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Climate of the Past Discussions

Interactive comment on "Sea level 400 000 years ago (MIS 11): analogue for present and future sea-level" *by* D. Q. Bowen

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COMMENTS ON: Bowen, D.Q., "Sea level 400 000 years ago (MIS 11): analogue for present and future sea-level"

COMMENTS BY: Dan Muhs, USGS

This is an intriguing paper and deals with an important topic that has received much attention lately. The topic is timely and certainly worthy of a good review; Dave Bowen knows the subject matter well and is the right person to do a paper like this.

Having said that, the present manuscript needs some revision (from a scientific viewpoint) and editing (from a compositional viewpoint). From the scientific side, the approach offered is reasonable assuming that certain conditions for the study areas are

met, as follows:

(1) the MIS 5.5 terrace or reef is dated (2) the MIS 5.5 terrace or reef's elevation is known (3) the MIS 5.5 terrace or reef's paleosol level is known (4) the MIS 11 terrace or reef's age is known (i.e., the terrace is independently dated) (5) the MIS 11 terrace or reef's elevation is known

Dave has covered the bases well on a key item (3) where there IS controversy about the precise paleo-sea level of MIS 5.5, i.e., +2 m, +4 m, or +6 m, as well as item (1), where there is controversy over its precise timing. He uses three possible ages, and three possible paleo-sea levels, thereby providing a range of uplift rate values that present the reader with all reasonable options. Using these uplift rate values, combined with items (4) and (5), it is then possible to "back out" the possible paleo-sea levels for MIS 11. These also of course show a range of values and that is the strength of the paper–an illustration of the probable ranges of high sea level during this time.

Having said that, there is a fundamental problem with many of the study areas chosen, as outlined specifically below. In many of the study areas, the "dating" of the MIS 11 terraces, by previous workers, was done by extrapolating the late Quaternary uplift rate back in time, assuming a paleo-sea level near present, and then identifying the MIS 11 terrace based on its present elevation. Where this has been done, one cannot then go back, use the terrace "age" and elevation, and then with the late Quaternary uplift rate, "back out" the paleo-sea level! That is circular reasoning. This applies to a number of the study areas chosen and those, sorry to say, have to be eliminated from the analysis. However, there are a number of places (Barbados, Sumba Island, Alaska, U.S. Atlantic Coastal Plain), where the MIS 11 terrace has been at least approximately dated independently and the other requirements (1, 2, 3, 5) above have been met. Those are just fine for the purposes of this paper and will still yield some significant findings: the paper can stand on the results of these alone, and the other study areas simply must be deleted.

From the compositional viewpoint, the paper is loaded with typographical errors, grammatical errors (misspelled words, incomplete sentences, tense errors, etc., etc.) and style problems. I started marking it up and then gave up after about three pages. I sense that the final manuscript was knocked out in a hurry without much proofreading, but all this needs to be cleaned up.

SPECIFIC COMMENTS:

—p. 1855: note that the "keep-up-catch-up-give-up" reef concept is attributed here to Muhs et al. (2002), but that is not correct: this was an idea of Neumann and McIntyre (1985), and was simply cited by Muhs et al. (2002).

—p. 1856: change Muhs and Sabot (1994) to Muhs and Szabo (1994)

—p. 1856 (bottom) on over to p. 1857 (top): for tectonically stable areas, several important studies in tectonically stable areas indicated +5 m to +6 m sea level estimates for MIS 5.5, including San Salvador, Bahamas (Chen et al., 1991, GSA Bulletin); northern Bahamas (Neumann and Moore, 1975, QR); Yucatan Peninsula (Szabo et al., 1978, Geology); Bermuda (Harmon et al., 1983, PPP); Isla Guadalupe, Mexico (Muhs et al., 2002, GSA Bulletin). In the review by Murray-Wallace and Belperio (1991) cited here, some of these papers had not yet appeared and the others were ignored.

—p. 1859, near the bottom: note that an age of 414 ka is way beyond what most careful luminescence geochronologists are willing to accept as an age limit. Most of the best in this field, including Ann Wintle, a pioneer in these techniques, feel that ∼100 ka (plus or minus) is about as far as one can go. Incidentally, the last sentence of this page is a good example of the typographical errors pervasive in this paper.

—p. 1860, section 4, Oahu: there is a big problem here. See discussion in Muhs et al. (2004), The Quaternary Period in the United States (ed. Gillespie et al.). [attached here]. Corals from the Kaena high stand deposit have been dated by U-series by three different labs (Veeh, as cited), Szabo et al. (1994, Science), and again by Larry

Edwards (collected by and cited in Hearty, 2002) as around 550-600 ka. Hearty chose to ignore all three of these concordant ages, one of which was collected by him, and instead came up with an amino acid age estimate, in turn calibrated to the MIS 5.5 reef, itself dated by U-series! Thus, Hearty is willing to accept U-series ages when he wants to use them for calibration, but when he wants a deposit to be younger than U-series would indicate, he rejects the ages. Thus, I doubt that the Kaena high stand dates to 400 ka and in my view cannot be used in this analysis. Sorry, Dave!

—p. 1861, section 4.1, Nome coastal plain: I wouldn't cite the U-trend age here in support of a MIS 11 age. That technique was never really completed tested for reliability. However, there are other lines of evidence that support a MIS 11 age for the higher deposits discussed here–again, see discussion of the Alaskan sea level record in Muhs et al. (2004).

—p. 1861, section 4.2, Rome: how was the MIS 5.5 terrace dated?

—p. 1861 (bottom) and continuing to p. 1862 (top): circular reasoning here! Kern and Rockwell (1992) estimated the 413 ka age based on the uplift rate of the 120 ka terrace AND AN ASSUMPTION OF A SEA LEVEL THE SAME AS PRESENT FOR ALL HIGHER TERRACES. You can't just reverse this by assuming the age is correct, using its elevation and then backing out the paleo-sea level. That is circular.

—p. 1862-1863, section 4.5, New Zealand: same comment as above. Suggate ASSUMED a sea level of present for a 400 ka age estimate using uplift. The terrace is NOT independently dated; in fact, I don't see any data indicating that the MIS 5.5 terrace is dated, either.

—p. 1863, section 4.7, Atauro Island: exactly the same problem again. The higher terraces are dated using an assumption of similar sea level and uplift rate from MIS 5.5. Here, at least the MIS 5.5 terrace is dated (as at Oceanside/San Diego). However, the higher terrace is not.

—p. 1863-1864, section 4.8, Peru: same problem again, but even worse: NONE of terraces are dated, simply counted back. Hall et al. (2008) dated pediments here, not marine terraces, so that is no help (although it has potential for the future).

—p. 1865, section 5.2, Bahamas: in the first sentence here, things are exactly reversed of what they should be: carbonate eolianites are blown onshore during times of HIGH sea level (although thought to be at the beginning, early transgressive stage: see Mylroie, 2008) and paleosols form on the eolianites during COOLER periods, i.e., glacial periods. Easily fixed, but quite fundamental.

Please also note the Supplement to this comment.

Interactive comment on Clim. Past Discuss., 5, 1853, 2009.