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Climate of the Past Discussions

This discussion paper is/has been under review for the journal Climate of the Past (CP).
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Comment on "Sea level ~400 000 years ago (MIS 11): analogue for present and future sea-level?" by D. Q. Bowen (2010) Can the extrapolation of uplift rates from MIS 5e shorelines to MIS 11 replace direct and tangible evidence of the latter's sea-level history?

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"The only reliable indicators of former sea-level (water levels) are geological actualities on the global shore and its immediate hinterland" (D. Q. Bowen, 2010, p. 19).

1 Overview

It is not unexpected that the assertion of a eustatic rise of sea level of up to +21 m would stir debate in the scientific community, and generate apprehension among governments and the general public, because the potential for a similar rise in the future is a significant cause for concern. To account for a rise of +21 m higher than present, all of the Greenland (GIS) and West Antarctic ice sheets (WAIS) would have to melt or collapse (perhaps with speed), with an additional 6 to 8 m sea level equivalent also being contributed from drawdown of the East Antarctic Ice Sheet (EAIS). This obviously has enormous implications for the present and future greenhouse world, and its possibility should not be blithely dismissed or ignored.

2 Previous work on MIS 11 rocks

The geological and paleontological documentation of MIS 11 sea levels is best derived from rocks from stable carbonate platforms, such as those in Bermuda and the Bahamas, as has been presented in a number of papers (Hearty et al., 1999; Kindler and Hearty, 2000; Hearty, 2002a; Hearty and Olson, 2008, Olson and Hearty, 2009; van Hengstrum et al., 2009; and references therein), which contain abundant geomorphological, stratigraphical, sedimentological, petrological, tectonic, macropaleontological, micropaleontological, and geochronological data. These deposits are correlated with MIS 11 at circa 400 ka. The stratigraphy reveals multiple MIS 11 stillstands at approximately +2–3 m, +7–8 m, and a final peak at +20 m, with the intermediate highstand (+7–8 m) the most prolonged event as indicated by broader erosional and depositional

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terraces. With a slight adjustment for uplift, the rock record from Oahu, Hawaii (Hearty, 2002a) reveals the same tri-fold succession of sub-events, as illustrated in Hearty and Olson (2008). It has never been postulated in our papers that the *only* sea level during MIS 11 was at +20 m.

Numerous other authors have described and dated similar rock sequences around the world (cited in the above) including Bowen (1999), who later recanted (Bowen, 2003a; b) due to an inability to "provide a definitive elevation for the MIS 5.5 sea-level" (Bowen, 2010; p. 25). Yet, he still suggested MIS 11 sea level may have been up to 13±2 m higher than present (Bowen, 2003b), the sea-level equivalent of all of GIS and WAIS.

3 A red herring?

Bowen (2010) attempts to counter the diverse body of "geological actualities" solely by estimating the likely position of MIS 11 based on the position of MIS 5e sea level. In this maze of numbers, Bowen (2010) *assumes* sea levels and ages for MIS 5e (disregarding key papers that contain global documentation of such facts), from which he then calculates and *assumes* constant uplift rates (that cannot be verified independently). Based on this fragile base (which disregards factual geological data on the subject and excludes the most fundamental eustatic variable of *how much polar ice melted during MIS 11*), he then generates numbers from several tectonically deformed localities upon which he estimates and averages the height of sea level during MIS 11. However, without rationale or precedent for his numerical manipulation, too low or too high outliers in predicted elevation of MIS 11 sea level are discarded if they fall more than 2σ from some concocted average. If one is looking for extremely high sea levels, how can one eliminate their possible existence with an arbitrary statistical cleansing technique?

4 Face the facts

Bowen (2010) ignores all relevant published geological data and facts regarding MIS 11; instead he engages in pure speculation by injection of unproven and implausible scenarios, so that he can assert that the +20 m sea level simply did not happen. He invokes a morass of sketchy data from numerous tectonic sites of dubious relevance, in order to construe an argument suggesting where stage 11 "ought to be" instead of confronting the geological documentation of where MIS 11 sea level actually was.

If the author and his colleagues possess tangible data that contradict the abundant physical and analytical evidence Hearty and others have presented over the past decade in several high-impact peer-reviewed publications, they should publish such facts in similar venues. Extensive physical and biological evidence from a variety of workers has been generated from numerous sites documenting MIS 11 marine deposits. The same diagnostic field and laboratory criteria have been used to define MIS 11 sea level as would be used for MIS 5e or the Holocene. Bowen obscures the primary MIS 11 highstand issues with a fog of numbers. It is simply insufficient to say (once again) a mega-tsunami did it, or wave splash did it, it is uplifted or paleoceanographers can not find it, so let's make it go away. We have previously addressed in publication nearly all of the issues (substantial or not) raised by Bowen and other detractors. Nonetheless, a point-by-point synopsis of Hearty's views of various aspects of Bowen's (2010) paper is provided in Table 1.

5 Conclusion

Bowen (2010)'s paper is rife with inaccuracies, misinformation, and outright errors, which cumulatively render it poor science. The neglect extends in an unacceptable manner where far too many relevant scientific works are misquoted or omitted (Point 26 in Table 1). The premise, methods, and numbers generated from his chain of assumptions are scientifically unsound, thus any conclusions drawn from them are equally

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unsound. At no point does Bowen (2010) confront the broad and diverse volume of "geological actualities" on the rocks and ages of MIS 11, which document the stepping up of SL from +2, to +8, to +21 m due to melting of polar ice.

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Table 1. Issues raised by Prof. Bowen and Hearty's reply to them. "We" refers to all the authors that have addressed or dealt with these issues previously in publication, but the responsibility for these statements below are Hearty's alone. Marine isotope stage (MIS) 5e = 5.5 and MIS 5a = 5.1; SL = sea level.

Point	Location	Issue; Bowen (2010) comment	Alternate/addressed view/publication/reply
1	P. 19, para 1	The value of isotopes for estimating eustatic SL	Olson and Hearty (2009; p. 281): "By definition, proxy methods cannot provide a concrete measure of sea-level position or global ice volume."
2	P. 19, para 2	"geological actualities" are the most reliable indicator of SL	Yet Bowen (2010) avoids any direct discussion of our body of "geological actualities" presented for MIS 11 SL.
3	P. 20, para 1	Two schools re MIS 11 SL.	In which school are we? MIS 11 SLs of +2, +7.5, +18-21 m were described in Hearty et al. (1999); Hearty and Olson (2008); Olson and Hearty (2009). SL was stable for the longest time at +7 to +8 m and peaked briefly at +20 m at the end of MIS 11.
4	P. 20, para 3	"No. . . agreement" on MIS 5e SL	But there are studies that narrow down the odds including a global assessment of MIS 5e SL in Hearty et al. (2007) (not in references). Note a 2 m miscalculation of MIS 5e SL can result in a >6 m SL error for MIS 11 – the equivalent of the WAIS.
5	P. 20, para 3	High MIS 5e SL "has received little support (Carew, 1997; Mylroie, 1997)"	Hollin 1965 citation is not in the Bowen (2010) references; Carew and Mylroie's (1997) "little support" papers, do not appear in Bowen (2010) references, nor have I added them here.
6	P. 20, para 4	"de facto" +6 m 5e SL because "fossil corals throughout the Pacific and Indian Oceans are found consistently between 2 m and 9 m."	Origin of "de facto +6 m SL" in Hearty et al. (2007, p. 2106) attributed to Bahamas +5.9 m notch (Neumann and Moore, 1975). The range of 7 m between +2 and +9 m does not seem to define a sea level at +6 m.
7	P. 20, para 4	"+2 m 5e SL"	Global survey of MIS 5e SL in Hearty et al. (2007); Yes +2.5 m@127 ka, and +6 to +9 m@120 ka. Evidence abounds! Why not cite it and use it?
8	P. 21, para 1	"MIS 5.1 deposits above SL" discussion.	An original geological work in Bermuda by Vacher and Hearty (1989) shows MIS 5a marine deposits above SL, and U/Th dates in Ludwig et al. (1996) support these original findings; neither appears in Bowen (2010) references.

[1] In a recent article, McMurtry et al. (2010) state: "there is currently no evidence for a MIS 11 highstand on Oahu". Although much more needs to be said elsewhere about this paper, it should be recognized that all ages older than the last interglacial, as reported in their *Geology* paper and supplement, are considered to be unreliable based on published screening protocols (Fairbanks et al., 2006) due to Th concentrations well in excess (4–30 ppb!) of the maximum acceptable <2 ppb limit and excessive recrystallization (up to 15%). Further, these authors exclude the existence of MIS 11 in Oahu without reporting a single new date or geological fact from the Kaena deposits Hearty (2002) correlated with MIS 11.

Table 1. Continued.

Point	Location	Issue; Bowen (2010) comment	Alternate/addressed view/publication/reply
9	P. 22–23, para 2	South Carolina; age of Canepatch?	Relevant discussion of the geology and age of the Canepatch Fm in Hollin and Hearty (1990); not cited.
10	P. 23, para 2	Coorong: East Avenue (EA) deposits yield 0.332 D/L Leu far out of sequence?	Murray-Wallace et al. (2001) show inconsistent AAR results at EA. They query if EA is even MIS 11. Likewise, Hearty (unpublished) found the same D/L anomaly along with an advanced state of carbonate diagenesis at EA, indicating deposits are probably much older than MIS 11.
11	P. 23, para 4	Curaçao: Notch at +10.5 m, and base of MIS 11 terrace at +21 m.	Lundberg and McFarlane's (2002) geology, dating, and interpretation of the MIS 5e and 11 deposits are perfectly reasonable and yield a SL history from rocks and U/Th ages similar to that we have described.
12	P. 23, para 6	Lazio, Italy	The stratigraphy, height, and age of middle Pleistocene SLs in Lazio are discussed in Hearty and Dai Pra (1986). In 1986, it was not clear if these were MIS 9 or 11.
13	P. 23–24, para 6	Barbados: "that of MIS 11 at 120 m".	The rest of the story: There are probably 3 (not one) MIS 11 terraces (T11?, 12, and 13 at 100, 110 and 120 m – *note range of 20 m*) identified by Schellmann and Radtke (2004, 86 and 100) in Barbados. Three highstands are described in Hearty et al. (1999); further discussion of MIS 5e and 11 in Barbados in Hearty et al. (2007) and Olson and Hearty (2009, p. 282); neither cited in this case by Bowen (2010).
14	P. 24, para 1	Sumba: Range of ESR ages of terrace III2 at 190 m.	Is an ESR age range of 274 to 456 ka "likely to mark the MIS 11 SL"? At given rates (0.2–0.5 m/ka) this age range would encompass from 36 to 90 m of uplift effectively negating its relevance.
15	P. 24, para 2	"MIS 11 SL at 1.5 m±3"	This number is the product of a statistical manipulation and has no factual basis! Why does not Bowen (2010) confront our geological evidence for stepping up of SL from +2, +7.5 m, and +20 m (op. cit.). Plus, there are serious doubts about the ages and interpretations of MIS 11 SL from both Barbados and Coorong, not to mention unresolved tectonics at both locations.

[1] In a recent article, McMurtry et al. (2010) state: "there is currently no evidence for a MIS 11 highstand on Oahu". Although much more needs to be said elsewhere about this paper, it should be recognized that all ages older than the last interglacial, as reported in their *Geology* paper and supplement, are considered to be unreliable based on published screening protocols (Fairbanks et al., 2006) due to Th concentrations well in excess (4–30 ppb!) of the maximum acceptable <2 ppb limit and excessive recrystallization (up to 15%). Further, these authors exclude the existence of MIS 11 in Oahu without reporting a single new date or geological fact from the Kaena deposits Hearty (2002) correlated with MIS 11.

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Table 1. Continued.

Point	Location	Issue; Bowen (2010) comment	Alternate/addressed view/publication/reply
16	P. 24, para 4	"...a wide range of TIMS ages (127–680 ka)" on "flowstone" suggesting a scatter of random ages.	This is a blatant misrepresentation of facts! Ages in question occur *in stratigraphic order* both above and below the +20 m marine deposits (Hearty et al., 1999, Fig. 2, p. 376), and at various levels of flowstones up to 35-cm-thick capping marine deposits (Hearty and Olson, 2008, Fig. 1A). Some ages listed by Bowen (2010) do not at all relate to MIS 11 deposits but constrain the age of the "big red soil" (Fig. 5 in Olson and Hearty (2009, p. 276)). Another important point missed by Bowen (2010) is that flowstone ages in mm scale contact with MIS 11 beach at +18 m yield a weighted mean age of 399±11 ka (Hearty and Olson, 2008, p. 311). We are not responsible for the quality or accuracy of dates from McMurtry et al. (2007); (2008, not cited).
17	P. 24, para 5	"tsunami" McMurtry et al. (2007). McMurtry et al. (2008) not cited by Bowen (2010).	Previously and meticulously addressed in comment-reply (op. cit.), specifically Hearty and Olson (2008) and also Olson and Hearty (2009). Not cited in this case by Bowen (2010).[1]
18	P. 24, para 5	Bermuda's +6 m MIS 5e; Ft. St. Catherine MIS 5a. MIS 11 at 5±2 m.	Inaccurate and selective referencing. Hearty's (2002b) revision of MIS 5 stratigraphy clarifies this issue. Vacher and Hearty (1989) address Ft. St. Cats issue. Neither cited (at all) by Bowen (2010). MIS 11 at 5±2 m in Bermuda?? See Hearty and Olson (2008).
19	P. 24, para 5	RE: Belmont Formation (as MIS 7 deposit) formed above or below SL.	Uncited: revision of stratigraphy and nomenclature in Bermuda Hearty (2002b). Even if Bowen (2010) does not agree, it is irresponsible to not cite this paper that is very relevant to this discussion.
20	P. 24, para 5	What is "sacrosanct about tectonic stability of Bermuda?"	If Bermuda is rising tectonically, especially at rates implied by Bowen (2010) (MIS 7 from −15 to +2 m in 200 ka!), the laws of gravity, rheology, and plate tectonics need to be rewritten. See Olson and Hearty (2009, p. 280) for discussion of Bermuda tectonics; not cited in this case by Bowen (2010).
21	P. 24, para 6	"...hurricane winds"	Did hurricane winds deposit the in situ mud burrows of the intertidal thalassinidian shrimp? See Olson and Hearty (2009, p. 276–277); not cited in this case by Bowen (2010).

[1] In a recent article, McMurtry et al. (2010) state: "there is currently no evidence for a MIS 11 highstand on Oahu". Although much more needs to be said elsewhere about this paper, it should be recognized that all ages older than the last interglacial, as reported in their *Geology* paper and supplement, are considered to be unreliable based on published screening protocols (Fairbanks et al., 2006) due to Th concentrations well in excess (4–30 ppb!) of the maximum acceptable <2 ppb limit and excessive recrystallization (up to 15%). Further, these authors exclude the existence of MIS 11 in Oahu without reporting a single new date or geological fact from the Kaena deposits Hearty (2002) correlated with MIS 11.

Table 1. Continued.

Point	Location	Issue; Bowen (2010) comment	Alternate/addressed view/publication/reply
22	P. 24, para 6	"...large boulders...at 20 m" and Mylroie (2008) re: Eleuthera – "highest marine deposits on the island...of 131 to 119 ka (Chen et al., 1991). "	Not sure of its relevance, but the citation is incorrect. It's 1997 not 1998b. Plus, Mylroie (2008) thinks these are "karst towers". The Chen et al. (1991) dates are from corals at a maximum of +2.5 m (not +6 m!) on San Salvador Island and have little relevance to Eleuthera, where we have described multiple stacked limestone-soil couplets representing several middle Pleistocene interglacials including MIS 11 (Hearty and Kaufman, 2000, not cited in text) at +2, +8, and +20 m.
23	P. 25, para 1	"Kaena marine deposits on Oahu, Hawaii"	Arguments fully explained in Hearty (2002a) and Hearty and Olson (2008) (not cited in this case by Bowen, 2010). All the U/Th dates are dodgy as I said in Hearty (2002a, p. 74), but the Kaena morphostratigraphy and its relationship to MIS 5e are quite clear. It is curious that Bowen (2010) does not employ this excellent Oahu sequence in his numerical manipulation.[1]
24	P. 25, para 2	Bowen (2010, 1999) +23 m MIS 11 SL; then Bowen (2010, 2003b) +13 m; then Bowen (2010, 2003a) no SL: *voilà, n'existe plus!*	Are we to understand that with an abstract, Bowen (2010) will make his own MIS 11 SL estimates go away due to a mix of amino acid ages in 5e deposits? Reworking is common in unconsolidated siliciclastic deposits. His arguments are paper-thin even in recanting his own +20 m MIS 11 SL.
25	P. 25, para 5	"Hearty et al. (1999) suggested Antarctica ... caused the MIS 11 SL"	Incorrect and misleading. Hearty et al. (1999, p. 378) stated: "In order to account for a 20 m increase in ocean volume, if all of GIS and WAIS (~12 m) melted, an additional 8 m of water equivalent of EAIS must have also melted."
26	P. 26–29	Sloppy, inaccurate references that would not pass editorial muster or the peer-review process in most journals.	No fewer than 17 references are incorrect, are in text but do not appear in references, or are in references but do not appear in text. These errors do not include blatant omissions of relevant works by Bowen (2010) listed in this table.

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