Response to Reviewer #2

Author's Response (AR): We would like to thank the reviewer #2 for his/her comments on our manuscript and the suggested corrections/improvements that he/she wrote. We answer below to each point raised by the reviewer.

Interactive comment on **"Impact of the oceanic geothermal heat flux on a glacial ocean state"** ***by*** **M. Ballarotta et al.**

Anonymous Referee #2

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The authors aim to show that potentially oceanic geothermal heating (OGH) has a significant impact on the Last Glacial Maximum (LGM) ocean state. To this end, they carried out two simulations with a numerical ocean model under LGM boundary conditions - one without, the other with OGH.

My main concern with this particular methodology is as follows: In my view, a comparison to the impact of the geothermal heat flux on the present-day state in the same ocean model and a comparable configuration (without salinity restoring) is missing. A second pair of "control" experiments "GH_control" vs. "REF_control" is needed for present-day conditions to allow for a meaningful assessment, minimizing the influence of the "model error" that arises from using different ocean models. The present-day simulations referred to in the Discussion section (p. 3603) were either carried out under different boundary conditions (with salinity restoring - Emily-Geay and Madec, 2009) or with a different ocean model (POTSMOM-1.0 - Hofmann and Maqueda, 2009).

Furthermore, in the present manuscript the simulations are not shown to be consistent with reconstructions (they are not "validated"), except for the temperature and salinity of the deepest waters in Section 3.1. Hence it is not clear in which sense the GH and REF simulations represent the LGM ocean state, other than that the atmospheric forcing fields from Brandefelt and Otto-Bliesner (2009) were obtained under LGM boundary conditions. As long it is not clear which ocean state is represented, it is not possible to assess in which way the results add to the previous work by Emily-Geay and Madec (2009) or Hofmann and Maqueda (2009).

AR: Our simulations are constrained with atmospheric forcing representative of the LGM, the quasi-equilibrated LGM2 period from Brandefelt and Otto-Bliesner (2009). Note that their LGM1 period of simulation (non equilibrated) is quite different from LGM2, and is still representative of the LGM. All the

studies about the impact of OGH during present-day ocean state have been done with ocean models of relatively similar resolution and experimental design. The main idea behind our study is to investigate the impact of the OGH during a glacial period, when the ocean structure was different from today. We simulate a glacial state (as we wrote in the results section when we present the zonal structure in temperature and salinity) and show that the OGH acts differently in glacial state.

As we wrote in the conclusion, we are aware of the limitation of the present study. Experiment with climate model will probably be more robust but the state of the art climate models do not allow to run 15000 years to investigate the impact of the OGH. It will take several months to run these experiments.

In this regard, the coarse-resolution simulation of the LGM ocean referred to in Ballarotta et al. (2013a) is actually compared to reconstructions, but this simulation may not be consistent with the current REF simulation as it is initialized from different temperature and salinity fields by Brandefelt and Otto-Bliesner (2009). It is interesting to note that the article by Ballarotta et al. (2013a) also lacks a consistent control simulation.

AR: Although the initial states used in \cite{Ballarotta2013} and in the present study originate from different models, they have similar structure: the temperature, salinity and sea-ice cover distribution nearly are similar, in particular the deep saline waters in the abyss and the simulated large sea-ice cover. We mention these similarities in the new version of the manuscript from line 92.

Note that a control simulation has been done in Ballarotta (2013), see Supplementary Material and in particular the discussion paper: "A Last Glacial Maximum world-ocean simulation at eddy-permitting resolution – Part 1: Experimental design and basic evaluation".

Further points

1. How sensitive are the results to the selected parameterization of vertical mixing (p. 3601)? In the selected parameterization, what is the source of the energy for mixing? What is the "mixing efficiency"?

AR: We didn't investigate the sensitivity to vertical mixing parametrisation, as it has been done in Emile-Geay and Madec (2009). In the present study, we followed standard practice for modelling mixing with background efficiency. We are aware that the mixing in our simulations might not be completely realistic,

as in most climate simulations of the LGM. The mixing during the LGM was probably larger than today due to the emerged continental plateaus which allow energy dissipation \citep{Schmittner2015}. As a result, larger energy in the mixing was probably supplied in the ocean interior, which contributes to erode the stratification more easily. Based on the study of \cite{Emile-Geay2009}, our abyssal overturning will be larger in a context of larger mixing. Our equilibrated state will thus be reached more quickly due to the larger energy in the mixing.

We added a paragraph in the discussion section (from line 233) to discuss the importance of the mixing and to better emphasize this fact.

2. Please explain which ocean state was taken from Zhang et al. (2013) to serve as initial conditions and why the atmospheric boundary conditions were taken from Brandefelt and Otto-Bliesner (2009).

AR: We used the cold state from Zhang et al. (2013). It corresponds precisely to their MPIOM LGM-W run and the monthly mean value over the years 6100-6200. We added in the manuscript the reference to the MPIOM LGM-W on line 87. We took the atmospheric forcing from Brandefelt and Otto-Bliesner (2009) because it corresponds to a quasi-equilbrated state representative of an LGM state where the abyssal ocean is equilibrated to the LGM forcing. We now mention this from line 89.

3. How does the total energy input from OGH forcing (29.9 TW) and the mean value over the ocean (~88 mW m-2) compare to observational estimates? Are these values "realistic"?

AR: These values are similar to the present-day observations since the OGH is estimated from a model that uses the age of the bedrock expressed in Million of year. Note that these values are slightly below the recent estimate of the OGH: the mean OGH value is 95.9 mW.m^{-2} and the total global heat flux is 30 to 31 TW \citep{Davies2010,Davies2013}. We mention this in the new version of the manuscript from line 104.

4. The LGM is not a glacial period, but just a part of it. Depending on the definition, it lasts about four thousand years, about twice the characteristic time scales shown in Figure 1. Forcing was probably not constant, and the climate not in perfect equilibrium.

AR: We agree with this argument that the impact of the OGH could of course be modulated by the surface forcing (change in surface fluxes, sea-level, etc…). We

mention this in the Discussion when we discuss the impact of the OGH versus the impact of the surface forcing on the AMOC for example (from line 250).

5. P. 3622, Figure 8: It looks as if almost NADW is formed south of about 45° N, which seems strange. What do the sea-surface temperature and sea-ice distributions look like, and how do they compare to reconstructions? What would the corresponding present-day overturning look like?

AR: The NADW is formed near 45°N in our simulation. The deepest mixed layers are located southward compared to the present-day simulation due to the presence of large sea-ice cover in the North Atlantic. Similar behaviour is found for example in the simulation by Brandefelt and Otto-Bliesner (2009) (see their LGM2 period). Since we have similar surface forcing, we obtain similar pattern in the NADW circulation. We attach below the simulated SST and sea-ice cover for the JFM and JAS periods. Note that our simulation is closer to the CLIMAP (green line) reconstruction than the MARGO (black line).

Minor points

P. 3601, line 9: sea-ice dynamics [plural]

AR: This has been corrected

P. 3601, line 11: a 4000-year long [without "s"]

AR: This has been corrected

P. 3604, line 2: subtracting the streamfunction in latitude-density coordinates ["in" is missing]

AR: This has been corrected

P. 3618: Figure 2 is very small.

AR: The figure in large size has been send to the journal.

P. 3622: Figure 6 and elsewhere: please note that the volume of the lower meridional circulation cell does not necessarily coincide with the volume occupied by Antarctic Bottom Water, because circulation boundaries do not necessarily match water-mass boundaries.

AR: We added this remark in the caption

Overall, the manuscript is well structured and well written. Once the methodological issues were taken care of, it would certainly present results that would be relevant to the paleoclimate community that is interested in the reconstruction and modeling of the LGM ocean.

References

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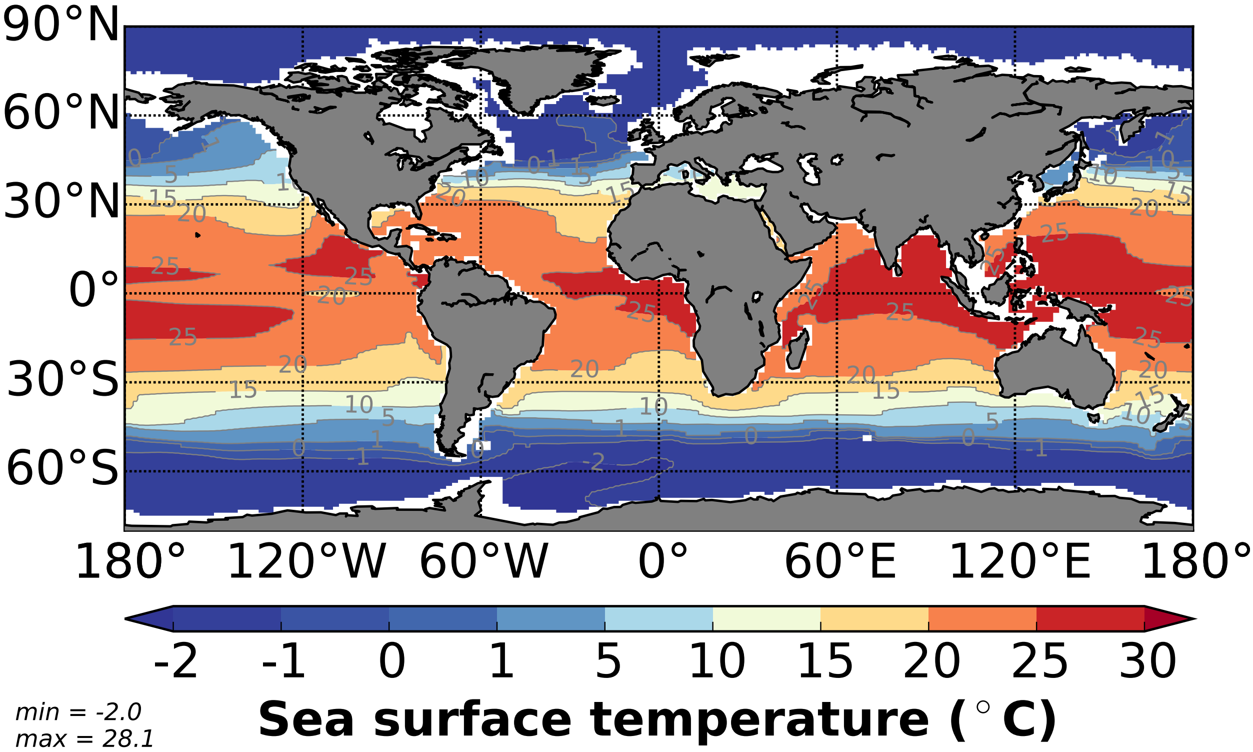
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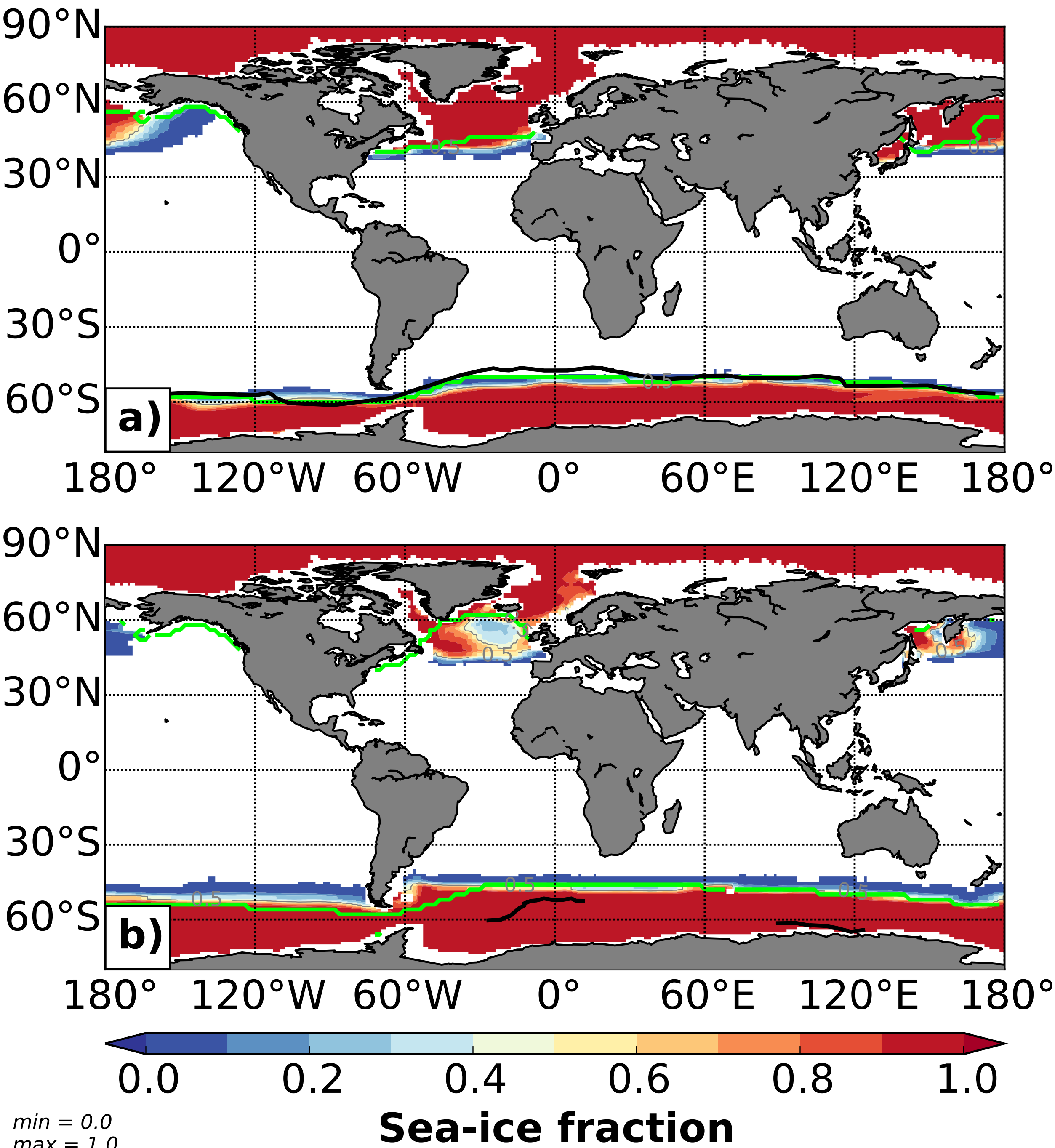
Sea surface temperature (°C)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min = 0.0
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Sea-ice fraction