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PERSPECTIVE



Disparate Perspectives on Evidence from the Cerutti Mastodon Site: A Reply to Braje et al.

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^aDepartment of Paleontology, San Diego Natural History Museum, San Diego, CA, USA; ^bCenter for American Paleolithic Research, Hot Springs, SD, USA; ^cMuseum of Paleontology, University of Michigan, Ann Arbor, MI, USA; ^dDepartment of Earth and Environmental Sciences, University of Michigan, Ann Arbor, MI, USA; ^eCentre for Archaeological Science, School of Earth and Environmental Sciences, Faculty of Science Medicine and Health, University of Wollongong, Wollongong, New South Wales, Australia; ^fGeosciences and Environmental Change Science Center, U.S. Geological Survey, Denver, CO, USA; ^gColorado Desert District, Stout Research Center, California Department of Parks and Recreation, Borrego Springs, CA, USA; ^hDepartment of Earth Science, Adams State University, Alamosa, CO, USA

ABSTRACT

The Perspective editorial by Braje, T., T. D. Dillehay, J. M. Erlandson, S. M. Fitzpatrick, D. K. Grayson, V. T. Holliday, R. L. Kelly, R. G. Klein, D. J. Meltzer, and T. C. Rick (2017). "Were Hominins in California ~130,000 Years Ago?" *PaleoAmerica* 3 (3): 200–202) takes issue with our argument [Holen, S. R., T. A. Deméré, D. C. Fisher, R. Fullagar, J. B. Paces, G. T. Jefferson, J. M. Beeton, et al. (2017). "A 130,000-Year-Old Archaeological Site in Southern California, USA." *Nature* 544 (7651): 479–483) that the assemblage of bones and stones at the Cerutti Mastodon (CM) site implicates hominin activity in site formation 130,000 years ago. Braje et al. propose instead that features of the CM site can be better explained by geological or other causes unrelated to ancient human activity. However, we contend that their conclusion reflects an incomplete assessment of our evidence. They further propose a standard of evidence at odds with current practice in the philosophy of science, and misuse a commonly quoted aphorism that "extraordinary claims require extraordinary evidence."

KEYWORDS

Cerutti mastodon site; taphonomy; early peopling of the Americas; philosophy of science; California

In their critique of our recent paper (Holen et al. 2017), Braje et al. (2017) chose not to consider the totality of evidence we presented in support of our hypothesis that humans were in coastal western North America 130,000 years ago. They examine and reinterpret certain lines of evidence that we developed, but they ignore other important features of the Cerutti Mastodon (CM) site that refute their conclusion that non-cultural processes, rather than human agency, most parsimoniously explain the data we presented. Here, we advance the discussion by specifically addressing their more serious allegations and omissions.

Regarding taphonomic control, the CM site was carefully excavated over a five-month period using strict excavation/taphonomic protocols (Holen et al. 2017, Methods), and not as a hurried "salvage paleontology project" as implied by Braje et al. (2017). This methodology allowed us to precisely plot the positions of every unearthed object > ~2 cm. We reject their assertion that the geologic data we presented are inadequate for understanding the context of the archaeological materials and reiterate that the fluvial fine silt/sand deposits exhibit an upward fining sequence typical of

overbank deposits formed along streams away from active channels (Holen et al. 2017, Supplementary Information (SI)). After explicit consideration of sedimentological evidence, surface condition of artifacts, and size ranges of bones and stones, supplemented by comparisons with skeletal distribution patterns of other mammalian fossils excavated in the same stratigraphic sequence, we found no evidence that the artifact distribution was disturbed by bioturbation or fluvial flow. Further, the local site topography and geomorphic setting exhibit low relief with no nearby slopes from which stones could fall or roll.

Braje et al. (2017) claim that we relied primarily on bone breakage experiments to support our conclusions. This misrepresents our study, where conclusions were based on multiple lines of evidence (e.g., sedimentology, use-wear, bone breakage, refits, and comparative taphonomy) – all standard archaeological methodologies. Replicative experiments were only used to understand the requirements for, and mechanics of, hammerstone percussion of proboscidean limb bones. Braje et al. (2017) asserted that we offered "no alternative hypotheses that fully assess the role of natural taphonomic

processes,” yet we specifically addressed and discounted carnivoran gnawing and trampling by large mammals as agents that could explain the CM breakage patterns (Holen et al. 2017, SI). Our experimental evidence and that of others (e.g., de la Torre et al. 2013) indicate that impact from bone breakage produces characteristic scars and abrasion marks on anvils, as found on the CM stones located at the centers of concentrations of broken fragments of bones and hammers. What is absent from Braje et al. (2017) is any testable hypothesis employing non-cultural processes to explain the breakage and distribution of the large stones present, the multiple stone refits (including small angular fragments found 3 m from the parent cobble), and wear patterns that typify human use of hammers and anvils.

Braje et al. (2017) also assert that we only argue that the CM data are *consistent* with human agency, but this ignores our discussions of alternative explanations (Holen et al. 2017, SI). We recognize that non-cultural processes can produce spiral fractures and that presence of such fractures, although *consistent* with human agency, does not by itself indicate human agency. For example, spiral fractures are reported from sites where trampling has been proposed as the causal agency (Haynes 1988). In defense of their argument concerning non-cultural production of spiral fractures, Braje et al. (2017) cite a study by McComb, Baker, and Moss (2006) focused on elephant interactions with skeletal remains of dead elephants; however, that study contains no mention of spiral fractures or any type of bone breakage. Regardless of cause, the importance of such fractures is that they indicate that breakage occurred while the bones were relatively fresh. Unfortunately, by ignoring the most important bone evidence, which includes impact features such as cone flakes, bulbs of percussion, and a large arcuate impact notch with associated negative flake scars, as well as bone distribution patterns, bone refits, and missing femoral diaphysis pieces at the CM site, Braje et al. (2017) have discarded from their analysis precisely those features that are individually and collectively least likely to have been caused by non-cultural processes. Instead, they propose an ad hoc rolling rock hypothesis (see below) that invokes a series of temporally and spatially coincident, non-cultural processes that are unlikely to co-occur. Furthermore, Braje et al. (2017) suggest that differential breakage of bone is not important. However, differential breakage where relatively fragile bones are more extensively broken than robust bones is characteristic of trampling (Haynes 1988; Holen 2006), yet this is the opposite of the pattern we reported at the CM site (fragile bones *less* thoroughly broken than robust ones). We know of no example of elephant trampling where limb bones are broken into

small spirally fractured fragments (some with evidence of impact), and yet lighter bones remain complete or more complete than robust limb bones (Haynes 1988, 1991). Because preferential breakage of fragile bones relative to robust ones is also characteristic of the action of geological processes, we considered the pattern of bone breakage observed at the CM site as consistent with human agency but inconsistent with geological processes.

Braje et al. (2017) follow a similar selective line of reasoning in their critique of our analysis of the CM cobbles. For example, their suggestion that well-rounded cobbles could “roll considerable distances” from some unspecified upland location and fortuitously end up in a low-energy depositional environment surrounded by concentrations of percussively broken limb bones, some exhibiting anvil polish, represents a highly unlikely scenario. Braje et al. (2017) offer geological explanations for individual observations but fail to provide a credible alternative hypothesis that explains the entire body of evidence at the CM site. They also dismiss our use-wear analysis, a widely used methodology that is applied across early Paleolithic to Holocene sites worldwide to determine stone tool use and site function (e.g., Clarkson et al. 2017; de la Torre et al. 2013; Yustos et al. 2015). Further, the absence of chipped stone tools is not critical to our argument for human agency (Lyman 2002) as there is no evidence of butchery or other activity at the CM site where chipped stone tools would have been necessary. Expedient hammers and anvils are the only tools needed to break mastodon bones. Chipped stone tools are not required evidence at every archaeological site, especially those involving bone processing (Holen 2006; Pobiner et al. 2008).

Regarding the apparent temporal and geographic uniqueness of the CM site, it is important to recognize that the patchy biogeographic distribution pattern of Pleistocene hominins in North America parallels the scant Paleolithic archaeological evidence in northeastern Asia (Meltzer 2003; Stanford and Bradley 2013). Even in other parts of Eurasia and Africa, the Paleolithic record contains large spatial and temporal discontinuities (Butzer 1988). In South America, the Chilean site of Monte Verde (Dillehay 1997) achieved its status as the “oldest widely accepted archaeological site in the Americas” (Braje et al. 2017, 201) despite earlier impressions that it was isolated in time and space. Geographic gaps are also a function of site-acceptance criteria, and gaps are reduced if other early South American sites are considered, such as Taima-Taima in Venezuela (Bryan et al. 1978) and Toca da Tira Peia in Brazil (Lahaye et al. 2013). To these, Dillehay et al. (2017) recently added Huaca Prieta, and Böeda et al. (2014, 2016)

added ca. 20,000- to 30,000-year-old Brazilian rockshelter deposits containing chipped stone tools exhibiting use-wear from contact with hard materials. Use-wear like that described by de la Torre et al. (2013), Yustos et al. (2015), Böeda et al. (2014, 2016), and Clarkson et al. (2017) is evidence for both chipped-stone and pounding-tool use, especially when accompanied by other features consistent with human agency. Similar use-wear patterns are present on expedient tools and associated bone fragments at the CM site. Braje et al. (2017, 201) insist that “if the antiquity of hominins in the New World is to be extended more than 110,000 years, the archaeological evidence must be unequivocal.” However, who shall decide what is “unequivocal,” and how can we defend such a threshold and still respect the process of evaluating the relative merits of competing hypotheses conditional on evidence in hand (e.g., Huelssenbeck et al. 2001)? Individual threads of evidence are rarely, if ever, “unequivocal,” but science advances nonetheless by rational weighing of available data even in the face of incomplete information.

Braje et al.’s assertion that there is no genomic evidence for such an early human migration is incorrect based on the work of Skoglund et al. (2015), although the age of the hypothesized early migration termed “Population Y” remains unknown. Conversely, Braje et al.’s claim that extinction of these early humans is “highly improbable” is unsupported and goes against basic biogeographic principles, as extinction and extirpation are common in the paleontological record, including that for the genus *Homo*. Their assertion conflicts with statements and examples cited in previous studies where it is suggested that early hominins could have become extinct in the Americas (Haynes 1967; Meltzer 1989; Merriam 1915).

In summary, Braje et al. (2017) have not presented a cogent alternative hypothesis to explain the total body of evidence described at the CM site. In that regard, the “principle of total evidence” (Good 1967), which requires consideration of all information relevant to assessment of a hypothesis, has not been honored. Instead, Braje et al. attempt to discredit our analysis by reminding readers of Sagan’s (1979) aphorism: “extraordinary claims require extraordinary evidence.” In a detailed analysis of this notion, Deming (2016, 1319) explains how “Ambiguity in what constitutes ‘extraordinary’ has led to misuse of the aphorism.” According to Deming, a claim only qualifies as extraordinary when there exists “overwhelming empirical data of the exact antithesis.” This standard is clearly not met in our case, so from Deming’s point of view, demands for extraordinary evidence for the CM site are misplaced. We maintain that choosing the best interpretation that

addresses all relevant data is the only standard that should matter. Asking for more than this would cross a line, toward arguing from a position of authority.

Disclosure statement

No potential conflict of interest was reported by the authors.

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