Clim. Past Discuss., 8, C2480–C2487, 2012
www.clim-past-discuss.net/8/C2480/2012/

Climate of the Past Discussions

Interactive comment on "Variations of oceanic oxygen isotopes at the present day and the LGM: equilibrium simulations with an oceanic general circulation model" *by* X. Xu et al.

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Received and published: 22 November 2012

General assessment

Formal review of manuscript "Variations of oceanic oxygen isotopes at the present day and the LGM: equilibrium simulations with an oceanic general circulation model" by X. Xu et al., submitted to Climate of the Past Discussions.
The manuscript I have reviewed is constituted of the initial submission of the cpd-8-4885-2012 manuscript, of an interactive comment provided by the authors online following discussion on the use of the MARGO database and a supplementary file

that was submitted as part of the interactive. Since this is an unusual situation and since the authors never provided a new version of the complete text (which would have helped greatly the review process), I have done my best to assess the text, part from the interactive discussion, part from the main document. In particular, I have **NOT** evaluated the figures 10 to 14 of the original manuscript, but the new figures provided in the author comment.
My assessment of the manuscript is that it is **not acceptable for publication in the present form** and whether a further version may be acceptable should be **subject to re-assessment**. My assessment is based on the main comments in the following section, the ones pertaining to the description and analysis of the simulation and to the database being the most problematic. I give some additional comments to improve the manuscript in the section following.

Major comments

1./ **Description and analysis of the simulation**. The model used by the authors is a oceanic general circulation model only. The drawback of this approach when dealing with past climates is clearly the method to derive the isotopic boundary conditions for their simulation. Xu and coworkers present their setup on p. 4889 on the initial manuscript, their section 2.3. The fluxes at the atmosphere – ocean interface are derived from a number of simulations : two simulations from the ECHAM-wiso model under PD and LGM conditions the latter using (since it is an atmosphere only model) the surface conditions from an LGM coupled run without isotopes. That is at least what I undertood from the description section, that is not easy to read as it is. A clear consequence of this setup is that the LGM ocean obtained in the present study is not fully coherent with the LGM COSMOS coupled simulation that provided the boundary conditions.

Though this is a fairly logical procedure, the authors should assess in details the differences in freshwater fluxes obtained as well as the differences in surface condition and deep ocean conditions obtained in the present manuscript with respect to the COSMOS simulation. Whether the results are very similar or not is an indication of the validity of the procedure used. Together with this analysis, the authors should as well provide a comprehensive discussion of the dynamic changes of their LGM ocean in comparison to the present-day, especially in the regions where they find most differences in terms of $\delta^{18}O_{sw}$. Two examples on this. The question of whether the LGM AMOC they find is in agreement with observations has been debated for over three decades, without a decisive mechanistic answer to my knowledge. Xu et al. report here an AMOC structure that is very close to the present-day one, albeit a bit shallower for the GNADW component. Whether this is or not in agreement with data, previous modelling studies etc. should be discussed in details. However, I found very little discussion of the matter: a description is provided on pp. 4891-4892 and the discussion only states *"simulated hydrological conditions, the behavior of the AMOC in our MPI-OM LGM simulation generally agrees with a robust AMOC scenario, which is constrained by observations (Lynch-Stieglitz et al., 2007). Therefore we rate our modelled LGM simulation results as a reasonable glacial circulation, suitable for our isotope studies."*. The latter statement is at the least careless for the large body of litterature that exist on the topic (see refs in Lynch-Stieglitz et al. 2007 for a start). As a large part of the conclusions they draw from their simulations is based on the fact that there is little change in the AMOC in LGM (see figure 7, panels a and c) there is a crucial need to assess those first.

2./ δ^{18}**O**$_c$ **database**. Since their is no database compiling $\delta^{18}O_c$ published before the present manuscript was submitted (as noted in the MARGO - related discussion), there is a need for such comparison to compile a database from

available records of $\delta^{18}O_c$ that include the LGM. The authors propose their own compilation in the submitted comment. Being myself a modeller, I am not an expert on those matters, which is why I discuss the matter with a colleague who is an expert on it (this is noted in the communication to the editor as well for clarity). Our assessment is that the method presented is not acceptable in the present form. Indeed, compiling all available records spanning the LGM and deriving precisely the LGM value given the uncertainty in chronology is an enormous work. The simple table given by Xu et al. as attachement is far from being what is expected. What is the definition that the authors used for the LGM? How did they intercompare the different chronologies used in the different papers to ensure they were looking at the same chronozone? How is the intercomparison and uncertainties in the intercomparison of $\delta^{18}O_c$ between different laboratories taken into account? A complete and precise answer to all these questions is **required** for the reader to accept the database provided as faithfull and likely to represent the current state of knowledge of surface $\delta^{18}O_c$ for the LGM chronozone. The discussion provided in the current version of the manuscript falls short of the mark.

3./ Since the manuscript have been heavily changed between the initial submission and the first online author comment, the text should reflect that change. This is not the case in several places, in particular pages 4897, 4902.

Other comments

1./ p. 4888, line 2-3 *"are implemented as passive tracers in terms of mass in MPIOM-wiso"*. How do you ensure conservation in the model? Does MPIOM ensure conservation of water mass as well as volume with variable layer thickness? Even in free surface models, the concentration is conserved, but rarely the mass.

2./ p. 4889, line 11-12 *"have been run for 3000 yr into a quasi-steady state."*. Please indicate the drift in the deep north Pacific for example, in per mil per century so that the reader can appreciate the steady state. To ensure full steady state (which have no reason to be more representative of the LGM) ones classicaly needs 5 to 6 millenia.

3./ p. 4891 and figure 1: the sea-ice concentration given is 50%. This choice is odd. The classical sea-ice concentration that mark the sea-ice edge and favourably compares to data is 15%.

4./ p. 4892 and figure 3: please shift the PD value by 1 per mil so that the reader can easily follow your description.

5./ p. 4895, lines 3-23: in your discussion the waters are depleted everywhere albeit in the surface ocean. *"Only the subsurface waters at tropical and subtropical regions are slightly enriched (+0.1 per mil)."* How come? Since the total water isotopes mass is conserved, the volume of waters where the content is increase and decrease should be even (after correction of the glacial 1 per mil). Are the waters in the Indian Ocean (not shown) all positive? How is the re-distribution promoted in details?

6./ Figures 5, 7 and 9 the colorscales are not adequate. All the discussion in the manuscript concentrate on the two-three colors at the center of the colorscale.

Please use a non-linear colorscale to highlight the interesting aspects you are discussing. Same applies to figure 13.

7./ p. 4899, line 14-15: *"LeGrande and Schmidt, 2006"* do not provide proxy reconstructions.

8./ p. 4899, line 17-18: *"The decrease in d18Ow is possibly due to less evaporation during the LGM"*. You are using a ocean only GCM with fixed boundary fluxes: you can check that.

9./ p. 4899, line 21-23: *"During the LGM, the closed Canadian Arctic Archipelago prevents the depleted Arctic water from entering Baffin Bay, which induces a further enrichment of Baffin Bay and Labrador Sea water masses."*. I do not agree with that explanation. If you compare figure 4 panels a and c, there is a clear advection of high $\delta^{18}O_{sw}$ in that area from the Atlantic. Please discuss the changes in the surface currents and why the input of very depleted routing water in the Labrador Sea from the neighbouring Laurentide ice-sheet do not act as a counter factor.

10./ p.4900, line 13-14: *"No direct measurements of the LGM sea surface waters' isotopic composition exist, which makes it difficult to validate the related model results."* That is true but Jess Adkins provided some pore waters measurments for deeper waters. Please include thoses in your discussion. Cited ref: ADKINS, J., MCINTYRE, K. et SHRAG, D. (2002). The Salinity, Temperature, and d18O of the Glacial Deep Ocean. Science, vol. 298:pp. 1769–1773.

11./ p.4901, line 14-16: *"The other way to look at this point is that either the prescribed global mean ice sheet effect in this region is underestimated, ..."*. What do you mean? A prescribed global change cannot be locally underestimated if it is applied globally! Please rephrase.

12./ p. 4901, line 26: *"This assumption, which excludes any temporal storage of glacial precipitation on the ice sheets, may introduce unrealistic river discharge into the polar seas, leading to highly depleted waters in this region."*. I do not agree with this argument. The LGM is the period when the ice-sheet stabilised. Thus, the water budget is in reality similar to yours. Even more so, since the European ice-sheets are already shrinking around the LGM, so the input of freshwater with very negative isotopic content should be important in the Artic.

13./ p. 4902, line 23-25: *"differences as compared with the observations point to around 3C cooling of the SST, which is comparable to the estimation by combining different proxies"*. This is true only if your surface $\delta^{18}O_{sw}$ is perfect. Since the temperature effect is dominant, this is not very problematic but should be mentionned. Please rephrase to take all uncertainties into account.

14./ p. 4903, line 1-3: *"Additionally, the closed water mass balance assumption in runoff calculation will obtain impractical river discharge into the polar seas, and simulate too depleted d18Oc values at these areas."*. See my previous comment on the topic.

15./ Figure 12 (figure 2 of comment): please explain in details what is going on with Pachyderma.

Interactive comment on Clim. Past Discuss., 8, 4885, 2012.