of a decline that is not documented.”

To be sure, something did happen in Younger Dryas times: as Anderson observes, the “Eastern Paleoindian record is quite different a few centuries into the YD than at the onset.” The same could be said of much of the Paleoindian archaeological record across North America (though Surovell judges such changes, perhaps too strongly for our tastes, to be “remarkably unremarkable”). But that’s hardly the point. The critical issue is whether an ET impact (or even climate change) necessarily triggered those changes. The answer, of course, is no. The changes that occurred in the Paleoindian record, as noted in our paper and as several commentators likewise discuss (Borrero, Hill, Huckell, Kornfeld, Surovell), could represent several possible processes: stylistic changes in point types, adaptive changes in subsistence or settlement, demographic change (and not just population collapse, as Anderson, Kennett and others claim: such could reflect population increase and/or changes in distribution), or some combination thereof. Although Hill suggests we should have expanded our paper to focus on “larger concerns over Paleoindian demographics and land use strategies” in order to explain those changes, that was neither necessary nor the point of our paper (see Meltzer 2009).

As to whether or not a demographic collapse occurred, and Anderson’s comments notwithstanding, we remain skeptical that numbers of projectile points from poorly or undated sequences are sufficient evidence of such a collapse. In addition, we are puzzled by his blanket (and unsupported) assertion that a “decline in dated sites during the early YD has

been observed throughout much of the northern hemisphere,” especially as we provide explicit evidence that is not the case in areas such as the Plains, where the post-Clovis sequence is well documented and well dated (evidence reinforced by Hill, Kornfeld, Surovell).

Indeed, we note that Anderson described the eastern North American point sequence as “fairly well documented,” but he did not describe it as “fairly well dated,” as it clearly is not. Moreover, Huckell very effectively argues there is less to that sequence than meets the eye, he questions the claim that Clovis and its variants in the east are essentially the same age as Clovis in better-dated contexts farther west, and he offers a thoughtful critique of the use of morphological typology to infer age (which also calls into question assertions by Bement about the technological relationship of Clovis and Folsom and how Goshen technology fits in chronologically [see also Holliday 2000]). Huckell likewise remains to be convinced that Redstone points—a linchpin of the original claim for a post-Clovis population crash—are necessarily post Clovis in age. We fully agree with Huckell that to use such temporally suspect evidence to track the response to a temporally specific event is at best highly optimistic (see also Miller, Gingerich, and Johanson 2010).

But how to resolve the record of such real-time responses to a specific event? As Kennett (and others such as Steele and Waters) observes, “determining whether or not there are archaeological sites dating to the early YD will be difficult because of radiocarbon reversals and plateaus during this critical interval.” We agree and have stated so elsewhere (see Meltzer and Holliday 2010). Likewise, Kennett is correct that more AMS ages from “short-lived materials . . . that are directly associated with archaeological materials” would be helpful. Unfortunately, these appropriate cautions were far less evident in Firestone et al. 2007, the first major paper on the ET impact to which, of course, Kennett was a contributor. That paper not only paid little heed to the vagaries of radiocarbon dating during the YDC but in fact paid little heed to radiocarbon dating. As we noted, five of the 10 research sites discussed there altogether lacked chronological control (Topper, Chobot) or could hardly pass muster with the standards Kennett now demands of others (Wally’s Beach, Blackwater Draw Locality No. 1, Gainey).

Even so, we would heartily welcome the effort on the part of Kennett and other ET impact proponents to provide sites meeting those standards. More and better-dated Paleoindian sites are needed, a point echoed by Huckell, Kennett, and Waters, and we need far better calibration of these sites during the Younger Dryas Chronozone (Meltzer and Holliday 2010).

As Kennett and Waters observe, IntCal09—which was not available at the time we wrote our original paper—provides a “substantial revision to the calibration curve” for this time period. Kennett even suggests that IntCal09 will force a re-evaluation of the boundary between Clovis and Folsom and possibly amplify gaps in the early YDC. As it happens, Steele independently anticipated this suggestion and analyzed the

dates we provided using both calibration curves. In his Comment and a just-published paper (Steele 2010), he shows that there is an apparent peak followed by a sharp drop-off of ages Clovis to post-Clovis times when the ages are calibrated using IntCal04, but it becomes flattened and shifted forward when calibrated using IntCal09, effectively shrinking (not widening) the post-Clovis gap. In effect, Kennett's suspicion was incorrect, but as Steele says, this is the result obtained by the current calibration; future ones may yet again change the result.

In any event, because of the complications with calibrating radiocarbon ages from this period, along with our skepticism that frequencies of radiocarbon ages can be used as a demographic proxy, we elected to take a more conservative and simplified approach and present the radiocarbon record in terms of continuity or discontinuity on a large temporal scale, to make the point that we see no obvious break in dating of Paleoindian sites across the YDC. We are, of course, well aware of the information sacrificed when treating frequency data in nominal terms and, for that matter, that a calibrated age is not necessarily the age of a site (Waters) and that different probabilities are associated with different years within a calibration age range (Collard and Buchanan). Although Collard and Buchanan came to the same conclusion regarding Paleoindian populations by examining summed probability distributions of radiocarbon ages continent-wide (see also Peros et al. 2010), we remain skeptical of their approach. This is so partly for reasons noted already, as well as Blackwell and Buck's (2003) criticism of such efforts, namely, that "calibrated radiocarbon dates from individual, randomly surviving archaeological samples does not give a good estimate of the underlying chronological distribution." Such summed probability distributions might be more meaningful or illustrative if based in smaller regions with ample and well-dated radiocarbon records (e.g., Louderback, Grayson, and Llobera 2010; Miller, Gingerich, and Johanson 2010), and particularly—as Collard and Buchanan appropriately suggest—around "ground zero" (the Great Lakes region) of the supposed ET impact. At the moment, however, we would demur from Collard and Buchanan that the Paleoindian radiocarbon record in that area is sufficient for such a test.

We also sought evidence of continuity/discontinuity in stratigraphic records, partly in response to claims by Kennett and West (2008) that few Clovis sites also had post-Clovis components. Kornfeld believes we may have somewhat overstated the discontinuity in our figure 2, identifying Paleoindian sites with multiple occupations we "could have mentioned" and suggesting that repeated occupation of specific sites was more common than we indicated. We welcome the additional sites mentioned, though our figure 2, in fact, includes at least half of those on his list. No matter: our intent was not an exhaustive inventory but to make the point that there are a great many single-component Paleoindian sites, and thus, the absence of a successive post-Clovis occupation says nothing about "demographic collapse." Surovell further observes, and we think rightly, that "the lack of immediately

overlying occupations at Clovis sites should be expected and not seen as somehow anomalous."

Bement calls attention to the Folsom component overlying a Clovis component at the Jake Bluff site, although for reasons unclear he takes this rare occurrence (Jake Bluff is only the second such site after the Clovis type site to have these two components in situ) to signal the lack of a cultural link between Clovis and Folsom. He suggests instead that it may offer evidence of a long hiatus between occupations. Perhaps, but as we noted in our discussion of Shawnee-Minisink and other sites, the thickness of deposits is not necessarily proportional to the passage of time. Thanks to Bement we had the opportunity to visit Jake Bluff some years ago, but we observed no evidence of weathering in the deposits separating the two components. Although the geoarchaeological details from the site are as yet unpublished, that observation suggests rapid sedimentation—unsurprising in this region and topographic setting—and a relatively brief interval between kills. Ultimately, however, the point is immaterial. As we noted, thick deposits devoid of archaeological remains are not uncommon at Paleoindian sites.

Despite our having given what Courty colorfully describes as the "death shot" to the archaeological claim of a demographic crisis among North American Paleoindians, might there be nonarchaeological evidence of one? Kennett thinks so, suggesting that recent genetic work indicates a "significant disruption in human populations . . . sometime in the early YD." However, the work from which he draws that conclusion (Schroeder et al. 2009) is far more circumspect, observing that the occurrence and distribution of a particular private allele (D9S1120), which they interpret primarily as evidence of the ancestors of modern Native Americans expanding into the Americas, "should not be taken as strong evidence in favor of one particular demographic model for the peopling of the Americas" (Schroeder et al. 2009:1012). Furthermore, their estimate of the age of the appearance of this allele, figured as time to the most recent common ancestor (TMRCA), is based on simulation models (not direct dating). These yielded age estimates ranging from 7,325 to 39,900 years ago. To be sure, averaging those ages yields a result of about 12,825 years ago (Schroeder et al. 2009:1006), but as the authors freely admit, they do not "want to bring undue focus to our point estimate of 12,825 years," as "there is a considerable amount of uncertainty in our estimate for the TMRCA" (Schroeder et al. 2009:1013).

Although our attention was focused on the North American Paleoindian archaeological record, the comments from colleagues who work on late Pleistocene human records in South America (Borrero) and Australia (Field) are welcome, for they provide a larger comparative context for an event that must have had some global signature or trace. Yet neither Borrero nor Field sees corresponding changes in their respective records that signal a response to an ET impact, and indeed, Field usefully reminds us that the YDC itself was not, cli-

matically speaking, a globally synchronous episode, particularly Down Under.

Paleoecologists Marlon and Bartlein, and Whitlock, also make it clear that climatic and environmental changes during the YDC are varied and causally complicated and that the period must also be seen in the larger context of changes over time, not necessarily as a unique event requiring a unique explanation (also Borrero). This brings up the matter of terminal Pleistocene large mammal extinctions and a point on which we disagree with Surovell: that the radiocarbon record is statistically “compatible” with a synchronous and catastrophic mass extinction event may be true, but that is not evidence the process was synchronous. It also begs the question, if synchronous, then synchronous with what? Extinctions were, as Marlon and Bartlein, Whitlock, and others note, part of a vast suite of changes taking place at the end of the Pleistocene, which affected not just large mammals but also smaller ones, other large mammal species, nonmammals, and plants (e.g., Scott 2010; Semken et al. 2010). Of course, even people arrived in North America around this time. As Surovell says, and here we readily agree, if the large mammal extinctions were synchronous, such could be consistent with a range of possible causes (Borrero and Field show that such is likely in their respective regions). Equifinality, again: highlighting the analytical value, as Marlon and Bartlein remind us, of Chamberlin’s method of multiple working hypotheses to identify the appropriate causal mechanism(s).

Finally, many commentators (Collard and Buchanan, Field, Huckell, Kornfeld, Surovell) made mention—occasionally with more than a hint of irritation—of how the ET impact hypothesis has been played. Collard and Buchanan, for example, complain of the ET impact proponents’ tendency to “pretend everything is okay” despite mounting evidence it is not, while Kornfeld criticizes their propensity to “‘cherry pick’ factoids to demonstrate their points.” Field and Surovell both decry how the hypothesis has been more of a media event than a scientific one (although Morrison [2010:17] observes that the press by now seems to have lost interest, with continued support coming “mostly from blogs by catastrophists who have long advocated cosmic intervention in human history”). We are sympathetic to these concerns, have voiced some of them ourselves elsewhere (Largent 2010; Meltzer 2009), and fully agree with the sentiment, well-expressed by Field, that “we must maintain a clear perspective on what is, and what is not, supported by strong empirical evidence.”

In the end, the ET impact hypothesis will generate research into terminal Pleistocene geological, climatic, and environmental records (Anderson, Hill, Marlon and Bartlein, Whitlock), if only to put the hypothesis to the test (e.g., Paquay et al. 2009; Surovell et al. 2009b) and explore its possible consequences for other records. Of course, this is a sufficiently important period of time for a host of other reasons (Whitlock) that such research will be ongoing regardless of an ET claim. Whether that hypothesis will propel investigations into altogether new arenas (Bement) seems unlikely—at least not

unless Courty’s intriguing suggestion of examining the archaeology of the blast zone around the Tunguska impact site is undertaken, to better explore the implications/consequences of a documented ET impact on humans and the archaeological record.

So far as we are concerned, there are no compelling data to indicate North American Paleoindians had to cope with or were affected by a catastrophe, extraterrestrial or otherwise, in the terminal Pleistocene. And as to whether there was a Younger Dryas-age ET impact, like many (nearly all) of our commentators, we are dubious. However, we will leave that empirical determination to the geologists. We suspect their verdict will be in soon, for it appears the failure of this hypothesis is near.

—David J. Meltzer and Vance T. Holliday

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