

Response to the reviewers

We thank all three reviewers for their positive reviews and constructive comments. We have revised the manuscript as described in detail below, and we hope that we have dealt with all suggestions in an adequate manner.

Anonymous Referee #1

Werner et al., present first results of the newly developed isotope-enabled version of the Earth System Model ECHAM5/MPI-OM. They focused on two equilibrium simulations under the pre-industrial and last glacial maximum period and compare the model results with observational data and paleoclimate records in the atmospheric/continental and oceanic components. Overall, isotope variations ($\delta^{18}\text{O}$, δD) for the PI and LGM climate are in good agreement with available data, although some bias are identified and discussed in the manuscript. The paper is well write, clear and the results interesting. In particular, the authors highlight interesting results that could be further explored in the future. Among them, the assessment of the stability of the δ -T relation for LGM-PI climate changes reveals that the temporal δ -T gradient might have been substantially lower than the modern spatial one for most mid- to high-latitudinal regions. Such a deviation could indeed cause a strong bias in the “classical” δ -paleothermometry approach. Also, the remarkable improvement in modelling the deuterium excess signal allows to question the approach by Merlivat and Jouzel (1979), question the cooling of SST during the LGM and support the “classical” interpretation of dex changes in Antarctic ice cores as a proxy for SST changes in the source regions of water transported to Antarctica. I think this paper is suitable for publication in GMD and I recommend publication after the authors have adressed the moderate/minor comments below.

Comments:

1) page 8837 Lines 15-19 : Some studies concerning Chinese speleothem suggest that $\delta^{18}\text{O}$ variations reflect changes to regional moisture sources and the intensity or provenance of atmospheric transport pathways (LeGrande and Schmidt, 2009; Dayem et al., 2010; Lewis et al., 2010; Maher and Thompson, 2012; Caley et al., 2014; Tan, 2014).

We have added these findings and the related references in the revised manuscript.

2) page 8841 lines 25-26 : “under pre-industrial and LGM, defined as the period 23 000–19 000 years before present” A reference is needed here.

We have decided to delete “defined as the period 23 000–19 000 years before present” as we realized that this has been a misleading statement in this part of the paper.

For the LGM simulation, glacial boundary conditions correspond to 21ka B.P. in accordance with PMIP3 rules, while the different LGM data from ice cores, speleothems, and marine records has been selected from the period 23ka -19ka (22ka-19ka for speleothems). This selection is described in detail in Chapter 3.2-3.4.

3) page 8846 line 12 : “with a prescribed glacial increase of $\delta^{18}\text{O}$ of +1 ‰ (δD : +8 ‰” Here, the authors prescribed a glacial increase of 8 ‰ in δD for the LGM period.

According to Schrag et al., 2002, the glacial increase would be around 7.2‰. Also, if all the GISS data (depth < 3000 meters) (Schmidt et al., 1999) are used, the present day relationship between $\delta^{18}\text{O}$ and δD give a glacial δD increase of 7.3‰ for a $\delta^{18}\text{O}$ value of 1‰ (assuming that this relationship is still valid during the LGM). Therefore, the two independent approaches lead to a δD increase of 7.2‰ rather than 8‰ during the LGM. What could be the implications of such a different value on the deuterium excess calculation presented in this manuscript in part 4.2.4?

A mean glacial ocean δD increase of +7.2‰ instead of +8‰ would lead to a small glacial decrease of the mean deuterium excess signal in the ocean of -0.8‰. As a first-order estimate, one can assume that such lowered deuterium excess signal in the ocean will lead to an equivalent lower deuterium excess value both in vapour above the ocean and, consequently, in precipitation, too. We have added these considerations in Chapter 4.2.4.

4) page 8848-8849 Kim and O'Neil 1997 equation. I don't understand how the data-model comparison is done. Does the authors have used the temperature in the model and the d^{18}O of the calcite from speleothem data to calculate a $\text{d}^{18}\text{O}_{\text{water}}$ value and then compare this to model values in Figure 1? On figure 1, there is only a scale of d^{18}O in precipitation and the speleothem records are included. Please explain in more details how the $\text{d}^{18}\text{O}_{\text{water}}$ of speleothems are calculated (which temperature values are used?).

We thank the reviewer for this very helpful comment. For the conversion of modern $\delta^{18}\text{O}_{\text{c}}$ of calcite from the selected speleothem sites (Table 2) to a $\delta^{18}\text{O}_{\text{p}}$ water value shown in Fig. 1, we have used annual mean ERA40 soil temperatures (ERA40 variable soil temperature, layer #1) averaged over the period 1961-1990. We have forgotten to mention this detail in our original manuscript but it is now explicitly stated in part 4.1.1.

In the legend of the figure 1, the Table 1 and Table 2 do not refer to the corresponding dataset, please inverse.

Corrected.

On Figure 8, I am again confused because the speleothem data are presented in green on a $\text{d}^{18}\text{O}_{\text{precipitation}}$ scale but the figure caption mention that the $\text{d}^{18}\text{O}_{\text{calcite}}$ changes are shown. I recommend to use atmospheric temperature to calculate the $\text{d}^{18}\text{O}_{\text{p}}$ of speleothem and then plot this on figure 1 or 8. Alternatively, the authors could separate the speleothems data and compare the $\text{d}^{18}\text{O}_{\text{calcite}}$ data with $\text{d}^{18}\text{O}_{\text{calcite}}$ of the model (calculate from temperature and $\text{d}^{18}\text{O}_{\text{p}}$ from the model) as it was done for marine carbonates.

We are sorry for this confusion. For the model-data comparison of the LGM-PI speleothem data, we have calculated the simulated LGM-PI change of $\delta^{18}\text{O}_{\text{c}}$ in calcite, using both modelled $\delta^{18}\text{O}_{\text{p}}$ in precipitation and surface temperatures for the PI and LGM climate, as it was done for marine carbonates. Thus, the axis labels of Fig 8b were erroneous as were compare both model vs. reconstructed $\delta^{18}\text{O}_{\text{p}}$ anomalies (for ice cores) and $\delta^{18}\text{O}_{\text{c}}$ anomalies (for speleothems) in the same scatter plot. The labels and figure caption of Fig. 8 have been corrected and we now describe this comparison in more detail in paragraph 4.2.2.

5) Page 8849 Shackleton (1974) equation. There is a conversion between the two scale (PDB and SMOW): expressed as $d18O_{water(VPDB)} = d18O_{water(VSMOW)} - 0.27$ (Hut, 1987) that is not describe here and that is necessary.

This information is now added to paragraph 3.4

6) Conclusion part, page 8866, line 29 "CLIMAP". I think this is cooler than MARGO, not CLIMAP.

Corrected.

7) Figure 4 : "arbitrary subset of 300 data". I rather suggest to the authors to revise the figure and show the model results without data on a new panel a) and add on a secondary panel with all the GISS data (Atlantic Ocean: n = 5811, Pacific Ocean: n = 2985) with or without model results. The comparison between model and all the GISS data will be possible with readability.

We have revised Figure 4 as suggested. The figure now includes two new panels c) and d) that display all available GISS data for the Atlantic and Pacific Ocean. The figure caption is changed, accordingly.

Anonymous Referee #2

The manuscript by Werner and co-authors presents first results on the pre-industrial and LGM conditions of a coupled ocean-atmosphere model equipped with water isotopes. The spatial repartition of $d18O$ and d -excess in the ocean and atmosphere is systematically confronted to available data for both periods with a general good match between model and data. The manuscript is very well written and provides all the necessary details in the text and in the figures for the general reader. The number of figures is quite high but I would recommend keeping all of them. I only have minor comments and I recommend publications of the manuscript:

-p. 8840, l.4: it would be nice to quote also the 2013 paper by Risi and co-authors on $17O$ -excess modeling.

The reference has been added to the text.

- p. 8843, l.22: The authors neglect as often done (but not always) the possible fractionation during evapotranspiration processes from terrestrial areas. It would be nice to expand a bit more the possible implications of such hypothesis on $d18O$ and especially d -excess in regions where the fractionation during evaporation of terrestrial water may become important (e.g. Amazonian basin following the suggestion of Gat and Matsui (1991)).

In Haese et al. (2013) we have analysed in detail the implications of a potential fractionation during evapotranspiration processes on the $\delta^{18}O$ and deuterium excess signal in different water reservoirs. We found no large effect of such fractionation processes on the $\delta^{18}O$ and d -excess signal in precipitation, but a potential large change

of the isotopic composition of soil water by several per mill (see Haese et al., 2013, Fig. 8). Such change might be especially relevant for paleoclimate records, where the isotope signal reflects changes in the soil water (e.g., speleothems, ancient groundwater). However, it remains an open question if such changes would also affect the simulated glacial anomalies ($\Delta_{\text{LGM-PI}} \delta^{18}\text{O}$, $\Delta_{\text{LGM-PI}} \text{dex}$), or simply lead to an equivalent strong change of $\delta^{18}\text{O}$ and dex for both the PI and LGM simulations (without a glacial anomaly change as compared to our chosen model setup). These considerations are now added to Chapter 2.

- In general, I think that some explanations on the added value of the coupled model compared to the atmosphere only model for the modeling of d18O and d-excess in precipitation should be given in introduction of the manuscript.

We have added some more arguments for using a fully coupled isotope GCM in Chapter 1.

- p. 8848, l. 8, replace “is” by “are”

Corrected.

- Figure 6a and corresponding text p. 8854: what do you mean exactly by isotopic values in “evaporation” ? do you mean water vapor or evaporation flux ? It would be nice to clarify since only isotopic values in water vapor can be compared to data.

In Fig. 6a, the annual mean deuterium excess values in the evaporation flux are plotted. As mentioned in the text, we are aware that this quantity is difficult to evaluate, as it has not been measured, yet. However, we prefer to show it as a counterpart to the deuterium signal in the precipitation flux (Fig. 6b). Furthermore, the deuterium excess in evaporation has recently been reconstructed by Pfahl and Sodemann (2014) and the ECHAM5-wiso results can be compared to their results. This is now explicitly mentioned in the revised manuscript.

- p 8856, l. 6: A good way to test the Merlivat and Jouzel (1979) formulation would be to look at the modeled slope between d-excess in the water vapor above the ocean and relative humidity. How does this compare to the Merlivat and Jouzel (1979) slope ?

For the region 60°S-60°N we calculate a slope m between d-excess in the vapour layer above the ocean and the related relative humidity of $m = -6.3\text{‰}/(10\% \text{ rel. humidity change})$. This is very close to the value of $-6\text{‰}/(10\% \text{ rel. humidity change})$ given in Merlivat and Jouzel (1979). We have added this information to the manuscript.

- I am quite convinced by the discussion on the influence of SST on d-excess presented on p. 8864. Still, it would be nice to justify further why the relative humidity of the source relative humidity was not different by more than 5% in the LGM compared to the pre-industrial situation.

The rather small variations of the LGM relative humidity changes are somewhat surprising, as cooler SST should lead to cooler air temperatures above the ocean surface, which then should lead to higher relative humidity levels (if the amount of water in the air stays constant). However, we find in our simulation that the air directly above the

ocean surface cools slightly stronger during the LGM than the SST themselves. This leads to a reduced glacial evaporation flux from the ocean to the atmosphere, which decreases the relative humidity of the vapour and counterbalance the first effect.

Similar small changes of relative humidity changes above the ocean surface and the counterbalance of different effects have recently been reported for a set of CMIP5 climate model results by Laîné et al. (2014). They have analysed a future water climate, though.

Anonymous Referee #3

Summary: The authors present results from a pre-industrial and Last Glacial Maximum simulation of climate using the isotope enabled version of the coupled ocean-atmosphere model ECHAM5/MPI-OM. This is a sound manuscript. I would suggest it requires only rather minor revisions before publication.

Major comments: I have only one more major comment which is on section 4.2.4 “Glacial changes of the deuterium excess”. It is really interesting that the authors find that glacial sea surface temperature which are cooler than the GLAMAP reconstruction, lead to an improved simulation of dex changes over Antarctica. Would it be possible to also comment on whether coupled model ECHAM5 simulation of sea ice around Antarctica is also in agreement with the available sea ice data e.g. from Gersonde et al.?

The simulated sea ice of the COSMOS LGM simulation has already been described in detail in Zhang et al. (2013) and our simulation results are comparable to this previous study. For the southern hemisphere, there is a reasonable agreement between the simulated sea ice concentration and proxy data by Gersonde et al. (2005), such as the austral winter sea ice extent in the Atlantic sector and the austral summer sea ice extent in the Indian ocean sector. However, our LGM simulation underestimates a larger extent of sporadic summer sea ice between 5°E and 5°W in the Southern Ocean, as reported by Gersonde et al. (2005).

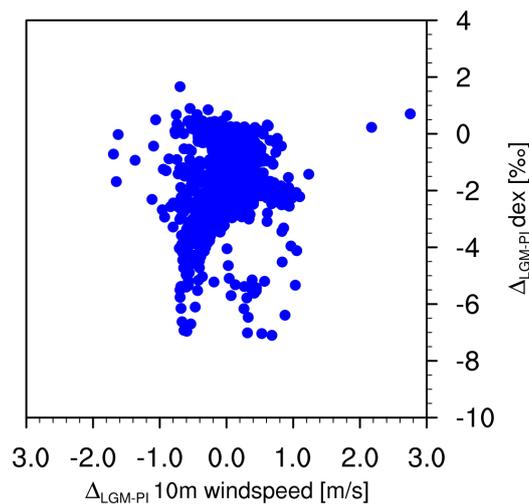
Compared to GLAMAP, we find a much reduced sea ice cover in austral summer. This might lead to a stronger contribution of vapour stemming from regions between 60°-65°S to the Antarctic ice sheet. As vapour from these regions has a strong negative dex signal (cf. Fig. 12) such shift in the contribution might lead to a more negative deuterium excess signal in precipitation, too. These considerations are now added to the manuscript (part 4.2.4.).

Whilst plotting simulated changes of dex in vapour against the modelled relative humidity change between LGM and PI over the ocean surface reveals no correlation between these humidity changes and the simulated dex variations in the vapour layer, these are over rather large changes in climate, with many changes in climate variables. Some work, such as that by Schmidt and LeGrande using the gissE model, indicates that near surface wind changes may also be important in dex changes. Examining the correlation and relationship between dex and relative humidity, and dex and SST does not eliminate the possibility that the dex-SST relationship could be dependent on other aspects of the climate shift – such as wind speed changes. It would therefore be useful if

the authors could support their dex-SST relationship assertion by providing a much wider examination of dex-climatic variable relationships.

Our intention of analysing the dex relation vs. relative humidity and SST changes was to simply test the recent hypothesis of Pfahl and Soedemann (2014) regarding an improved interpretation of dex variations in ice core records. Of course, we fully agree that the shown correlation between dex and SST changes does not rule out other factors, like wind speed changes, which could affect both dex and SST changes. (However, we do not find a clear correlation between the simulated annual mean glacial 10m windspeed anomalies and the dex signal in the vapour above the surface; see Figure below). These considerations are now mentioned in the manuscript.

More sensitivity studies and analyses are certainly required to explain the simulated glacial dex changes both in vapour and Antarctic precipitation in more detail. We think that such analyses are well beyond the general scope of this manuscript (which is a first, rather general presentation of this new fully-coupled isotope model setup). Thus, we prefer to perform such wider examination of dex-climatic variable relationships in a separate future study, and hope that this decision is adequate.



Minor comments:

P8837 L15 “the combination of water isotopic ratios permits to have a tracer of the low latitudes in polar ice cores” provide a reference, and perhaps make the reference to d-excess more explicit?

We have added 2 references here but do not explicitly mention the deuterium excess at this point, as we discuss this quantity in detail just a few lines below (p.8838, line 4ff.)

P8838 L2 “that they allow reconstructing the three-dimensional structure” rephrase, for example “the reconstruction of” would be better.

Rephrased as suggested.

Section 4.1 Might be better to also include a present day simulation. This would enable the authors to also test the simulation against isotopes in vapour satellite data.

In several previous studies, we have evaluated present-day simulations of the atmospheric model, ECHAM5-wiso, in detail. Some of these studies also included a comparison of model results to available satellite vapour observations both on a global and regional scale (e.g., Werner et al., 2011; Butzin et al, 2014). As the overall results of the coupled ECHAM5/MPI-OM setup are very similar to these previous atmosphere-only ECHAM5 simulations, we decided against a duplication of such detailed present-day vapour data analysis in this study, but rather focus on the simulated modern vs. glacial isotope changes. We hope that this decision is acceptable for the reviewer.

P8864 L1 “constraint”

Corrected.

Additional remark

Very recently, the supercomputer at AWI, which has been used for conducting the presented simulations, has been replaced by a new machine. For future work and consistency, we have decided to prolong both the PI and LGM simulation to a total of 1,500 simulation years (before: PI experiment: 1,400yrs, LGM experiment: 1,300yrs) using this new hardware configuration. In the revised manuscript, we now present the results of the last 100 simulation years (year 1,400-1,499) of both prolonged simulations.

None of the originally submitted results has been affected by this prolongation, but some calculated quantities (mean values, RMSE, isotope slopes, etc.) have slightly changed.

All figures have been updated, too. Only a few of them display very minor changes in global isotope distributions with respect to the previous figure versions.

References

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- Haese, B., Werner, M. and Lohmann, G.: Stable water isotopes in the coupled atmosphere–land surface model ECHAM5-JSBACH, *Geosci. Model Dev.*, 6(5), 1463–1480, doi:10.5194/gmd-6-1463-2013, 2013.
- Laîné, A., Nakamura, H., Nishii, K. and Miyasaka, T.: A diagnostic study of future evaporation changes projected in CMIP5 climate models, *Clim. Dyn.*, 42(9-10), 2745–2761, doi:10.1007/s00382-014-2087-7, 2014.
- Zhang, X., Lohmann, G., Knorr, G. and Xu, X.: Different ocean states and transient characteristics in Last Glacial Maximum simulations and implications for deglaciation, *Clim Past*, 9(5), 2319–2333, doi:10.5194/cp-9-2319-2013, 2013.