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

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'I have read the paper with pleasure. The text is concise, well written and easy to follow. Having the isotopes in MPI-OM and hence in the future in the coupled system ECHAM – MPI-OM is and excellent development. The status of the model is assessed with care at the end of a (too rarely seen) long equilibrium simulation. I think the manuscript is almost ready to be published in GMD, provided that the one main comment below and the few adjustments are taken. I trust the authors will achieve these and will not need to see the manuscript again.'

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

Main comment

1. 'Page 282, line 3-10 as well as page 285, line 11-25: the state of the ocean model, especially at the ocean surface strongly depends on the atmospheric forcing applied. I strongly miss an in-depth discussion of the atmospheric forcing in terms of isotopes. Indeed, the surface d18O-salinity, along with the d18O in the deep waters formed among others will depend on it. In the present state of the manuscript, there is no description that may allow us to understand the quality of the forcing (excellent for sure, since it is ECHAM-iso, but nevertheless ...) and how it compares to the observations. It does not need to be fully detailed since it was published elsewhere, but the main characteristics in terms of salinity, d18O, etc. needs to be mentioned.'

Thank you for this suggestion. The detailed description of the isotopic simulation by ECHAM5-wiso is in a recent paper by Werner et al (2011). In the revised text, we will include a short characterization to give a brief view of the quality of the isotopic forcing.

2. 'In addition, the results obtained depends as well greatly on the ocean surface assumed as a boundary condition for the atmospheric run. Could you provide a figure that shows the ocean surface as seen by the atmosphere and discuss the resemblance /differences with your own surface obtained (in terms of salinity, isotopes ...). It would be of great help to understand the likely influence of your modelling choices, as well as a first indication on the differences of the fully coupled simulation to come (though probably not so simple ...).'

There is no prescription of sea surface salinity in the atmospheric simulation. For water isotopes, the Schmidt gridded data set (2006) is interpolated to the ECHAM5-wiso grid as sea surface d18O values, and the sea surface dD values are calculated as dD = 8*d18O. Thus, the deuterium excess of sea surface water is assumed to be 0‰Ḟigures of these boundary conditions will be included in the supplement.

Other comments

1. 'You use d for the deuterium excess. A more common notation is d-excess. The latter is easier to read.' 'P. 283, line 10-12: "As same as d18O, the measurements ...":

what do you mean?'

d is revised to d-excess accordingly. The sentence 'As same as d18O, the measurements ...' means that the dD observations are selected in the same way as we applied for d18O. The phrase is re-written to avoid confusion.

2. Figure 1: it would greatly help to add the observations on the plots of the surface d18O as dots for example.

We have applied the cell-to-cell comparison in the results part. For this reason it should not be necessary to plot the station data together with the zonal mean values.

3. Figure 4: could you comment what are the points off the 1:1 line in panel B of the figure?

The points clearly off the 1:1 line are mainly located at some marginal seas, which are explained in the discussion part (P.288, line 8-12).

4. Figure 6, panel A: similarly, where are the points completely off the 1:1 line for dD?

Thank you for the question. There are a small number of points completely off the 1:1 line. We re-checked this figure and have found that these points are located at the costal line of the Weddell Sea. The depletion in our simulation is probably mainly due to the runoff calculation, which considers all the snowfall as river runoff into the ocean according to the mass balance. We will discuss this in the discussion part.

5. Figure 6, panel B: the range of misfit between your model and the observations are of 3 to 4 per mil in d18O at -2 per mil in d18O in the observations. This needs to be a bit more commented in the main text: where are those points from? Likely reason of discrepancy?

Thank you for pointing this out. We checked the points which are out of the $\pm 1‰$ range from the observations. It turns out that these points are mainly in the Gulf of St. Lawrence. Based on our model resolution, it is impossible to solve the physical

processes in this region, which may have caused the discrepancies. We will include this discussion in our text.

6. Figure 9: similarly as in Figure 1, could you add the (very sparse) data points available?

We also did the cell-to-cell comparison for d-excess, it may be not needed to add the observation data here.

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