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**Geoscientific
Model Development
Discussions**

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

Anonymous Referee #1

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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.

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 unphysical 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.

Minor points:

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

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 over-enriched) without this. Perhaps this could be quantified in the meantime - i.e. what is the isotopic composition of the sea ice compared to observations (Hajo Eicken et al)?.

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.

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.

p 290. 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.

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