The origins of the first settlers in the Americas

The recent proposal that North America was first settled by Upper Palaeolithic people from Europe who crossed the Atlantic along the edge of the Arctic ice sheet has generated considerable controversy. Here Michael O'Brien and colleagues challenge the evidence that has been presented in support of that hypothesis. There follows a response by Dennis Stanford and Bruce Bradley, and a closing reply from O'Brien et al.

Keywords: Atlantic, Last Glacial Maximum, Solutrean, Clovis, colonisation, stone tool technology

On thin ice: problems with Stanford and Bradley's proposed Solutrean colonisation of North America

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Across Atlantic ice: the origin of America's Clovis culture (Stanford & Bradley 2012) is the latest iteration of a controversial proposal that North America was first colonised by people from Europe rather than from East Asia, as most researchers accept. The authors, Dennis Stanford and Bruce Bradley, argue that Solutrean groups from southern France and the Iberian Peninsula used watercraft to make their way across the North Atlantic and into North America during the Last Glacial Maximum (LGM). According to Stanford and Bradley, this 6000km journey was facilitated by a continuous ice shelf that provided fresh water and a food supply. *Across Atlantic ice* has received a number of positive reviews. Shea (2012: 294), for example, suggests that it is "an excellent example of hypothesis-building

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in the best tradition of processual archaeology. It challenges American archaeology in a way that will require serious research by its opponents". Runnels (2012) is equally enthusiastic.

In its initial formulation, which was presented at the 1999 *Clovis and Beyond* conference and picked up by the popular press (e.g. Preston 1997; Begley & Murr 1999), the hypothesis was based primarily on similarities between the stone tools and production techniques of Solutrean people from Western Europe (*c.* 23 500–18 000 cal BP [Straus 2005]) and those of North American Clovis people (*c.* 13 300–12 800 cal BP [Haynes 2002; Collard *et al.* 2010]). Flaws in this argument were quickly pointed out. Straus (2000) and other critics noted that the existence of a several-thousand-year gap between the Solutrean and Clovis made an ancestor–descendant relationship highly improbable. They argued that similarities in tool design and production were therefore much more likely to be the result of convergence—the result of people independently developing similar ways of making stone implements. Subsequently, Stanford and Bradley revised their hypothesis in an effort to deal with the chronological gap (Stanford & Bradley 2002; Bradley & Stanford 2004). Instead of highlighting similarities between the Solutrean and Clovis, they pointed out supposed similarities among Solutrean, pre-Clovis, and Clovis tool types and production techniques (although emphasis remained upon Solutrean-Clovis similarities). It is this version of the hypothesis that is given extended treatment in *Across Atlantic ice*.

In our view, this version is no more defensible than the original one. Some of the problems were highlighted before—in some cases long before—publication of *Across Atlantic ice* and were ignored by Stanford and Bradley. One is that the ice shelf Stanford and Bradley claim facilitated the passage of Solutrean groups across the Atlantic may not have existed, or if it did, it was not as biologically rich as they suggest (Westley & Dix 2008). Obviously, if there was no ice shelf, or if the ice shelf existed but was biologically poor, the challenge for any people attempting to cross the Atlantic from Europe to North America would have been considerably greater than Stanford and Bradley aver. Another problem they ignore is that there is no evidence for anything other than opportunistic marine mammal hunting in Solutrean, Clovis, or pre-Clovis sites, despite the fact that Solutrean sites in Cantabrian Spain were located close to the LGM shoreline (Cannon & Meltzer 2004; Straus *et al.* 2005). It is unparsimonious, to say the least, to argue that Solutrean groups developed marine foraging ecological knowledge and hunting practices after leaving south-west Europe, only to abandon them upon arrival in North America. A third problem that was highlighted before publication of *Across Atlantic ice* but ignored by Stanford and Bradley is that neither the spatial gradient in Clovis-age radiocarbon dates from North America nor the pattern of variation in Clovis-point shape is consistent with Clovis having diffused from eastern North America, which is what their hypothesis predicts (Buchanan & Collard 2007; Hamilton & Buchanan 2007).

Problems with the new version of the hypothesis do not stop there. There are at least two others. One concerns the evidence that Stanford and Bradley use to build their hypothesis, and the other pertains to chronology. Again, they focus on similarities among Solutrean, pre-Clovis and Clovis tool types and production techniques, which they claim are evidence of ancestral–descendant relationships that link the three cultures such that Solutrean is the ancestor of pre-Clovis, and pre-Clovis is the ancestor of Clovis. Stanford and Bradley argue that this is supported by the results of a phenetic analysis they carried out, but that is not

the case. The reason is that phenetics—often referred to as numerical taxonomy—informs us only about the overall similarity of assemblages and not historical relatedness. Phenetics places objects in groups according to the degree to which they are alike or not alike, with no distinction made among the kinds of character states used. It neither establishes the existence of historical relationships nor demonstrates that the likeness indicates that sets of phenomena are related.

There are, however, a number of analytical approaches that were created specifically to evaluate shared ancestry, and they are seeing increasingly wide usage in anthropology and archaeology (Collard *et al.* 2006; Gray *et al.* 2007; Currie *et al.* 2010; Tehrani *et al.* 2010; O'Brien *et al.* 2012). One, maximum parsimony, is based on a model that seeks to identify the smallest number of evolutionary steps required to arrange the taxonomic units under study. Parsimony trees are evaluated on the basis of the minimum number of character-state changes required to create them, without assuming *a priori* a specific distribution of trait changes. Two other commonly used methods, maximum likelihood and Bayesian Markov Chain Monte Carlo, are probabilistically based, where the criterion for constructing trees is calculated with reference to an explicit evolutionary model from which the data are assumed to be distributed identically (Kolaczkowski & Thornton 2004).

All three methods are grounded in a model of descent with modification in which new taxa arise from the bifurcation of existing ones. The model defines ancestor–descendant relationships in terms of relative recency of common ancestry: two taxa are deemed to be more closely related to one another than either is to a third taxon if they share a common ancestor that is not also shared by the third taxon. The evidence for exclusive common ancestry is the presence of evolutionarily novel, or derived, character states. Two taxa are inferred to share a common ancestor to the exclusion of a third taxon if they exhibit derived character states that are not also exhibited by the third taxon. Phylogenetic methods are powerful tools for constructing phylogenetic orderings of anything that evolves over time, including language, cultural norms and material items. It makes little sense to ignore that tool and to rely instead on methods that are entirely agnostic on matters of ancestry.

One could, in principle, use Stanford and Bradley's data in a phylogenetic analysis, but there are problems. First, they do not list which archaeological assemblages are used in the analysis, saying only that "selected Beringian, Early Paleo-American, and Late Paleolithic European assemblages" were used (Stanford & Bradley 2012: 160). Second, they show 13 archaeological cultures in their phenograms (Stanford & Bradley 2012: figs. 6.3 and 6.4), but their tables A.2 and A.3 list presence/absence data for only nine cultures. Thus it is not possible to assess whether Stanford and Bradley's data support their hypothesis when analysed properly using the information published in *Across Atlantic ice*. We asked Stanford and Bradley for the data they used to create their phenograms several times but to no avail.

There is another reason to be sceptical about the ancestral–descendant relationships proposed by Stanford and Bradley. Recently, Eren *et al.* (2013, 2014; cf. Lohse *et al.* 2014) used a combination of experimental and archaeological analyses to evaluate the key trait that Stanford and Bradley argue supports the hypothesis that the Solutrean is ancestral to Clovis. This trait is overshot flaking, in which flakes are struck from prepared edges of a biface and travel from one edge across the face and remove a portion of the opposite margin. Eren and colleagues found that overshot flaking is most parsimoniously explained as a technological

by-product rather than a complex knapping strategy. They concluded that it therefore cannot be considered to be good evidence for an ancestral–descendant relationship between the Solutrean and Clovis. An additional reason for rejecting overshot flaking as evidence for an ancestral–descendant relationship between the Solutrean and Clovis is that virtually no evidence for it exists among any of the putative pre-Clovis assemblages. As such, even if it *were* a complex knapping strategy, it would be a convergent similarity between the Solutrean and Clovis rather than a similarity indicative of ancestor–descendant relationships.

The chronological problem with Stanford and Bradley's hypothesis is perhaps more profound than all the other problems put together. Needless to say, for the Solutrean to give rise to pre-Clovis, the Solutrean must precede it. However, if the pre-Clovis dates and artefact contexts Stanford and Bradley cite in defence of their argument were correct, then they actually *predate* the Solutrean, not the other way round. In an earlier discussion of the Solutrean hypothesis, Bradley and Stanford (2004) suggested that Solutrean sites date between 22 000 and 16 500 BP. They did not state whether those are radiocarbon years—we believe they are—but regardless, in *Across Atlantic ice* they extend the Solutrean back to *c.* 25 000 radiocarbon years BP (RCYBP) (Stanford & Bradley 2012: fig. 7.10). This adjustment was dictated by two new dates for what Stanford and Bradley call 'Solutrean-like' evidence from the Chesapeake Bay region of North America. We will discuss these dates in turn.

A key component of Stanford and Bradley's hypothesis—one that appears on the cover of the book—is a 188mm-long biface made from banded rhyolite that was dredged from the continental shelf in 1970 by the vessel *Cinmar* while harvesting deep-sea scallops 75km off the Virginia shore (Lowery 2009). Pieces of a mastodon skull, including the tusks, were dredged up with the biface. Although a description of the specific equipment in use on *Cinmar* in 1970 is lacking, the primary sea-scallop gear used in the mid-Atlantic fishery is the New Bedford-style scallop dredge, which typically is 4m wide, often used in tandem, and towed for distances of 1–9km (Stevenson *et al.* 2004). Given the scale of such dredging operations, the purported association of the biface and the mastodon tusks is dubious. We do not doubt the reported age of 22 760±90 RCYBP (UCIAMS-53545) on bone collagen from one of the mastodon tusks, but we see no reason to associate that date with the biface. Furthermore, because Stanford and Bradley (2012) use the 'association' of the tusk and the date to suggest that the *Cinmar* biface is Solutrean, rather than making a typological argument, we assume that without the association of the biface with the date, the biface could typologically be associated with any one of a number of late prehistoric periods of New England.

Discounting the tusk date as a valid age estimate for the *Cinmar* biface, the Stanford and Bradley database for a Solutrean-related pre-Clovis occupation hinges on data from Miles Point and Oyster Cove—two sites on the eastern shore of Chesapeake Bay (in Maryland, not Virginia, as Stanford and Bradley (2012: 99–100) state) that they claim (2012: 97) "should clinch the argument" for pre-Clovis on the Atlantic Coast. We do not accept that Miles Point has yielded evidence of a pre-Clovis occupation of Chesapeake Bay because it is not clear that the artefacts are in their original, undisturbed stratigraphic position. But that is beside the point. The problem for Stanford and Bradley is that even if Miles Point is considered to be good evidence of a pre-Clovis occupation of Chesapeake Bay, the dates from the site are inconsistent with their hypothesis.

The date from Miles Point used by them is BETA-236977 (21 490±140 RCYBP) (Bradley & Stanford 2004). Lowery *et al.* (2010) report that this date was obtained on organic matter recovered from a buried palaeosol overlying what is interpreted as a pre-Clovis archaeological stratum. It should be noted, however, that Lowery *et al.* obtained five additional assays—three radiocarbon dates and two OSL dates—from the same palaeosol. All five dates are at least 4000 radiocarbon years *older* than the single date Stanford and Bradley use. OSL and bulk-soil radiocarbon may exhibit systematically older biases, but Stanford and Bradley fail to discuss why they prefer the youngest of four radiocarbon dates obtained on *organic* material over the remaining two dates, both of which centre on 27 000 RCYBP.

A significantly older date for Miles Point would be problematic for Stanford and Bradley's hypothesis because, as they say, "the artifact assemblage from Miles Point includes biface projectile points, blades, scrapers, and burins that are technologically close to artifacts found in Solutrean levels 4, 5, and 6 at La Riera Cave [Cantabria, Spain] that date to 20 970±620 [RCYBP]" (2012: 183). Accepting all of the dates and the stratified context of the supposed archaeological stratum at Miles Point would mean that any evidence from that site was likely to be older than 27 000 RCYBP and perhaps older than 29 000 RCYBP—almost 10 000 years older than the 'technologically close' artefact assemblage at La Riera Cave and significantly older than the earliest Solutrean evidence in Iberia.

The single date from Oyster Cove (25 800±120 RCYBP; no laboratory number given) was obtained on a bulk soil sample from an unspecified context—though presumably from the buried palaeosol at the site—and is reported by Stanford and Bradley (2012: 100). Questions remain about this date and its association—if any—with the archaeological materials reported from the site, and about the artefacts reported from the location. First, Stanford and Bradley present drawings of two artefacts from the site, one of which appears to be a basally thinned or fluted projectile point and the other a prismatic blade. They note (2012: 100) that the projectile point was found "protruding from the Tilghman Paleosol exposed in a steeply cut bank" and the chert blade "was also recovered from the beach". Lowery *et al.* (2010: 1478) state that "non-diagnostic artifacts. . .lie on top of buried paleosols" at Oyster Cove and other sites in the region, but there is no mention of the 25 800 RCYBP date nor of the biface and blade illustrated by Stanford and Bradley. Further, Wah (2003: 91, citing a 2002 personal communication from Lowery) states that "a Late Paleoindian projectile point was also recovered from the buried surface horizon at Oyster Cove Point". Given that Late Palaeoindian point styles are well dated to *c.* 15 000 radiocarbon years younger than the 25 800 RCYBP date reported for this buried surface horizon, we would expect at least some discussion of these finds and how they relate to the reported age. In short, the archaeological significance of the reported Oyster Cove date cannot be evaluated until additional information concerning the archaeological context and stratigraphic integrity of this site is provided.

To investigate the chronological data further, we used IntCal13 (Reimer *et al.* 2013) in OxCal v.4.2 (Bronk Ramsey & Lee 2013) to calibrate the radiocarbon dates Stanford and Bradley cite. We then modelled the first- and last-appearance dates of the Solutrean in France, Portugal, Cantabrian Spain and Mediterranean Spain using Lee and Bronk Ramsey's (2012) Bayesian trapezoidal method and a total of 141 radiocarbon dates obtained from published sources by one of the present authors (LGS) via a review of the primary French,

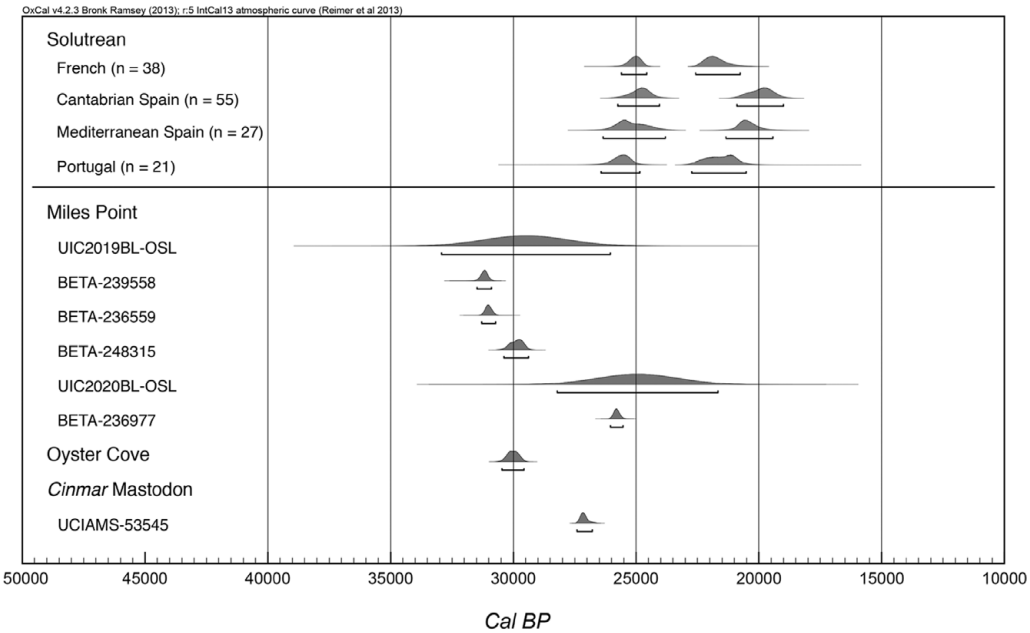

Figure 1. Modelled first- and last-appearance dates for the Solutrean based on 141 radiocarbon dates from France, Spain and Portugal, and radiocarbon dates (calibrated here) and two OSL dates from North America discussed by Stanford and Bradley (2012). Dates modelled and calibrated using IntCal13 in OxCal v.4.2 (Lee & Bronk Ramsey 2012; Bronk Ramsey & Lee 2013; Reimer et al. *2013). Horizontal lines below date curves are error bars at two standard deviations. For dates used in this analysis see online supplementary material.*

Spanish and Portuguese literatures (Burleigh *et al.* 1977; Straus 1986; Straus & González Morales 2003, 2007, 2012; Aura *et al.* 2012; see online supplementary material). Results are shown in Figure 1, together with the two OSL dates from Miles Point. The results further erode Stanford and Bradley's hypothesis. Three of the radiocarbon dates from the Miles Point palaeosol, the date from Oyster Cove, and the date from the *Cinmar* mastodon tusk are clearly *older* than the first-appearance date of the Solutrean in Europe (Figure 1). Only the youngest radiocarbon date from Miles Point overlaps with the modelled beginning of the Solutrean in Portugal and Mediterranean Spain, and only briefly. Despite the claim that the artefact assemblages from Miles Point and Oyster Cove are 'technologically close' to material from La Riera Cave in Cantabrian Spain, calibration of these dates makes it clear that the Oyster Cove date (30 460–29 570 cal BP) *predates* Cantabrian Solutrean (25 750–24 100 cal BP) by 4000–5000 years, and the single date chosen by Stanford and Bradley from Miles Point (26 050–25 550 cal BP) overlaps with the beginning of Cantabrian Solutrean for less than 200 years.

We should recall that Stanford and Bradley extend the beginning of the Solutrean to 29 500 cal BP instead of 25 600 cal BP, which is the mean first-appearance date for *any* Solutrean evidence in Europe indicated by the 141 calibrated dates. Even if we accept the extension of the Solutrean as far back as 29 500 cal BP—and, again, there is no evidence that we should—two of the three remaining radiocarbon dates from Miles Point would still be earlier than this beginning date, and the third, together with the date from

Oyster Cove, would barely overlap with it. In summary, if we are to believe that the technological similarities Stanford and Bradley observe between the Middle Atlantic and Iberia are historically related, we are forced to conclude, from their own data, that they appeared *first* in North America and then were transferred to Europe.

In conclusion, we do not share Shea's (2012: 293) opinion that if Stanford and Bradley's hypothesis is wrong, "it should be more convincingly wrong". Given that several lines of evidence have been shown to be inconsistent with the predictions of the hypothesis, and that the main lines of evidence that Stanford and Bradley use to build the hypothesis do not in fact support it, it is not clear how the hypothesis could be more convincingly wrong.

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Reply to O'Brien *et al.*

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Across Atlantic ice (AAI) sets out specifically to propose an alternative hypothesis of early human entry into the Americas and to stimulate research (Stanford & Bradley 2012: 16). O'Brien *et al.*'s response (above) primarily contains an unsupported dismissal of the evidence and these ideas. Unfortunately, to date, no work equivalent to *AAI* has been presented for any other Clovis origin hypothesis; this would be most welcome, especially from those who critique and reject the Solutrean hypothesis with unsupported assertions.

For convenience we respond to each of O'Brien *et al.*'s critiques in the order in which they appear.

In their first critique, O'Brien *et al.* again fall back on the assertion that similarities between the Solutrean and pre-Clovis/Clovis are best explained by convergence, yet no evidence has been presented to support this claim. Without evidence it is a 'just so' explanation.

In *AAI* (Stanford & Bradley 2012: 186–87) we clearly point out that the evidence for the coastal settlement model can now only be seen in the Cantabrian area since the rest of the west European continental shelf is under water. We suggest that coast-adapted people may have lived along much of the outer margin of the continental shelf. However, as the Aquitaine-Armorican-Celtic portion of the shelf turns westward, near-coastal site locations become submerged (see Stanford & Bradley 2012: fig. 8.1). An arc distance of only 1200 miles of winter–spring sea ice connected the Celtic shelf to the Late Glacial Maximum (LGM) islands of the Grand Banks at various times. North Atlantic LGM reconstructions are many and highly varied. The extent of sea ice and marine productivity is presented by most scholars as an event with 'low productivity, too cold, etc.'. In reality it was a waxing and waning process shifting over several thousand years of climate change (see Stanford & Bradley 2012: fig. 9.2). Moreover, as Westley and Dix (2008) suggest, the LGM palaeoecological evidence has not been examined in enough detail to support or invalidate our hypothesis. In a recent analysis, Vettoretti and Peltier (2013) concluded that large variations on both interannual and decadal time scales are constant features of the sea ice. This is a major step forward towards understanding what Solutrean hunters *may have* faced.

O'Brien *et al.*'s second critique addresses evidence for sea mammal hunting by Spanish Solutrean people. We did not ignore the assertion that there is "no evidence for other than opportunistic marine mammal hunting" in Cantabrian Spain. O'Brien *et al.*'s argument does not consider the issues about the possible occupation and use of the now submerged continental shelf, estimated to have covered 22 000km^2 in the Bay of Biscay (Rigaud & Simek 1990). However, Spanish Solutrean sites are not functionally 'close' to the LGM shoreline as insinuated by Cannon and Meltzer (2004). They may be close by present-day measurements, but are well beyond the normal limits of the transportation of most aquatic resources, and the artefacts used to acquire these resources by hunter-gatherer peoples

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(Erlandson 2001; Bird *et al.* 2002). Notable exceptions are aquatic remains from Solutrean levels 4 and 5 at La Riera Cave. These are the same levels that have the majority of concave base points from this site (Straus & Clark 1986). *AAI* Chapter 8 (Stanford & Bradley 2012: 186–203) discusses Solutrean maritime evidence. O'Brien *et al.* may not agree with our conclusions, but a significant amount of evidence and theory is discussed in *AAI*.

As O'Brien *et al.* point out in another critique, the evidence as to which direction fluted point technologies spread is indeed thin; whether west to east or east to west. However, the latest summary of the 'ice-free corridor' proposal for the settlement of North America clearly indicates a south to north expansion of fluted point evidence (Ives *et al.* 2013).

There may very well be better analytical methods for investigating probable historical relationships than cluster analysis. We strongly qualified the use of this method (Stanford & Bradley 2012: 152–53). Until other methods are applied to more robust data sets, the results will remain questionable. While critical of this approach, O'Brien *et al.* have not presented an alternative analysis. The lack of a list of the assemblages used in our analyses in part relates to publisher-imposed page limits as well as the intended audience of *AAI*. However, as pointed out by O'Brien *et al.*, tables A.2 and A.3 (Stanford & Bradley 2012: 256–57) do indeed lack several archaeological techno-complexes we used in our analyses. This was an editorial error. Tables 1 and 2 below present the missing information. The intimation that this was a deliberate attempt to withhold information is inaccurate.

The question of overshot flaking was also raised by O'Brien *et al.* Nowhere in any of our publications have we indicated that overshot flaking was the key technological trait linking Solutrean and Clovis. In all of our trait analyses we have given overshot flaking no more

Table 1. Typological information for four techno-complexes missing from *AAI* (Stanford & Bradley 2012: tab. A.2). 0 = absent, 1 = present.

	Late Dyuktai	Denali	F. Gravettian	F. Magdalenian
Bipointed point	1	1	0	0
Indented base point	0	0	0	0
Shouldered point	0	0	1	1
Willow leaf point	0	0	0	0
Stemmed point	0	0	1	1
Plane face point	0	0	0	0
Thin flake point	0	0	0	0
Biface knife	1	1	0	0
Side scraper	1	1	0	0
End scraper on blade	0	0	1	1
End scraper on flake	1	0	0	0
Spurred end scraper	0	0	0	0
Single graver	0	0	1	1
Multiple graver	0	0	0	1
Drill/borer	0	0	1	1
Burin	1	1	1	1
Limace	0	0	0	0
Adze	0	0	0	0

Table 2. Technological information for four techno-complexes missing from *AAI* (Stanford & Bradley 2012: tab. A.3). 0 = absent, 1 = present.

	Late Dyuktai	Denali	F. Gravettian	F. Magdalenian
Microblade	1	1	0	0
Bladelet	1	1	1	1
Large blade	1	0	1	1
Proportional biface	1	1	0	0
Thinned biface	0	0	0	0
Thickened biface	1	1	0	0
Overshot thinning	0	0	0	0
Diving thinning	0	0	0	0
Full-face thinning	0	0	0	0
Pressure edge retouch	1	1	0	0
Full facial pressure retouch	1	1	0	0
Fluting	0	0	0	0
Basal thinning	0	0	0	0
Fully bifacial precores	1	1	1	1
Flat-backed blade cores	0	0	0	0
Unidirectional blade cores	0	0	1	1
Edge grinding	0	1	0	0
Exotic material	0	0	0	1
Heat treatment	0	0	0	0
Isolated ground platforms	0	0	1	1

weight than any other technological trait (Stanford & Bradley 2012: e.g. 49–51, fig. 6.1, tab. A.3). The lack of non-Clovis controlled overshot flaking is discussed in Chapter 2, note 37 (Stanford & Bradley 2012: 264). A detailed reply to Eren *et al.* (2013) is available elsewhere (Lohse *et al.* 2014).

O'Brien *et al.* present a valid challenge to one aspect of the Solutrean hypothesis, specifically the disjuncture between *some* early radiocarbon dates from the eastern seaboard of North America and those from Solutrean sites in south-western Europe. Recent research resolves the issue of the radiocarbon dating.

A research team of the Smithsonian Institution Paleoindian Paleoecology Program, consisting of Darrin Lowery, John Wah and Dan Wagner, has been working on Quaternary geology of the Eastern Shore of the Chesapeake Bay. The development and dating of a regional occurrence of a LGM palaeosol, now called the Tilghman soil, observed in the eroded banks of the Bay, has been a major target of investigation (Lowery *et al.* 2010). To date, 22 radiocarbon dates ranging from 27 897±171 BP (30 288–29 297 cal BC (2σ)) to 17 070±180 BP (19 118–18 164 cal BC (2σ); all dates in text calibrated using OxCal v.4.3 and IntCal13 at 2σ (Bronk Ramsey 2009; Reimer *et al.* 2013)), obtained from bulk soil samples, charcoal flecks and humates taken at 10 localities are providing insights into the complex depositional history of the mid-Atlantic states of Delaware, Maryland and Virginia and the adjacent continental shelf. The Tilghman soil is mantled by a wide-spread Younger Dryas (YD) loess. Clovis artefacts commonly occur on or above an unconformity between the YD loess and the Tilghman soil. At six localities, archaeological specimens have been recovered

in situ in the soil below the YD unconformity. Three localities produced formal tools, while only blade fragments and flakes along with large stones were found at the other three sites.

The Oyster Cove site was discovered in 2002. A projectile point was found protruding from the palaeosol exposed in the bank cut and a small blade was recovered from the beach below. The projectile point was thought to be a heavily reworked late Palaeoindian point. When a date of 25 800±120 BP (28 514–27 616 cal BC) was derived from a bulk sediment analysis, we suggested that the date was probably too old and it was discounted (Stanford & Bradley 2012: 100).

At Miles Point, in 2006, an *in situ* bipolar flaking activity area was found (see Lowery *et al.* 2010: fig. 4). This comprised a bipolar quartzite blade core and two quartzite blades scattered around a large quartzite anvil with two hammerstones. In the same stratigraphic level, a projectile point and an exhausted polyhedral blade core were present. Eight additional artefacts, including a bifacial preform, a burinated biface fragment, five blades and two retouched flakes were found at the base of the Miles Point exposure. Four radiocarbon dates ranging from 27 230±230 BP (29 525–28 934 cal BC) to 21 490±140 BP (24 091–23586 cal BC) were obtained from bulk sediment and individual charcoal flecks. The Miles Point projectile point was of the same style as the one from Oyster Cove, giving added significance to the Oyster Cove point and blade.

We used the conservative 21 490±140 BP assay, which was derived from individual charcoal flecks, as the most likely date rather than the older dates because we were aware that there was a problem with the radiocarbon dates from the Tilghman soil. We were hoping that excavation at the site could resolve the problem. Unfortunately, the State of Maryland's Archaeology Compliance Officer informed the landowner that there was no such thing as a pre-Clovis site, and that phase 2 mitigation was not necessary. The landowner subsequently denied us further investigations at the site, and the state office issued a permit to have the site area buried under crushed stone. Hence, perhaps one of the most important sites yet found in North America was destroyed!

Recently, evidence from a new site found on Parson's Island in the Chesapeake Bay has helped to resolve the issues of the wide range of radiocarbon assays obtained from the Tilghman soil. There a 3m-thick exposure of the soil was capped by a 67cm-thick Younger Dryas loess dune. At least 1m of aeolian sand and a *c*. 0.5m-thick deposit of loess occur beneath the Younger Dryas boundary and overly the 4Ab1 palaeosol. Pop-down tension wedges (crack-like downward deformations), as a result of post-LGM transpression or isostatic squeezing, have been observed in the pedogenically developed aeolian sand deposits beneath the YD loess. Based on comparisons between the relative Middle Atlantic and global eustatic sea level curves, isostatic squeezing of these sediments would have occurred prior to 14 500 years ago. Isostatic depression at this time is further supported by the fact that incision into the bedrock along both the Potomac and Susquehanna rivers ceases prior to 14 400 years ago. Identification of plant remains from the soil indicates a cold environmental setting where accumulated organic material did not readily decay. The soil at Parson's Island is composed of multiple stratified organic levels: 6Ab, dated 36 308±258 BP (39 563–38 403 cal BC); 5Ab, 30 689±202 BP (33 067–32 249 cal BC); 4Ab2, 25 125±141 BP (27 585–26 857 cal BC); 4Ab1, 23 403±114 BP (25 841–25 452 cal BC). The organic

components of each level apparently became amalgamated into a single palaeosol when re-deposited downwind at most locations on the Eastern Shore. As a result, radiometric ages obtained from a bulk sample will represent an average age of the accumulated organic carbon.

Two bi-pointed laurel leaf bifaces were found *in situ* at the base of the 4Ab1 palaeosol. A date of 17 133±88 BP (18 990–18 478 cal BC) was derived from charcoal associated with one of the bifaces exposed in the profile. A third biface, along with a broken *point à face plane* and a complete projectile point, were found at the base of the exposure. The bifaces were clearly used as knives and two were heavily re-sharpened. The projectile point is identical to those from Miles Point and Oyster Cove. This assemblage indicates that Parson's Island was probably a kill/butchering site.

The date associated with the artefacts at Parson's Island is equivalent to dates from the early Solutrean occupations in northern Spain. Moreover, all of the 23 artefacts found at these sites are also found among early Spanish Solutrean collections. This artefact sample is too small for meaningful 'stone cladistics', and the artefact types and manufacturing parallels may result from independent innovation by some unknown population. It may also represent a Solutrean occupation of eastern North America. Only time and additional data will resolve this issue.

Lengthy arguments have been published as to why we interpret the Cinmar laurel leaf as being older than Clovis (Stanford & Bradley 2012: 101–103). It is possible that the biface dredged by the *Cinmar* vessel may not have been associated with the mastodon. However, the *Cinmar* was a wooden dredger built in the 1950s (Figure 1). It was smaller than modern dredgers and operated with a single winch anchored to a wooden deck. It would not have pulled heavy loads or dredged the distances indicated by O'Brien *et al.* Clearly it had not, as there was little damage to the knife from tumbling among shells, rocks and debris caught up in the dredge (Figure 2). The flake scars on the knife are crisp and microscopic striations of use-wear and hafting are preserved on the knife, all of which testifies that the knife was not pulled any great distance by the dredge and very likely came up with the mastodon bones. Furthermore, geochemical analysis indicates that both were deposited in a freshwater peat bog providing excellent preservation for the faunal remains and the knife. During meltwater pulse 1A (*c.* 14 500 years ago), both were subjected to reduction followed rapidly by oxidation when rapidly rising sea levels formed a saltwater marsh over the peat bog for a short period of time. The knife is made of a rhyolite high in iron content so if it had been exposed to salt water for an extended time it would have dissolved. Thus we can surmise that the stone knife was deposited in a peat bog on the outer continental shelf before 14 000 years ago (Stanford *et al.* 2014). It is not sufficient for O'Brien *et al.* (above) to simply assume that the Cinmar biface could be a Late Prehistoric biface; some evidence must be presented or referenced. We discussed this possibility in *AAI* (Stanford & Bradley 2012: 105) and rejected it through an extensive evaluation of collections from the eastern seaboard in which no similar bifaces were identified from any post-LGM context.

On what basis are the Miles Point artefacts rejected by O'Brien *et al.* (above) as not being "...in their original, undisturbed stratigraphic location"? Evidence must be presented and discussed in order to reject the investigator's interpretations.

Solutrean date tables are very welcome. The Eastern Shore dates are subjected to detailed contextual evaluation while the same has not been done for the Solutrean samples; they are

Figure 2. View of the Cinmar *under construction, illustrating its small size relative to modern scallop vessels.*

just accepted. A serious challenge to the validity of a vast majority of Upper Palaeolithic dates has been published (Pettitt *et al.* 2003; Higham 2011), and the validity of all dates should be subjected to the same level of scrutiny. Also, the dating discussions in *AAI* are not limited to Miles Point and Oyster Cove. Even if these ultimately prove to be suspect (see above), what about the other older-than-Clovis dates from sites such as Cactus Hill and Meadowcroft (see Collins *et al.* 2013)? The radiocarbon dates from these sites also help close the chronological gap between Solutrean and Clovis.

The date associated with the artefacts at Parson's Island is equivalent to dates from the early Solutrean occupations in northern Spain. It is, however, significant that no evidence for Clovis origins has been found during the 70 years that scholars have been searching western North America or eastern Asia. If Clovis came from Asia, why are all of the earliest sites found in the east? In the last few years, eight LGM-age sites have been found in the mid-Atlantic states. In other words, where did the people who lived along the Susquehanna River *c.* 20 000 years ago come from?

In conclusion, we welcome critical evaluations of the Solutrean hypothesis presented in *AAI*. O'Brien *et al.*'s article has made some relevant points and presented some useful challenges to move this research forward. However, it repeats a number of unsupported assertions that have already been published (see Straus *et al.* 2005). The Solutrean hypothesis is testable, it consists of real data: a specific ancestral place, a supportable timeline, and an

Figure 3. Typical dredge contents of a mid-century scallop boat.

explanation of how and why people may have explored an LGM ice edge. *Across Atlantic ice* represents our attempt to re-evaluate this hypothesis for a new generation of scholars. 'On thin ice' is another point in this debate. This debate continues with growing interest in the evidence, and demonstrates that we have accomplished one of our main goals; to reinvigorate the study of the way people came to and colonised the Americas.

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Solutreanism

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Introduction

The comments of Stanford and Bradley (above) do not address our criticisms and obfuscate the topic at hand with irrelevant data (e.g. the south-to-north movement of fluted points through the Ice Free Corridor), nonexistent data (e.g. 'under the water' or 'destroyed sites'), and questionable data (e.g. Meadowcroft and Cactus Hill are by no means widely accepted, nor are Stanford and Bradley's 'eight LGM sites' in the mid-Atlantic region). Before touching on some of these points, we direct the reader to several recent articles (e.g. Morrow 2014; Raff & Bolnick 2014) that provide new evidence or arguments inconsistent with a trans-Atlantic migration, including the fact that DNA from the Clovis Anzick child (Montana) shows no European ancestry (Rasmussen *et al.* 2014). Although Stanford and Bradley describe their Solutrean 'solution' (Stanford & Bradley 1999) to the Pleistocene colonisation of North America as 'testable', their position is that the idea is correct until falsified. They propose that their colleagues have yet to provide sufficient 'critiques' or 'challenges' to discount it (see also Collins 2012; Collins *et al.* 2013). Yet they are the ones proposing a hypothesis inconsistent with overwhelming multidisciplinary evidence, and they ignore results of tests that do not support their claims.

Rigorous standards

Stanford and Bradley assert above that "evidence must be presented and discussed in order to reject the [Miles Point] investigator's interpretations". The fact of the matter is, the Chesapeake Bay 'sites' must be evaluated using the standards warranted by the gravity of the claims being made. Given the potential importance of the sites, it should not be hard to find geoarchaeologists willing to take on that task. However, even if a pre-Clovis occupation of eastern North America is ultimately accepted, this acceptance would not support a trans-Atlantic migration. The questions of pre-Clovis and Solutrean migration are distinct and unconnected.

Radiocarbon dates

Stanford and Bradley's claim that "four radiocarbon dates . . . were obtained from bulk sediment samples" contradicts Lowery (2009). Only one date was obtained on bulk

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sediment; the remaining three were on plant material from the same context. Stanford and Bradley chose one date—21 490±140 RCYBP—and ignored the other two, which are some 5000 years older than the earliest Solutrean evidence in Europe. They state that "the validity of all dates should be subjected to the same level of scrutiny", but we did not scrutinise their dates; we calibrated and compared them. The results are unwavering: the pre-Clovis dates Stanford and Bradley use in *Across Atlantic ice* (2012) are earlier than Solutrean. Given this inescapable conclusion, the argument should be, for those so wedded to trans-Atlantic contact, that pre-Clovis people migrated eastward across the Atlantic and became the Solutrean people.

Investigating historical relationships

Stanford and Bradley state that "there may very well be better analytical methods" for investigating historical relationships. The issue is not that there are better methods; rather, the issue is that there are analytical methods for investigating historical relationships, and they did not use them.

Data sources

Stanford and Bradley provide additional data tables that are missing from *Across Atlantic ice* (Stanford & Bradley 2012) and state that in our paper we "have not presented an alternative analysis". We could not, of course, have analysed data that did not exist prior to our comment. Moreover, they do not reveal from which sites and by what means—direct assemblage analysis or literature review—they acquired their data.

Solutrean facts

There is no evidence for a Solutrean maritime adaptation. Solutrean sites such as La Riera and Altamira are within two hours' walking distance of the Late Glacial Maximum (LGM) coast but have yielded only five seal flipper bones between them. Further, there is no evidence of humans in unglaciated Brittany, southern England, Wales or Ireland during the LGM.

Even more on overshot flaking

Stanford and Bradley claim that "nowhere in any of our publications have we indicated that overshot flaking was the key technological trait linking Solutrean and Clovis"; yet they have written: "[t]he most impressive similarity is the basically identical manufacturing technology of thin bifaces using an overshot flaking method" (Bradley & Stanford 2004: 465; see also Bradley & Stanford 2006; Stanford & Bradley 2002, 2012). They cite a paper by Eren *et al.* (2013) refuting the notion of overshot flaking and mention only a response by Lohse *et al.* (2014), but tacitly ignore the concluding reply by Eren *et al.* (2014) that deconstructs the comments by Lohse *et al.*

Conclusion

There is always the chance that scientists will discover: 1) an unambiguous Solutrean site, in the right time period, on the shores or continental shelf of eastern North America; or

2) European ancient DNA in a Palaeoindian skeleton. However, hypothetical possibilities cannot be construed as facts in need of disproving. If one day there is multidisciplinary scientific evidence supporting such a scenario, we might well accept that such a migration occurred. However, in the present—the reality of the here and now—there is no evidence that supports the Solutrean 'solution'.

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