

Interactive comment on “Modeling Mediterranean ocean climate of the Last Glacial Maximum” by U. Mikolajewicz

P.Th. Meijer (Referee)

meijer@geo.uu.nl

Received and published: 15 November 2010

Central to this paper are several experiments carried out with a regional ocean circulation model as applied to the Mediterranean Sea during the Last Glacial Maximum (LGM). The atmospheric and open-ocean conditions required to set up this ocean-only model are derived from a combination of two global-scale models: an atmospheric general circulation model and a coarse-resolution earth system model. Bathymetry, including the important depth of straits, is adjusted to LGM conditions.

The paper is of transparent structure, reads well mostly, and arrives at important insight not in the last place through its detailed comparison of model results with available observations (proxy data). Most of my comments and suggestions, listed below, relate to the manner of presentation.

Interactive
Comment

My one major concern is this. On page 2008, line 13 the author states that a first goal is to investigate the mechanisms responsible for the difference in the amount by which the western and eastern sub-basin are observed to have been cooler at the LGM than at present. Even without reading further one expects that this cooling is at least partly governed by changes in the temperature of the atmosphere over the sea, that is, by something that will be used as “input” to the regional ocean model. Indeed, the section that appears to present the answer to the first research question (starting line 13 on page 2022), discusses responsible changes in atmospheric circulation. Moreover, as explained in section 5.4 (page 2031), the cooling is already essentially there in the global-scale earth system model (see also conclusions, line 4). My point is that, in order to truly understand the answer to what is presented as the first objective of the paper, one would like to see more (literally) of the underlying atmospheric run. I appreciate that this is not the way things were meant but one gets the feeling that while the paper focusses on the results obtained with the ocean-only model, the real insight resides in the “input fields” obtained with other models.

Other comments:

p. 2007, l. 17: see also Bozec et al., The Mediterranean thermohaline circulation during the Last Glacial Maximum inferred from a 1/8 resolution oceanic model, Geophysical Research Abstracts, Vol. 8, 02580, 2006.

p. 2008, l. 14.: “the strong east-west gradient” but the compilation of observations on the preceding page also discussed evidence to the contrary?

p. 2008, l. 26: I don’t think it is mentioned, in what follows, where the sponge conditions needed for the LGM-experiments are derived from (the earth system model, I presume?).

p. 2010, l. 1: please help the reader in seeing the difference between “ocean grid points” and “grid of the ocean model” (“ocean grid points of the atmosphere model”?).

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

p. 2010, l. 26: because light water is easily replaced by heavier water produced at the surface? Please clarify.

p. 2013, l. 23: the criterion consists of the density difference with the surface being less than the quoted value, I presume. Please spell out.

p. 2015, first paragraph: this brings up the question: what is strait width in the model?

p. 2017, l. 16-19: what is the point of this paragraph?

p. 2017, last paragraph: these results are very interesting but it is not always clear to me whether the spatial distribution or basin-integrated values are being referred to.

p. 2025, l. 21: perhaps better to start a new section here; there follows a long piece on strait flow.

p. 2028, l. 3: on what is this statement about the long-term mean exchange based?

p. 2028, second paragraph: This seems disconnected from the preceding and subsequent text and is better moved up.

Figures 5 and 12: vectors are very hard to read. Perhaps showing only the vectors or splitting the information over two panels is better.

Minor points:

p. 2007, l. 12: comma after “would” and “control”.

p. 2009, l. 12: I would suggest “is prevented by” or “is corrected for”, rather than “is accounted for”.

p. 2011, l. 8: the abbreviation “CTL” has not been introduced at this stage (it is explained on lines 15 and 24).

p. 2011, l. 13/14: use “he” and “his” rather than “they” and “their” (as is done elsewhere; but see also p. 2022, l. 10).

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

- p. 2012, l. 5: comma after “For consistency”.
- p. 2013, l. 14: “overestimates the low-salinity signal” is correct but hard to digest. Isn’t “produces too fresh water” more direct?
- p. 2014, l. 2: this sentence is not finished.
- p. 2015, l. 7: I always read “Gulf of Lions” in the English literature.
- p. 2017, l. 24: here and elsewhere, “cf.” rather than “cw.”?
- p. 2019, l. 9: type error in foraminifera.
- p. 2019, l. 13: the word “Both” seems too much.
- p. 2010, l. 4: “At this depth” = at 37 m, right?
- p. 2023, section heading: contents would justify calling this “Salinity and circulation”.
- p. 2023, l. 15: Sentence does not read well.
- p. 2023, l. 25: Sentence not complete.
- p. 2025, l. 24: “adjacent” rather than “ambient”?
- p. 2028, last line: a word is missing.
- p. 2029, l. 18: two type errors.
- p. 2029, l. 17: no capital for “Whereas”.
- p. 2030, l. 25: sill depth is lower but the sill is higher.
- p. 2031, l. 7: bracket missing.
- p. 2032, l. 16: “reduce” rather than “reduces”.
- caption to Fig. 6.: the leading “Left:” seems out of place.
- caption to Fig. 11: type error in “logarithmic”.

Interactive comment on Clim. Past Discuss., 6, 2005, 2010.

CPD

6, C982–C986, 2010

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C986

