

REPLY

Is There Any Evidence of Mega-Lake Manly in the Eastern Mojave Desert during Oxygen Isotope Stage 5e/6?

I am pleased to have the opportunity to defend and clarify my hypothesis regarding the extent of the Blackwelder stand of Lake Manly during marine oxygen isotope stage 6 (OS6). Let me start by addressing a couple of general points.

First, with reference to the pejorative prefix “mega” in Enzel *et al.*’s comment, let me put the size of the lake under discussion in perspective. If water were to fill Death Valley to the 90-m level today, the surface area would be ~1600 km² (Meek, 1997), whereas the area Hale proposed, based on overflow at Ash Hill pass, would have been ~8000 km² (Hale, 1985). The surface area of the OS6 lake proposed in my paper is ~2800 km², less than half the size of Hale’s lake and less than twice the size a 90-m lake would have were it to occupy the valley today.

Secondly, an issue that enters into this discussion in a couple of places is the interpretation of deposits of pale brown to pale green mud resting on alluvial substrates. Quade *et al.* (1995) have shown that many such deposits are related to springs rather than lakes. This greatly complicates delineation of the limits of pluvial lakes in these arid basins, including not only Lake Manly but also the putative Lake Dumont discussed by Anderson and Wells (1996, 1997). Indeed, most of the paleoenvironmental indicators in the sediments ascribed to Lake Dumont are indicative of “spring-supported wetlands,” not lakes (Anderson and Wells, 1997, Table 2). The sole evidence for a lake is the presence of the ostracode *Candona caudata* in one sample dated at about 18,000 ¹⁴C yr B.P. which was interpreted (Forester written communication to Anderson and Wells) as lacustrine given the stratigraphic context of the ostracode species from this through younger horizons. However, *C. caudata* also lives in springs, if a spring has sufficient flow, and in streams (R. Forester, Written communication, May 2001). For example, such springs could have existed at 18,000 ¹⁴C yr B.P., but if the gap presently cutting through the Salt Spring Hills were present then, the springs would have fed a stream through the hills, not a lake. Anderson and Wells (1997) suggested that cutting of the gap postdates 18,000 ¹⁴C yr B.P., but their only evidence for this appears to be this presence of *C. caudata* in the sediments.

Let me now respond to the remaining points in Enzel *et al.*’s comment in the order presented therein.

Age. It is not clear why Enzel *et al.* included this section, as we appear to agree on the age. However, the implication that I ignored Ku *et al.*’s (1998) U/Th dates is not correct; I referenced Lowenstein *et al.* (1999) on which Ku is a coauthor. Moreover,

the implication that Ku’s dates are more reliable than those of Hooke and Lively (see Hooke and Dorn, 1992) is debatable, given that both use the α -counting technique and thus may suffer from problems with U migration.

Salt Spring Hills shoreline. Enzel *et al.* maintain that this shoreline is cut into colluvium and, at its southeastern end, into alluvial fan deposits. I agree with the former. On the other hand, air photos, a map in Anderson and Wells (1997), and my own observations do not support the interpretation that it is cut into an alluvial fan at its southeastern end. Even if further study shows that it is, however, Enzel *et al.*’s statements about the age of the fan unit are misleading (1) because they are based on dates and stratigraphy at Silver Lake (McFadden *et al.*, 1989), which lies ~30 km south of the shoreline in question, and (2) because McFadden *et al.*’s (1989) age estimate is a *minimum* age.

Enzel *et al.* note that clasts on the shoreline bench are “unvarnished” (see Hooke, 1999, Fig. 1c). Clasts on the bench itself are also typically pebble-sized, whereas coarser clasts characterize the beach face down slope from the bench. The latter are well varnished. Lack of varnish on the bench itself may be consequence of Holocene slope processes, of the finer grain size and related increased Holocene weathering, or of reoccupation of the shoreline by the putative “Lake Dumont.”

Despite being advised in writing to the contrary, Enzel *et al.* also misrepresent my interpretation of the relation between this shoreline and the silts that lie slightly below and lakeward from it. I clearly reference (p. 329) the OS2 ¹⁴C dates obtained by Anderson and Wells (1997) from the silts, but the context makes it obvious that I think the shoreline may well have been cut earlier, during OS6. This shoreline appears to me to be too well developed to have been cut solely by a shallow and short-lived OS2 lake, if one ever existed.

Enzel *et al.* state that I “offer no new data . . . to refute the latest Pleistocene age for this shoreline.” This is also incorrect. I describe five other sites in the vicinity that are at roughly the same elevation, that have characteristics suggestive of lacustrine conditions, and that could not have formed in their Lake Dumont.

Saddle Peak Hills. This short paragraph again misrepresents the facts. As just noted, I described five sites in this area, only two of which were originally described by Butler and Mount.

Silver Lake shorelines. These shorelines are not relevant. They are known to be younger than OS6 (Ore and Warren, 1971).

Soda Lake core. We basically agree on the probable age of the uppermost ~35 m of sediment in Soda Lake, although Enzel *et al.*'s reference to "numerous" ^{14}C dates between 9330 and 20,320 ^{14}C yr B.P. is seriously misleading, as none of these dates is from Soda Lake.

Enzel *et al.*'s reference to 19 cores and 13 boreholes drilled in the Silver/Soda Lake playas since the Muessig *et al.* (1957) publication is also both erroneous and misleading. First, Brown and Rosen (1995), citing Wells *et al.* (1989) as the source for their information, state that these numbers include the five cores described by Muessig *et al.*, so only 14 cores postdate the Muessig *et al.* publication. Secondly, at least 13 of the remaining 14 cores are from Silver Lake (Wells *et al.*, 1989, p. 75) and are therefore irrelevant to the present discussion. Thirdly, of the holes discussed by Wells *et al.* (1989, p. 112) drilled in Soda Lake using rotary techniques, none appear to have exceeded 50 m, and none of the cores from Silver Lake appears to have exceeded ~30 m in depth or to have penetrated into pre-OS2 sediments. Thus, these cores contain no evidence for or against the presence of an OS6 lake in Soda Lake basin.

On the other hand, well logs published by Burnham (1955) provide support for a significant lake in the Soda Lake basin at roughly OS6 time. A well (well number 11/8-7X3) near Crucero, ~5 km northeast of the Mesquite Spring shoreline locality, penetrated "bluish clay" between 46 and 87 m. In a second well (11/8-10-1), ~4 km east of Crucero, "clay" was encountered between 44 and 84 m. A third well, at Baker, went through "clay (lake bed)" between 45 and 99 m. While these descriptions lack detail, the bluish color, the parenthetical addition "lake bed," and the thicknesses of the units are suggestive of perennial lacustrine conditions. These clay beds are all well below the OS2 lake sediments described by Wells *et al.* (1989). In the Death Valley core, Lowenstein *et al.* (1999) encountered OS6 perennial lake sediments below 128 m. If the sedimentation rate at these sites in Soda Lake is ~one third that in Death Valley, as seems

eminently possible, these clay beds would be, at least approximately, of OS6 age. It is not clear, however, how this stratigraphy is related to that in a different part of the basin described by Muessig *et al.* (1957) and discussed in my paper (p. 332).

Enzel *et al.* conclude that "only a late Pleistocene lake occupied the Silver/Soda basin." With respect to Soda Lake and taken literally, this seems naïve, considering that there are more than 670 m of sediment in the basin (Brown and Rosen, 1995, p. 288).

Mesquite Spring. Bassett and Muessig's (1957) abstract begins with the words "Topographic benches, green sediments, and sand ridges . . . are interpreted as features of a Pleistocene lake." Although they did query "wave-cut" and "green lacustrine" later in the abstract, as Enzel *et al.* note, their conclusion does not seem "reluctant." With reference to Thompson's (1929) discussion of Mesquite Spring, I could not find a description in this book of the sediments I described.

As noted, the possibility that many fine-grained deposits are related to springs rather than lakes (Quade *et al.*, 1995) complicates the situation. However, I am not aware of any instances in which horizontal benches or gravel clinofolds dipping up to 28° lakeward have been genetically associated with such spring deposits. Even if the silts at the Salt Spring Hills and at Mesquite Spring are spring deposits, the benches and, in the latter locality, the gravel foresets remain as evidence for a deep lake.

Enzel *et al.* state that the benches at Mesquite Spring are "not within a continuous line of sight of each other" so it is "difficult . . . to follow Hooke's arguments in the field by leveling or to eliminate alternative interpretations." Using a level to establish relative elevations of features that are out of sight of one another is pretty elementary.

For the benefit of Enzel *et al.* and of others who have not had the opportunity to visit the Mesquite Spring locality, I am including a stereo pair of aerial photographs of it (Fig. 1).

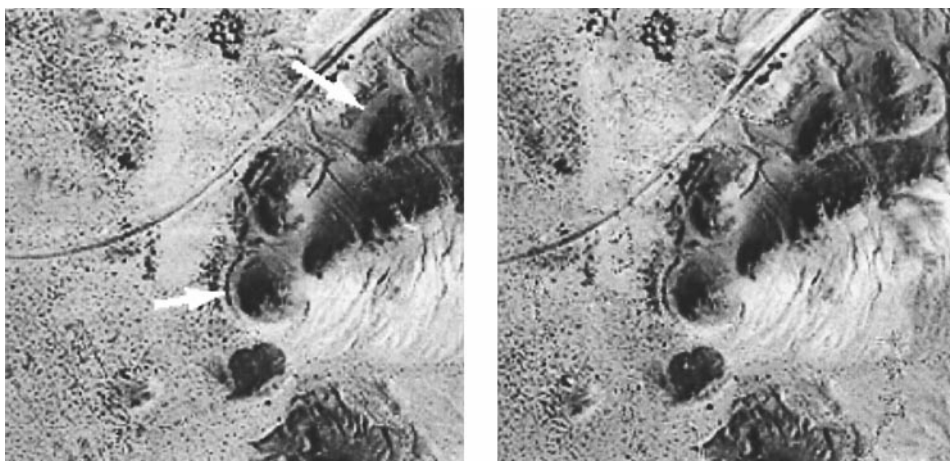


FIG. 1. Stereo aerial photographs of the Mesquite Spring shoreline locality. Arrows point to shorelines. Feature that appears to be a road is the roadbed of the abandoned Tonapah and Tidewater Railroad. North is to top. (USDA photographs AXL-7K-10,11 dated 11-10-52).

Tectonic warping. There are two errors in Enzel *et al.*'s discussion. (1) My model calls for 250 m of relative vertical displacement between Mesquite Spring and Shoreline Butte, not 290 m as stated by Enzel *et al.* (2) The Black Mountains are not composed of Quaternary rocks.

Referring to the shorelines on the Black Mountains and on Shoreline Butte, Enzel *et al.* appear to be confused by the notion of relative motion. In relative motion, Shoreline Butte, the Black Mountains, and the floor of Death Valley can be sinking relative to Mesquite Spring, and the floor of the valley can be sinking relative to Shoreline Butte and the Black Mountains.

Enzel *et al.* mention the lack of fault scarps and the dominance of strike slip motion as evidence against warping, citing Bull and McFadden (1977) in support of this argument. This suggests that they misunderstand the term *warping*. A dictionary definition of warping is "to twist or bend out of a flat plane." There is no connotation of rupture (faulting). Indeed, Bull and McFadden (1977, p. 117) stated explicitly that their analysis cannot detect broad warping.

Where warping of the crust takes place, it is probably driven largely by vertical flow in the asthenosphere. Differential vertical movement, driven by such asthenospheric flow and at rates comparable to those I've hypothesized for the Mojave (Hooke, 1999, Fig. 4c), is occurring today, without faulting, in Sweden (Milne *et al.*, 2001) and along the east coast of the United States (Davis and Mitrovica, 1996). In the Mojave, horizontal movements in the asthenosphere and lithospheric mantle are probably responsible for stretching the crust, leading to the listric faulting responsible for the Basin and Range topography. It would be strange, indeed, if these deep horizontal movements were not accompanied by some vertical movement.

With regard to the leveling data cited by Enzel *et al.*, let us recognize that it is probably not possible to identify the water level on an ancient beach to within ± 1 m. Thus, their data suggest that there has been less than this amount of deformation in the last $\sim 10,000$ yrs. Over 40 km, my model predicts an *average* of ~ 4 m of deformation in this time period. While it is certainly possible that my model is wrong, it is also possible that the deformation has been nonuniform in space, time, or both. (My model, incidentally, includes nonuniformity in space; see Hooke, 1999, Fig. 4c.) Thus, this lack of observable deformation in the last 10,000 yrs does not invalidate my hypothesis.

How is the lake surface stabilized? My argument here is, once again, both misrepresented and misunderstood. I did not say that "sills must have been present to prevent expansion of the lake." On the contrary, I argued that lakes may be stabilized by topography which *allows* significant expansion with only a small increase in level. This topography may be in the form of a sill separating the lake from a (shallow) basin without a lake or with a lake that is lower than the sill level (Hooke, 1999, pp. 328, 334), or it may involve gentle topographic slopes along the shoreline (Hooke, 1999, p. 335).

Weather patterns vary from year to year, resulting in changes in levels of lakes that have no outlet or other stabilizing topo-

graphic control. Where such fluctuations are muted for a period of years, a shoreline may develop. In such situations, one would expect the best developed shorelines to form at intermediate levels reflecting the average climate, not at the highest level representing an extreme climate. However, in Death Valley the best developed Blackwelder stand strandline, by far, is the highest one at 90 m. It seems unlikely, at least to me, that a bench as prominent as the 90-m shoreline on Shoreline Butte and Mormon Point could be cut by a lake that was not controlled topographically, particularly when that bench is the highest in a series consisting of much less well-developed strandlines.

Closing statement. I believe that (1) the leveling data from the OS2 Silver and Soda Lake shorelines and (2) the lack of age and environmental control for my model are the only points of merit in Enzel *et al.*'s comment. The former concern is far from fatal, and I fully acknowledged the latter.

Stephen Wells and his students have done some outstanding stratigraphic work on OS2 lakes in the Mojave. Perhaps in the future they will turn their attention to older lakes, for which the exposed stratigraphic evidence is far less abundant. If they do so with an open mind, we are sure to be rewarded with fascinating insights into this tantalizingly enigmatic period of pluvial history.

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