Experimental futures in archaeology

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Experimental archaeology can help to explain human patterns of production and discard from the Palaeolithic to historical periods, and can inform debates on topics as diverse as human migration and diet. When conducted unsystematically and used to support bold conclusions, however, experimental archaeology may quickly assume the trappings of bad science.

Drawing on experimental and archaeological data, Holen *et al.* (2017) have argued for the presence of an approximately 130 000-year-old archaeological site in California. In our recent critique (Magnani *et al.* 2019), we evaluated the experimental data used by the authors to support their claims. In considering Holen and colleagues' rebuttal (2019), we first draw attention to their openness to quantitative analysis and further experimentation. While this approach is positive, we maintain that more rigorous experimentation should have been performed before publication of the original extraordinary claims.

We agree with Eren and Bebber's (2019) succinct criticism: as with other scientific disciplines, experimental archaeology has matured. As Eren has stated elsewhere,

> *This discussion might sound axiomatic or commonsensical, but we have encountered archaeologists who think that the mere act of 'busting rocks' or using a stone tool to butcher an animal constitutes publishable research. This may have been the case at one time in the same way that the act of dissecting a mollusk would have resulted in a published biology paper 150 years ago* (Eren *et al.* 2016: 108).

It is important, however, that all archaeologists interested in drawing on experimentation keep pace with accepted best practice. Remarkable claims such as those made by Holen *et al.* (2017) must be held to the highest empirical standard.

The experiments conducted by Holen and colleagues are not unjustifiable, although they are more suitable for the early phase of study of archaeological contexts—as suggested in our original manuscript (Magnani *et al.* 2019). Preliminary observations made regarding bone-breakage patterns and hammer fracture may inform more robust experimental design. In keeping with best practice, Holen *et al.*'s (2017) experiments may serve as the basis to conduct further testing, but cannot support the claims made by the authors. With a limited sample

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size, inconsistent comparison of materials across experiments and minimal controls, it is not possible to evaluate meaningfully the experimental signatures against archaeological material and rule out alternative causes. While there is a place for visual, qualitative comparisons, these observations should not replace a more thorough evaluation of archaeological remains.

Holen *et al.*'s (2017) experiments were only one type of evidence used to support their claims. In his response, McNabb (2019) extends his analysis beyond experimental archaeology. He expands our criticism through archaeological and palaeoanthropological data, considering candidate species for the makers of the Cerutti site and evaluating migration patterns based on site distribution. While he identifies archaic *Homo sapiens* or Denisovans as potential culprits, he also notes that no accepted archaeological evidence currently supports early migrations of any hominin species to either the upper reaches of North-east Asia or any part of the Americas until the Upper Palaeolithic migrations of *Homo sapiens*.

McNabb points to faunal evidence that may also contest the conclusions reached by Holen *et al.* (2017). This includes characteristics of elephant femora, which, he states, lack large, marrow-filled cavities. Further investigation of the properties and processing of proboscidean bones may be beneficial to this debate. At the same time, McNabb deems bone-blank extraction for tool manufacture unlikely, due to the types of bone implements manufactured by other contemporaneous hominins and the presence of suitable stone for knapping around the Cerutti site. From our understanding of Holen *et al.*'s (2017) original article, we also consider it unlikely that evidence reported from the site reflects marrow or tool-blank extraction.

Finally, we draw attention to the rapidly emerging subfield of archaeogenetics, which, together with archaeological and experimental work, will continue to elucidate early human migrations. While there is no clear support from currently available genetic data—modern or ancient—for such an early hominin occupation of the Americas or adjacent North-east Asia, we should remember that this is not an absolute exclusion. There are many cases in which Pleistocene lineages of *Homo* appear not to have contributed significantly to later populations in the same regions, or where new genetic findings lend support to archaeologically unanticipated or disfavoured migration histories. One example of this is the new research by Jacobs *et al.* (2019) on archaic admixture, which increases the probability of a Denisovan presence in Wallacea, or even New Guinea.

Evoking an *American Antiquity* article by Surovell *et al.* (2017), which characterises archaeological sites as a diminishing resource, Eren and Bebber (2019) suggest an experimental future for the field. Together with archaeological and genetic evidence, experimental archaeology will play an increasingly significant role in contemporary archaeological debates. Although we currently do not accept the claims made by Holen *et al.* (2017), we remain open to reviewing additional evidence that may suggest earlier human migrations, as well as interpretations that incorporate rigorous experimentation protocols. Looking forward, we expect an experimental archaeology guided by best practice to support a complex understanding of the past, including human dispersals. Just as pilot experiments will retain a place in hypothesis generation, the archaeological community will remain responsible for critically considering and evaluating experimental practices that support archaeological interpretations.

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