Great Basin Calcite Vein and the Pleistocene Time Scale

Isaac J. Winograd *et al.* (*1*) challenge the conventional Pleistocene time scale, which is based on deep-sea-core oxygen-isotope ratios and stratigraphy (*2, 3*). Their well-dated oxygen-isotope profile from a calcite vein (DH 2) deposited by Great Basin ground water 21 meters below the water table in Devils Hole, southeastern Nevada, covers an interval from 50 to 280 ka (thousand years ago). They list possible effects that could influence the DH 2 isotope ratios and conclude that, because the oxygen-18 ($\delta^{18}O$) (*1*, p. 1276) in worldwide precipitation correlates with surface air temperatures, the peaks in the DH 2 profile are therefore associated with maxima in mean annual surface-air temperatures corresponding to interglacial and interstadial warm climates. The DH 2 profile bears a resemblance to that typically found in carbonate microfossils in deep-sea sediments, although critical data for the last glacial extreme 18 ka is missing. However, the positive $\delta^{18}O$ peaks occur substantially earlier than the interglacial peaks in deep-sea cores, the difference being about 15,000 and 20,000 years, respectively, for the DH 2 "interglacial peaks," which have an amplitude of as much as 2 per mil and precede glacial terminations II and III in the deep-sea-core record.

Alternative causes for the DH 2 data peaks are consistent with the conventional time scale and with the climatological record. We replotted the DH 2 data on a time scale (Fig. 1) that incorporates the 16,000-year ground-water residence time suggested by Winograd *et al.* We found that the two broad positive $\delta^{18}O$ peaks then closely coincide not with peaks, but rather with major minima of the deep-sea-core $\delta^{18}O$ record. These features correspond to the Illinoian glaciation of 150 ka and to the next earlier major glaciation, which occurred about 270 ka. The results of this replotting lead to the question, "What factors might have caused positive isotope-ratio peaks at the DH 2 site during such glaciations?"

A major factor in the isotope ratios is the ^{18}O enrichment of the source water vapor reaching the DH 2 site from the ocean. At glacial extremes the average ocean $\delta^{18}O$ was about 1.3 per mil more positive, as noted by Winograd *et al.*, and this difference would surely have contributed directly to the DH 2 peaks. In addition, three other positive effects are associated with a lower-latitude moisture source. At the last glacial extreme, the source vapor for the southwestern United States was drawn from lower latitude ocean surface water, as indicated by paleoclimate model results reported by the Cooperative Holocene Mapping Project group. (*4*). They show that, under the domination of the maximum continental ice sheet, the jet-stream trajectory marking the path of more prevalent winter storms into the southwestern United States 18 ka crossed the coast west of the DH 2 area at 30° to 35°. This is in contrast with the modern path, which crosses much farther north in southern Canada at about 50°N. The lower latitude ocean source during glacial times would therefore have supplied (i) vapor from water which had a $\delta^{18}O$ that was about 1.4 per mil more positive due to the lower latitude surface water composition difference (*5*); (ii) vapor with a $\delta^{18}O$ value that was 0.7 to 1.6 per mil more positive because of evaporation at the warmer temperatures (3° to 7°C) of low latitudes (*6*, figure 1; *7*); and (iii) vapor which experienced less ^{18}O depletion by fractionation because of a shorter path to the DH 2 site over lower altitude land. All these factors could have significantly enriched the Great Basin precipitation in ^{18}O at glacial extremes, perhaps by as much as 4 per mil.

On the other hand, the interpretation of Winograd *et al.* would be supported by the effects of the atmospheric absolute humidity that influence the temperature required to initiate condensation of precipitation. The lower humidity during desiccated interglacials like the present (*4, 8*) in the U.S. Southwest would result in lower condensation temperatures and a tendency toward more positive $\delta^{18}O$ precipitation values. However, measurements of deuterium isotopes from wood recovered from Holocene lake sediments in the San Juan Mountains in southern Colorado by Friedman *et al.* show a strong *negative* trend in the δD values (which generally follow $\delta^{18}O$ relations) from 9.5 ka to the present. This change, during an interval of increasing desiccation, is attributed to a probable change in moisture source, not atmospheric temperature (*9*).

We conclude that several hypotheses could explain the Devils Hole data beyond the single proposal offered by Winograd *et al.* We favor the factors enumerated above: moisture derived from relatively warm, low-latitude Pacific Ocean waters already enriched in ^{18}O, with a short travel distance of air masses to Devils Hole driven by the course of the displaced jet stream, and without the isotope fractionation that occurs today due to a longer land path and strong lifting over the western mountains. This interpretation, combined with the 16,000-year shift in the time scale to account for the ground-water residence time, would permit our revised mirror-image $\delta^{18}O$ profile for Devils Hole (Fig. 1) to support rather than oppose the chronology from deep-sea cores.

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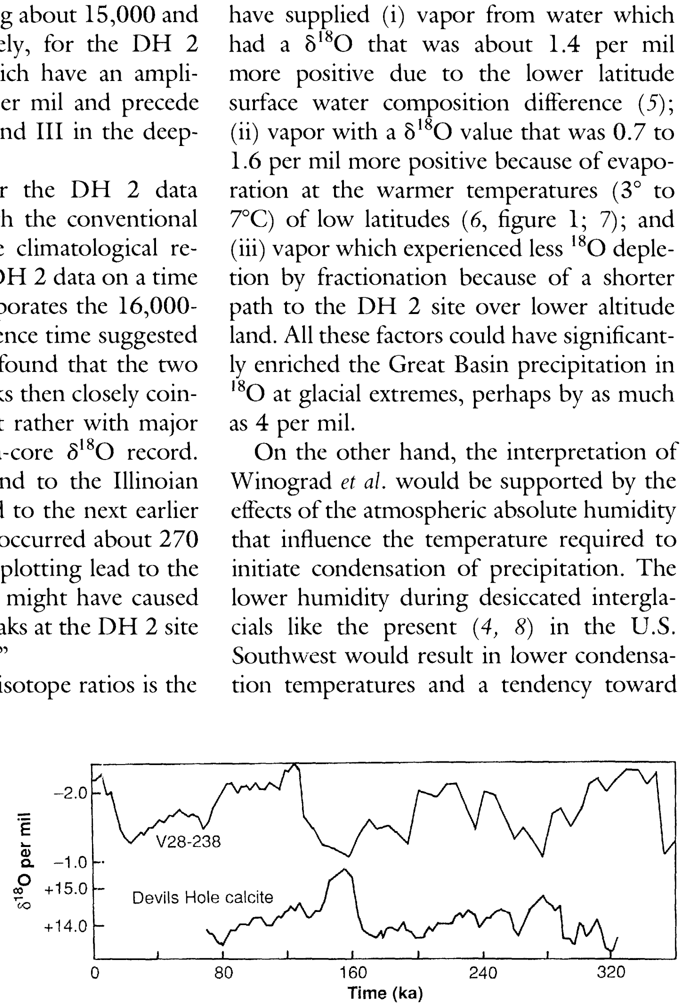

Fig. 1. Comparison of oxygen-isotope ratios from uniformly deposited tropical deep-sea core V28-238 (*10*) with those of Devils Hole (DH 2) calcite reported in (*1*). The calcite values have been displaced backwards 16,000 years on the time scale to compensate for ground-water residence time.

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Response: Johnson and Wright present an imaginative attempt to reconcile the discrepancy in the timing of glacial termination II in the Devils Hole (DH 2) and marine oxygen-18 ($\delta^{18}O$) curves. First, the preponderance of field evidence from continental environments does not support their suggestion that the peak (that is, the heaviest or most positive) $\delta^{18}O$ values in the Devils Hole curve occurred during full glacial times. Second, their explanation is internally inconsistent.

1) Johnson and Wright accept the marine and Devils Hole $\delta^{18}O$ chronologies as accurate, shift the Devils Hole record back 16,000 years to account for ground-water residence times, and then present several mechanisms to explain the occurrence of the peak $\delta^{18}O$ values at Devils Hole during full glacial times. Because the DH 2 $\delta^{18}O$ record ends at about 50 ka, Johnson and Wright and we (*1*) cite paleoclimatologic studies of termination I (that is, the late Wisconsin-Holocene transition or marine isotope stage 2/1) in an attempt to explain the variations in $\delta^{18}O$ during termination II (that is, the Illinoian-Sangamon transition or marine isotope stage 6/5). This procedure is deemed acceptable because the configuration and amplitude of the $\delta^{18}O$ curves is similar across terminations I and II in both the marine and the Vostok records (*1*, figure 4). That is, Johnson and Wright, and we, assume that whatever climatic mechanisms caused the sharp shift in $\delta^{18}O$ at termination I also caused the equally sharp shift during termination II in all three (marine, Vostok, and Devils Hole) environments.

Studies of southwestern U.S. and other mid-latitude ground waters (*2*), fluid inclusions in speleothems (*3*), and southwestern pedogenic $CaCO_3$ (*4*) indicate that the peak values of $\delta^{18}O$ (or δD) occurred during the Holocene and the lightest (that is, the least positive) values occurred during the late Wisconsin. This body of relatively well-dated continental paleoclimatic data is in direct contradiction to the contention of Johnson and Wright that heaviest $\delta^{18}O$ values in ground water at Devils Hole occurred during full glacial times.

We know of only one continental study in which heavier isotope values were reported as occurring during full glacial, rather than interglacial, times, namely, a reconnaissance study of δD in North American tree cellulose (*5*). However, deciphering the paleoclimatic meaning of isotopic variations in tree cellulose is considerably more difficult than deciphering the meaning of variations in ground waters or speleothems, as shown by subsequent studies (*6*), including two which report that heaviest tree cellulose, δD, occurred during the Holocene and that the lightest values occurred during the late Wisconsin (*7*).

Briefly, the attempt of Johnson and Wright to reconcile the differences in timing of termination II in the marine and DH 2 $\delta^{18}O$ curves, by assigning the $\delta^{18}O$ peak in DH 2 to full glacial times, is unsupported by a variety of continental paleoclimatic records. The preponderance of field evidence indicates that $\delta^{18}O$ (or δD) values in southwestern and mid-latitude continental proxy-climatic records were heaviest during the Holocene and that the values were lightest during the late Wisconsin. Similarly, for the reasons outlined above, we assume that the Sangamon interglacial and the Illinoian full-glacial ground waters, respectively, had heavy and light isotopic signatures.

2) There are also conceptual problems with the alternative explanation of Johnson and Wright. First, referring to figure 1 of Johnson and Wright, we ask, "What would prompt the sudden decline in $\delta^{18}O$ in DH 2 at about 145 ka in the midst of the near full glacial climate indicated by the marine $\delta^{18}O$ curve?" Second, the argument of Johnson and Wright contains an inconsistency. The mechanisms they invoke (in their third paragraph) to produce a peak $\delta^{18}O$ value in DH 2 during full glacial times should have been acting in the opposite direction to produce a major $\delta^{18}O$ trough during interglacial times. During interglacial times the isotopic content of the oceans would have been the lightest due to melting of the ice sheets; and, storm tracks—according to Johnson and Wright—would have crossed the west coast of the United States at 50°N, rather than at 30° to 35°N, purportedly bringing cooler and isotopically lighter precipitation to the Great Basin. But figure 1 of Johnson and Wright clearly shows that the lightest $\delta^{18}O$ in sample DH 2 do not occur beneath the 125 ka peak in the marine curve, a peak that Johnson and Wright accept as indicative of interglacial (that is, Sangamon) times; beneath the peak of the marine curve, DH 2 $\delta^{18}O$ is only halfway between its peak and its minimum values (see figure 1 of Johnson and Wright). The $\delta^{18}O$ values in the DH 2 record were lightest tens of thousands of years after the interglacial $\delta^{18}O$ peak in the marine record and, in fact, coincide closely in time with a major stadial (marine isotope stage 4) in both the marine and the Vostok records (*1*, figure 4); that is, the marine and DH 2 curves are not the mirror images suggested by Johnson and Wright in their closing paragraph. Johnson and Wright might counter the above argument by stating that isotopically heavy summer monsoonal precipitation counteracted a rapid return of DH 2 $\delta^{18}O$ to the lightest values during the Sangamon. We rejoin by pointing out, as we did in (*1*), that there is considerable evidence that summer precipitation does not contribute significantly to ground-water recharge in the major mountains of Nevada and other parts of western United States.

In summary, we see no reason to change our belief that the peak $\delta^{18}O$ values in our DH 2 record—whether reflecting relatively warm temperatures, a shift in moisture sources, or multiple causes—coincide with interglacial times, and that the marine $\delta^{18}O$ chronology appears incorrect with respect to the timing of termination II. Since publication of our paper (*1*), we have extended the Devils Hole $\delta^{18}O$ and $\delta^{13}C$ time series back to about 550 ka and have replicated our published (*1*) age for termination II.

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