LETTER

Reply to Anderson *et al*., Jones, Kennett and West, Culleton, and Kennett *et al*.: Further evidence against the extraterrestrial impact hypothesis

Before we deal with the claims about our study (1), we want to point out that the 5 letters (2–6) are not independent. Three of them are from members of the research team whose hypothesis we tested (8). Two of the others come from a student and a collaborator of one of the aforementioned individuals, D. Kennett (3, 5). Obviously, the nonindependence of the letters does not invalidate the claims that they contain. However, it does have the potential to influence the consensus regarding the status of the extraterrestrial (ET) impact hypothesis. Hence, we hope that readers will keep in mind that the letters are the work of what is in effect a single research team and do not represent the views of multiple unconnected scholars.

Anderson *et al*. (2) contend that the results of our analyses would have been different if our dataset had included more dates from the southeastern United States. The reason for this is that they believe that they have found a gap immediately after the proposed impact in 181 dates from this region. This claim does not withstand scrutiny. Anderson *et al*. (2) find a gap because they incorrectly ignore the dates' error ranges and probability distributions. When these are taken into account there is no gap immediately after 12,900 ± 100 calendar years BP (calBP) (Fig. 1*A*). Thus, in contrast to what Anderson *et al*. (2) suggest, the 181 dates from the Southeast provide another reason to reject the ET impact hypothesis.

Anderson *et al*. (2) also contend that changes in the number of projectile points through time in the Southeast are consistent with the ET impact hypothesis. This claim rests on the assumption that full fluted points postdate the proposed impact at 12,900 ± 100 calBP. However, the few reliable dates that are associated with points in the Southeast do not support this assumption. For example, the earliest date for Dalton is ISGS-48 (8). After calibration (9, 10), the 1-sigma range for ISGS-48 is 13,041–11,396 calBP. Thus, Dalton points potentially overlap with the proposed impact. When this possibility is allowed for, changes in the number of points in the Southeast do not support the ET impact hypothesis, contrary to what Anderson *et al.* (2) suggest.

It is worth noting that, even if full fluted points are assumed to postdate the proposed impact, point counts from North America as a whole do not support Anderson *et al.*'s claim (2). In many parts of North America, Clovis is followed by a full-fluted form called Folsom. Using the Paleoindian Database of the Americas compiled by Anderson (http://pidba.utk.edu/main.htm, accessed on October 20, 2008), we calculated the number of Clovis and Folsom points recovered in the 20 U.S. states and 1 Canadian province where they co-occur. There are ≈85% more Folsom points than Clovis points (2,125 Folsom compared with 1,142 Clovis). Following Anderson *et al.*'s line of reasoning (2), this suggests that across much of North America the post-Clovis population was larger than the Clovis population. Again, this is inconsistent with the ET impact hypothesis.

Jones (3) argues that in California the small number of occupations dated to 12,900 to 10,500 calBP compared with the large number dated to 10,500 to 9,000 calBP is consistent with the ET impact hypothesis. However, the ET impact hypothesis predicts a major decline in Paleoindian population after 12,900 ± 100 calBP. Thus, to test the hypothesis it is necessary to evaluate the magnitude of the change in population size at 12,900 ± 100 calBP. Fig. 1*B* is the summed probability distribution of the 73 Californian dates on which Jones bases his argument (11). The dates neither support Jones's (3) reconstruction of Paleoindian demography in California nor show evidence of the major post-12,900 ± 100 calBP population decline predicted by the ET impact hypothesis. So, like Anderson *et al*. (2), Jones (3) provides another reason to reject the ET impact hypothesis rather than a reason to accept it.

Kennett and West (4) contend that the decline in the summed probability distribution we identified at 12,800 calBP and concluded is insignificant actually equates to a 40% reduction in population and therefore supports the ET impact hypothesis. In addition to greatly exaggerating the size of the decline, Kennett and West (4) ignore 2 crucial issues. Because Firestone *et al*. (7) do not specify the scale of the population decline beyond suggesting that it was "major," it is not possible to evaluate their hypothesis in absolute terms. Accordingly, in our study we reasoned that, if the ET impact hypothesis is correct, any decrease at 12,900 ± 100 calBP should be markedly more pronounced than other decreases in the summed probability distribution. The results of our analysis were not consistent with this prediction. The decline that occurs at 12,800 calBP is no more pronounced than some of the other declines that occur in the summed probability distribution. The other issue that Kennett and West (4) ignore is the failure of our spatial analysis of dated occupations to support the ET impact hypothesis. We compared the distribution of dated occupations before and after the proposed impact. We reasoned that, because the effects of the proposed impact can be expected to have been more pronounced in high latitudes than low latitudes, any decline at 12,900 ± 100 calBP should be associated with a change in distribution of dated occupations. Our results were not consistent with this prediction either. The distribution of dated occupations is not significantly different on either side of the proposed impact. Because the decline at 12,800 calBP is neither as exceptional as the ET impact hypothesis predicts nor accompanied by a change in distribution of dated occupations that is consistent with the hypothesis, there is no reason to conclude that it supports the hypothesis.

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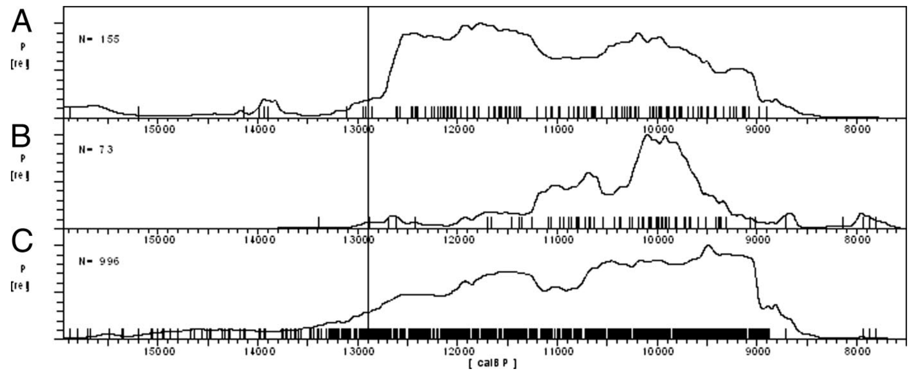

Fig. 1. Summed probability distributions of North American radiocarbon dates covering the period 16,000 calBP to 7,500 calBP. (*A*) Summed probability distribution of 155 radiocarbon dates from the southeastern United States. These are the dates from the dataset cited by Anderson *et al*. (2) that fall in the specified time range. (*B*) Summed probability distribution of 73 radiocarbon dates from California on which Jones bases his argument (11). (*C*) Summed probability distribution of 996 North American radiocarbon dates randomly sampled from Buchanan *et al*.'s (1) complete set of 1,678 dates. The black line that runs through the summed probability distributions marks the proposed ET impact event at 12,900 calBP.

Kennett and West (4) also contend that there is stratigraphic evidence for an abrupt environmental perturbation at the onset of the Younger Dryas and that this provides further support for the ET impact hypothesis. This claim is problematic too. Opinions differ regarding the relevant stratigraphic evidence. The researcher they cite, C. V. Haynes, contends that the transition from Clovis to post-Clovis coincides with stratigraphic evidence for a rapid shift to wetter conditions that occurs at the same time across North America (12). In contrast, Holliday (13) interprets the stratigraphic evidence from the Southern High Plains as indicating not only that the change from Clovis to post-Clovis involved a shift to drier conditions but also that it occurred at different times in different places. Thus, contrary to what Kennett and West (4) imply, the available stratigraphic evidence does not unambiguously support the ET impact hypothesis.

The third claim made by Kennett and West (4) concerns the occupation of Clovis sites after the end of Clovis. They point out that at a number of well-dated Clovis sites there is no evidence for post-Clovis occupation, whereas at some others there is a hiatus in occupation. They interpret this as evidence that there was a major post-Clovis decline in population, as predicted by the ET impact hypothesis. This claim ignores sites at which there is evidence for human occupation in Younger Dryas-age sediments but no evidence for Clovis (12). When these sites are taken into account, the archaeological record suggests a change in land use rather than a decline in population. Indeed, as is pointed out by C. V. Haynes in one of the articles that Kennett and West use to support their argument, the post-Clovis increase in cultural diversity and kill sites suggests that the Paleoindian population actually grew during the Younger Dryas (12).

Whereas Anderson *et al*. (2), Jones (3), and Kennett and West (4) accept that our summed probability distribution reflects Paleoindian demography but contest our interpretation of it, Culleton (5) and Kennett *et al*. (6) argue that our summed probability distribution is uninformative regarding Paleoindian demography.

Culleton (5) avers that the Paleoindians must have experienced a significant population decrease at 12,900 ± 100 calBP because this marks the onset of the Younger Dryas, the extinction of the megafauna, and the transition from Clovis to Folsom. Because such a decrease is not evident in our summed probability distribution, he argues, it must be inaccurate. He then goes on to propose reasons why our analyses were biased against the ET impact hypothesis. Culleton's reasoning is flawed. Humans are sufficiently flexible in their behavior that there is no a priori reason to expect the Paleoindians to have experienced a population decrease at 12,900 ± 100 calBP. Thus, the failure of our summed probability distribution to show that such a decrease does not constitute evidence that it is inaccurate. Culleton's claim (5) that our analyses were biased against the ET impact hypothesis is also incorrect. Our decision to pool statistically indistinguishable dates from a given occupation actually biased the analyses in favor of the hypothesis. We reran the summed probability distribution analysis with 996 dates that were randomly sampled from the complete set of 1,678 dates [996 is the maximum number of dates that can be analyzed simultaneously in CalPal (9)]. As can be seen in Fig. 1*C*, the new summed probability distribution is even less consistent with the ET impact hypothesis than the original one.

Kennett *et al*. (6) contend that our results are invalid because both our dataset and our analyses are unreliable. However, their position is not defensible. To date, neither the Firestone team (7) nor anyone else has published peer-reviewed empirical work supporting the predictions of the ET impact hypothesis regarding Paleoindian demography. Equally significantly, Kennett *et al*. (6) make no attempt to demonstrate that any of their claims are empirically valid, even though we have made our dataset freely available. In the absence of data that conflict with our dataset and/or analyses demonstrating that our dataset is problematic, there is no reason to assume that our results are invalid.

In sum, we see no reason to change our conclusions regarding the validity of the ET impact hypothesis. On the contrary, given that the authors of some of the letters have inadvertently highlighted more evidence that is inconsistent with the ET impact hypothesis, we submit that there is even less reason to accept the hypothesis now than there was before.

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The authors declare no conflict of interest.

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