Geosci. Model Dev. Discuss., 5, C126–C128, 2012
www.geosci-model-dev-discuss.net/5/C126/2012/

Geoscientific
Model Development
Discussions

Interactive comment on "Water isotope variations in the global ocean model MPI-OM" *by* X. Xu et al.

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Received and published: 5 April 2012

'This paper is a clear description of the results of a water isotope-enabled ocean GCM. There are a few issues that require more detailed exposition, but the paper is publishable after those minor revisions.'

Thank you for the comments. They are very helpful in improving our paper. The following are item-by-item responses to the comments.

1. 'As the authors are well aware, the water isotope distribution is driven by the surface boundary conditions (with the exclusion of minor effects associated with sub-ice shelf fluxes and-sub surface marine ice formation). Thus particular care has to be taken to examine what impact various assumptions made have on the solution. Specifically, because this simulation is an ocean only simulation which contains a necessary (though unfortunate) salinity restoring term to maintain the circulation, there is a clear unphys-

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ical aspect to the surface fluxes. i.e. the restoring imparts an implicit freshwater flux to the surface, which implicitly has an isotope flux value of the flux times the tracer concentration at the surface. It would be useful to examine where in the model this was seriously affecting the solution - i.e. by plotting the ratio of this (a-physical) flux to the actual fluxes (via E, P or runoff). This matters because of the desire of the authors to examine isotope-salinity slopes, and where the implied fluxes are significant, those slopes will not be reliable. A figure quantifying this would therefore be welcome.'

Thank you for this suggestion. In our simulation, the salinity and temperature boundary conditions are deliberately chosen in such a way that the model MPIOM is close to observations. We realized that the only way is to implement the underired restoring in sea surface salinity. Our relation between oxygen-18 and salinity is therefore determined by the modeled oxygen isotope tracers as well as the sea surface salinity which is close to observations. We are aware that this is not the optimal solution, but we cannot see an alternative. We added therefore some text to explain this matter and state that the relationship between salinity and oxygen-18 is not purely determined by the model, but also by the salinity data. We hope to overcome this drawback in the near future when applying a fully coupled set up.

Minor Points: 1. 'This paper is described as a MIP description paper, but I see no mention of what MIP this is associated with.'

This paper here is more like a model development description paper. We plan to complete the isotope hydrology in the earth system model COSMOS (a coupled atmosphere-ocean-vegetation model). This paper presents the isotope simulation in the ocean part within this framework.

2. 'p282. line 5. The sea ice fractionation is non-zero during ice formation, and this shouldn't be difficult to implement. Ocean isotopes in sea ice formation zones will therefore be slightly over-depleted (and isotopes in areas of sea ice melt, slightly overenriched) without this. Perhaps this could be quantified in the meantime - i.e. what is

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the isotopic composition of the sea ice compared to observations (Hajo Eicken et al)?'

For simplicity, we have excluded any small fractionation processes during ice formation at this first stage of implementing water isotope diagnostics into MPIOM. We agree that the problem of over-depleted/enriched values exists but is minor, and we state this point in the discussion. We are currently working on including this fractionation effect into our model and will then compare the new results with observation and this first simulation where we excluded the sea ice effect.

3. 'The observations/model comparison plots (figs 4 & 6) should have an aspect ratio of 1 (i.e. they should be square) - it is much easier to assess the offsets in that case.'

Revised accordingly

4. Note that the Med data from Gat et al (1996) – seen clearly as the most enriched values in fig 8 – do not appear to be reproducible, and may be corrupted. Cox et al, 2011 (http://www.ocean-sci-discuss.net/8/39/2011/osd-8-39-2011.html). It is interesting that the simulations here don't support the stable dD values seen in that data for the Med.

Thank you for this valuable information. We re-checked this figure, and found out our dO18-dD simulation doesn't agree with the data from Gat et al (1996). It follows the general dO18-dD relation, which is consistent with the new data by Cox et al (2011) as well as simulation by Schmidt (2007).

5. p290. lines 15-19. The use of an ocean-only simulation in paleo-climate experiments is somewhat problematic (since the surface salinity is not known and therefore the restoring term completely ambiguous). I would suggest that future work focus on the coupled OAGCM simulations.

We agree with this comment and are currently working on such a coupled model.

Interactive comment on Geosci. Model Dev. Discuss., 5, 277, 2012.