Reply to Referee #2, by V.L. Meccia and U. Mikolajewicz

Review of the paper entitled "Interactive ocean bathymetry and coastlines for simulating the last deglaciation with the Max Planck Institute Earth System Model (MPI-ESM-v1.2)" by Virna Loana Meccia and Uwe Mikolajewicz.

This paper has been long awaited by the community working on the last deglaciation from LGM to present day. But, in fact, this methodology could also be interesting for simulations for future deglaciations of Greenland and West Antarctica in the next century.

Indeed in the framework of PMIP4 deglaciation project (Ivanovic 2016) in which models intend to provide transient simulations from LGM to PD, such tool is absolutely needed.

The authors aim to use the MPI ESM to produce deglaciation transient runs. They cope with a long lasting issue and resolve it: how to modify boundary conditions that account for sea level rise during the deglaciation and modify the topography (bathymetry and coastal lines) all along this process using algorithms that avoid manual and more or less subjective corrections. They describe the algorithms they used for adaptation of the ocean model MPIO at low resolution used in the PMIP4 exercise with boundary conditions evolving every 10 year.

The paper is well written and its structure is clear. The detailed description of strategy target and problems is convincing.

We thank Referee #2 for his/her useful comments. We give a detailed response to each issue in what follows.

My major comments are the following:

1. the paper is perfectly suited for GMD. Nevertheless the authors never tackle the effect of their boundary condition changes on deglaciation. Therefore I suggest that they address this question at least concerning two important points

We propose this paper to GMD as a "Development and technical paper" because it presents a novel methodology consisting of several steps tackling a challenging technical problem. We believe that a detailed description of the algorithms deserves a paper itself and therefore we are submitting a purely technical paper. Thus, we are not aiming at analyzing the climate response to a changing bathymetry and land-sea mask in this study. The effects of including our algorithms in a transient simulation of the last deglaciation in terms of the climate response will be faced in another paper and it is an ongoing work.

- **Discussing the added value of this study compared to previous simulations where the bathymetry was not changed to better emphasize what may be the interest of this study beyond the technical challenges.**

  Discussing the added value of applying the algorithms described in the manuscript in comparison to a simulation in which the bathymetry and land-sea mask are fixed is for sure a very interesting and necessary issue. However, as mentioned before, it is not the aim of our manuscript and it will be the topic of another paper. We believe that the effects of including a variable topography for simulating the last deglaciation deserve a detailed study itself. Indeed, we have run the model with the same conditions as the ones described in section 3 of the original manuscript, but with fixed bathymetry and land-sea mask to the LGM, that is, without applying the algorithms. Figure A1 shows an overview of a comparison between both simulations. As an example of some variables, we plotted time series of a) AMOC at 26N and 1000 meters depth; b) sea-ice extent in the Arctic; c) global SST and; d) global SSS for the run with variable (red) and fixed (blue) bathymetry and land-sea mask. We observe differences in the behaviour of the variables, particularly from 14 kyrs BP onward, when the ice-sheet melting rate is high and the changes in the coastline are large. Therefore, there are substantial differences between both simulations. However, a detailed study would be needed to explain the effects of applying our methodology in terms of the physical mechanisms and the climate response. We are planning to face it in another paper. In our current manuscript, instead, we intend to present the technical problem and the way we propose to solve it as a “Development and technical paper”.

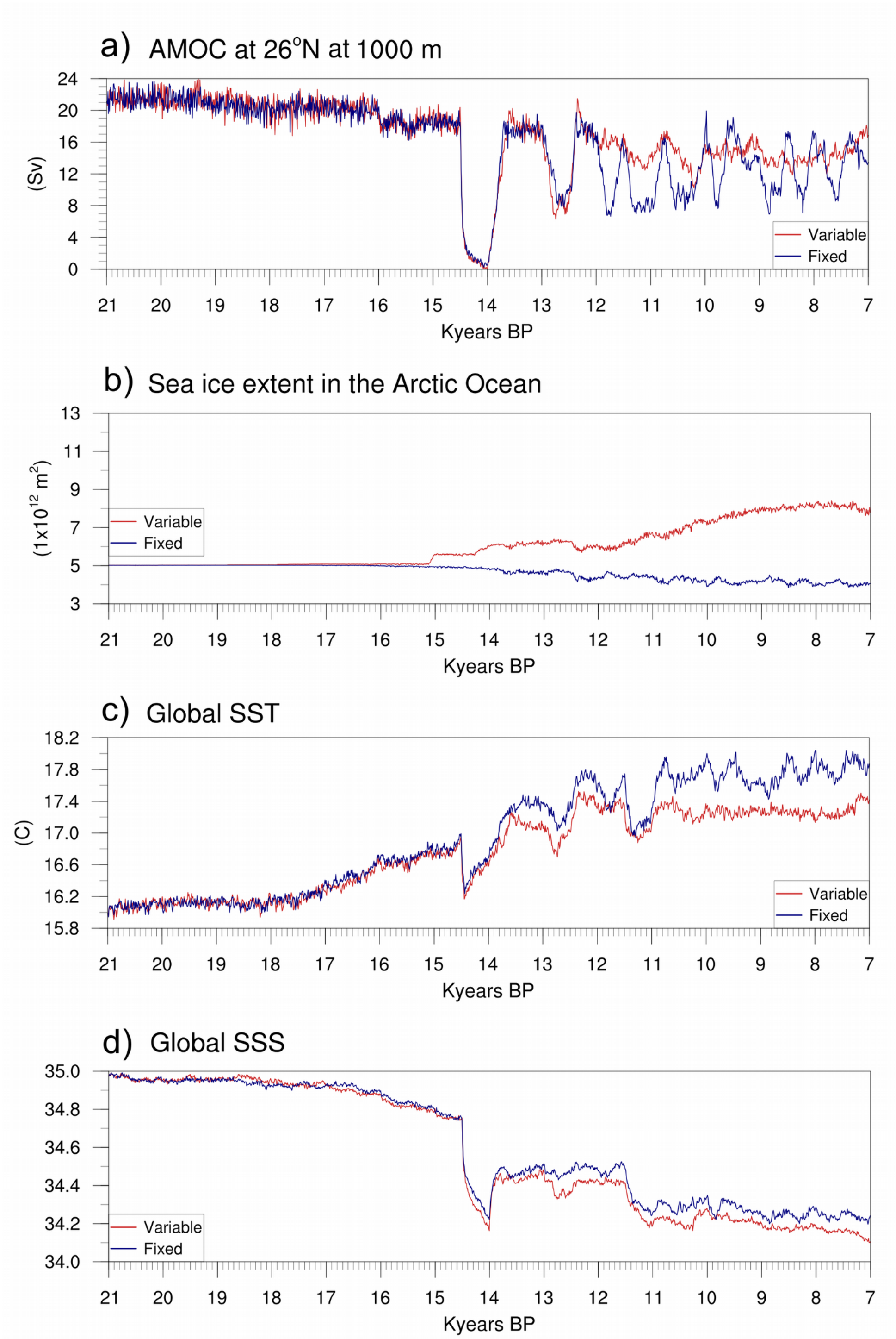

Figure A1: Time series of a) AMOC at 26N and 1000 meters depth; b) sea-ice extent in the Arctic; c) global SST and; d) global SSS for a simulation of the last deglaciation with variable (red) and fixed (blue) bathymetry and land-sea mask.

- **The authors should also emphasize the potential limitations of this method in terms of simulating abrupt events during deglaciation due to many linear processes they used, both in time and space. I perfectly understand smoothing procedures the authors described to avoid crash of the model, but during deglaciation many non linear changes occurred for**

instance MPW and more generally acceleration of melting rates described for instance in C. Waelbroeck et al., Quaternary Science Reviews 21, 295-305, 2002, for the last 30k. Therefore the authors should discuss in more details what is the compromise between avoiding crash and capturing real non linear events.

We apply our methodology to MPIOM, the ocean component of the MPI-ESM. We are not computing a variable topography in response to the melting rates and the isostatic adjustments. Instead, our algorithms read the topography fields in high resolution and construct a usable bathymetry to run the ocean model in a coarse resolution. Thus, changes in topography due to the ice-sheet growth or decay and the isostatic adjustments of the bedrock are prescribed input data for our tool. In the experiment we present in section 3 of the manuscript, we use the ICE6-G reconstructions to construct the prescribed high-resolution topography. Changes in topography can also be solved by an ice-sheet model and a solid earth model coupled to the ESM, but these changes are computed neither by the ocean model nor by our algorithms. In that sense, the abrupt events and non-linear changes in the melting rates that took place during the deglaciation are not affected by our procedure. We agree with the reviewer, that the abruptness of some of the past changes is not captured by linearly interpolating between time slices 500 years apart. We did not produce these data, so we had to work with what was available. However, we should stress, that the PMIP deglacial simulation is not the goal, but only a simple test bed. Our ultimate goal is the fully coupled model with atmosphere, ocean, ice sheets and solid earth, which automatically generates higher resolution (in time) signals.

When applying our methodology in a transient simulation, the changes in bathymetry and land-sea mask are limited for the ocean model, but those limitations are not affecting the evolution of the bottom topography due to the ice-sheet retreat. The algorithms read the high resolution topography and allow only small changes when generating the coarse resolution bathymetry to run MPIOM. This fact can slow down the flooding and drying events of the shelves regarding the ocean domain. Therefore, due to the smoothing method, the propagation of the coastline is affected. In any case, if this is a problem for the solution, the algorithms can be applied more often (every year, for example). From the results shown in section 3 of the manuscript, we conclude that changing the bathymetry every 10 years during the last deglaciation is an optimal compromise for our model setup between both, model performance and computing time. In general, the stencil for adaptation could be widened, which would allow a faster flooding of e.g. the Hudson Bay. This might be necessary also when using a model with higher horizontal resolution.

We clarify this point in section *4 Remarks*:

“There are mainly three limitations in our technique. First, the fact that changes in depth and coastlines are limited can slow down the flooding and drying events of the shelves. However, it is important to note that changes in topography in response to the ice-sheet retreat and isostatic adjustments are solved neither by the ocean model MPIOM nor by our algorithms. Instead, the HR topography is prescribed to our tool or solved by the ice-sheet model. In this sense, the non-linear changes or abrupt events that occurred during the last deglaciation are not affected by our methodology. Still, if the timing of the flooding and drying events of the shelves is considered to be critical, the algorithms could be applied more often within the simulation (every year, for example). However, in MPI-ESM, changing the topography implies also changes in the river routing and the land mask for the atmospheric model. Therefore, there should be a compromise between the frequency that topography is being changed and the computational time. From our results, we conclude that changing the bathymetry every 10 years during the last deglaciation is an optimal compromise between both, model performance and computing time. Another possibility would be to widen the stencil used for collecting water for new ocean points. This would allow a faster propagation of coastlines by more than one grid point per iteration. This might also turn out to be necessary when applying the tool to ocean configurations with higher horizontal resolution."

2. The authors should also clarify the part of the paper that may be directly useful for the PMIP4 deglaciation community and those that have been developed specifically for MPI ESM.

We include it in section *4 Remarks:*

"Second, this tool was originally written for the curvilinear orthogonal grid (GR) with two poles. Although we presented in this paper the results for the coarse resolution GR30, the tool can be also applied for the low resolution (GR15) configuration of MPIOM. Still, for the moment its usage is limited to GR grids. We are currently working on a new version to include the tripolar (TP) quasi-isotropic grid (Murray, 1996) among the applications. In general, the algorithms are easily adapted to any ocean model that meets the same requirements as MPIOM: Arakawa-C grid in the horizontal, z-grid in the vertical including partial bottom cells, free-surface and mass flux boundary conditions. However, there are some parameters inside the scripts that depend on the grid. They are the location of each pair of points in order to perform the checking steps described in Sect. 2.1 for correcting the bathymetric details."

Whereas this paper is worth to be published in GMD, I have also minor comments that it would be important the authors answer to before publication.

Minor comments:

Abstract:

A1 What do the authors mean by conservation of mass and tracers at regional scale. It is a bit misleading in the abstract. I think the authors have in mind to keep regional conservation when changing spatial resolution. They should clarify this issue.

If some correction is needed to globally conserve mass and tracers, it is enough to distribute homogeneously a single value around the globe. By conserving mass and tracers at a regional scale, we mean that changes in a single grid point are not propagated globally. In other words, we avoid propagating water properties over long distances by affecting only regionally the potential changes in a single grid point.

We clarify this point in the *Abstract*:

"The strategy applied is described in detail and the algorithms are tested in a long-term simulation demonstrating the reliable behaviour. Our approach guarantees the conservation of mass and tracers at global and regional scales, that is, changes in a single grid point are only propagated regionally."

A2 The authors, first tackle a very general problem: the bathymetry adaptation when simulating the last deglaciation. How far the algorithm developed here, beyond grid specificity can be easily

adapted to other models. A sentence in the abstract should clarify this point.

We add a sentence in the *Abstract*:

"For the first time, we present a tool allowing for an automatic computation of bathymetry and land-sea mask changes in the Max Planck Institute Earth System Model (MPI-ESM). The algorithms developed in this paper can easily be adapted to any free-surface ocean model that uses Arakawa-C grid in the horizontal and z-grid in the vertical including partial bottom cells. The strategy applied is described in detail and the algorithms are tested in a long-term simulation demonstrating the reliable behaviour."

Introduction:

I1 The first sentence is very general and partially untrue because of some aspects of the unprecedented speed of ongoing climate change. The authors should remove or modify this sentence.

The sentence is removed:

"During the last deglaciation, the Earth transitioned from the last glacial to the present interglacial climate, experiencing a series of abrupt changes on decadal to millennium timescales."

I2 The authors should mention that major uncertainties remain on reconstruction of Antarctica at LGM. Indeed, NH ice sheet reconstructions are better constrained, whereas Antarctica ice sheet reconstruction has often been an adjustable parameter. Therefore, the authors should mention Antarctica reconstruction uncertainties at LGM both from data and models (G. Philippon, Earth and Planetary Science Letters 248 (2006) 750.)

The quality of the reconstructions is not the point of our paper. Our algorithms are applied to the ocean model and they do not care about the prescribed topography. Therefore, the tool we are presenting is independent of the uncertainties on reconstructions. We are using ICE6-G in our transient simulation just as a test case. We could also use Tarasov or even the modelled topography from the coupled ice sheet solid earth model PISM/VILMA as it is planned for the future. Anyway, we mention it in *1 Introduction*:

"Differences in ocean bathymetry and land-sea mask between present-day conditions and 21 ka BP calculated from the ICE-6G_C ice-sheets reconstructions (Argus et al., 2014; Peltier et al., 2015) are plotted in Fig. 1. In general, the topography of the NH ice sheets does not vary substantially between different reconstructions whereas uncertainties show larger for Antarctica (Abe-Ouchi et al., 2015). Values up to 125 meters in ocean depth variations (Fig. 1a) are estimated, representing deepening of the ocean with time. The largest changes in the oceanic boundaries occurred in the northern hemisphere where the extensive areas covered by ice sheets during the LGM were flooded due to the ice melting (blue areas in Fig. 1b). It is important, therefore, to consider these changes when attempting to simulate the last deglaciation, for example by including a varying ocean surface area and volume."

The following citation is added to *References*:

"Abe-Ouchi, A., Saito, F., Kageyama, M., Braconnot, P., Harrison, S. P., Lambeck, K., Otto-Bliesner, B. L., Peltier, W. R., Tarasov, L., Peterschmitt, J.-Y., and Takahashi, K.: Ice-sheet configuration in the

CMIP5/PMIP3 Last Glacial Maximum experiments, Geosci. Model Dev., 8, 3621-3637, https://doi.org/10.5194/gmd-8-3621-2015, 2015."

I3 The authors should also mention that there have been already many successful publications on glacial-interglacial simulations cycles from EMIC (A. Ganopolski et al., Nature 529, pages 200–203 (2016)) and from GCM (A. Abe-Ouchi et al., Nature 500, (2013)190. Moreover, the authors should better emphasize what in this context would be the added value of accounting for sea level rise.

We add your suggestions to *1 Introduction*:

"Moreover, some research was carried out by using comprehensive climate and ice-sheet models (Abe-Ouchi et al, 2013) or climate models interactively coupled with a dynamic ice-sheet model for studying the last glacial-interglacial cycles (Bonelli et al., 2009; Heinemann et al., 2014; Ganopolski et al., 2016) and more specifically, the LGM (Ziemen et al., 2014). Still, in standard ESMs, land-sea mask is traditionally treated as fixed."

Later in the same paragraph:

"In the PMIP4 last deglaciation Core experiment design, the bathymetry and land-sea mask are considered boundary conditions that cannot evolve automatically in the model. Thus, the decision of how often to make manual updates was left to the expert (Ivanovic et al., 2016). However, by varying the bathymetry in small steps, the artificial signals produced by changes in the ocean configuration might be reduced yielding to a more realistic representation of the ocean circulation and its interaction with the other climate components during the last deglaciation.¨

The following citations are added to *References*:

"Abe-Ouchi, A., Saito, F., Kawamura, K., Raymo, M. E., Okuno, J. I., Takahashi, K., and Blatter, H.: Insolation-driven 100,000-year glacial cycles and hysteresis of ice-sheet volume, Nature, 500, 190-194, 2013.

Ganopolski, A., Winkelmann, R., Schellnhuber, H. J.: Critical insolation–CO2 relation for diagnosing past and future glacial inception. Nature, 529 (7585): 200 DOI:10.1038/nature16494, 2016."

I4 Superimposed to the vertical resolution of MPIO, an important issue to be discussed is the choice of the initial horizontal resolution.

We clarify this point in *1 Introduction*:

"In this paper, we use the MPIOM coarse resolution configuration with a curvilinear orthogonal grid (GR30) and two poles (Haak et al., 2003), over Greenland and Antarctica. We decide to use the coarse configuration to reduce the computational time, but the algorithms presented in this paper can easily be adapted to higher resolution grids. In the vertical, the model has 40 unevenly spaced levels, ranging from 15 meters near the surface to several hundred meters in the deep ocean."

Methodology:

M1 It is not clear for me that accounting for only two big lakes (Caspian and Black Sea), the authors can capture abrupt climate changes occurring during deglaciation, as for instance the 8.2 ka event. Moreover, the evolution of Caspian and Black Sea associated to Eurasian ice-sheet melting and large modification of the catchment is not easy to be reconstructed and depicted. The authors should clarify more explicitly what is the limit of their method. Specifically, they should explain how they cope with river run-off and changes in catchment areas during deglaciation for these two epicontinental seas. These issues have been shown to have drastic consequences on atmosphere and ocean circulation (see for example R. Alkama et al., GRL 33 (21) 2006, R. Alkama et al., 2008, Climate Dynamics. 30 and M. Wary et al, J. Quaternary Sci. 32, 908–922, 2017).

Actually, we are not accounting for lakes in order to capture the abrupt climate changes. Our algorithms are applied within the ocean model and therefore, they work on the ocean domain. In that sense, we are interested only in lakes that are connected to the ocean, that is the Black Sea. The Caspian Sea is, indeed, an exemption because it is not connected to the oceans. However, the Caspian Sea is much larger than the other minor lakes. We decided to include it to solve the SST there that might impact on the climate of Central Asia. Therefore, solving the SST of the Caspian Sea might be important for coupled climate models.

As it was mentioned before, we are presenting a tool that is independent of the uncertainties on the reconstructions. We are not solving the response of the topography to the ice-sheet melting and isostatic adjustments, but we are only prescribing them to our scripts. This is a problem to be accomplished by the ice sheet-solid earth models.

Finally, you are right that changes in catchment areas during deglaciation have drastic consequences on atmosphere and ocean circulation. But, this is a problem that is treated in the hydrologial discharge model (part of the land module) instead of the ocean as described by Riddick et al. (2018), as it is stated in the manuscript.

We clarify these issues at the beginning of section *2 Methodology*:

"Finally, we check for the presence of lakes in the GR30 bathymetry; the Caspian Sea and the Black Sea (under LGM condition, for example) are the only cases that are permitted. Because we are dealing with an ocean model, we are interested in lakes that are connected to the ocean, that is the Black Sea. However, we include the Caspian Sea in our calculations because of its potential impact on the climate of Central Asia. Solving the SST of the Caspian Sea, which is much larger than other minor lakes, might be important for coupled climate simulations. All other lakes need to be removed from the ocean domain either by connecting them to the open ocean or by considering them as land. The atmospheric model component allows accounting for lakes on land (only the thermal component). The adequate place to calculate water storage in lakes is the hydrological discharge model."

M2 At the end of paragraph 2.3, in the spatial smoothing procedure for SSH, there are also changes in water mass reorganization that lead to spatial variations of the sea level rise during melting as shown for instance in Mitrovica (Nature 2001,...). Is this effect accounted for? If not, the authors should clarify the possible impact of this process.

This issue is not specifically part of our algorithms but of the HR prescribed topography that enters to our scripts as input data. We assume that those effects are accounted for in the prescribed topography which should already contain the gravitational adjustments. In the fully coupled simulation that we are planning to run, the effects you mention are solved by the ice sheet-solid earth component.

Results:

R1 Whereas this paper is submitted for publication in GMD and devoted to technical and model development aspects, it is difficult to consider the validity of the process only analyzing the stability of the response without any information on the potential climate effect. Indeed, accounting for bathymetry with time steps of 10 years should allow the authors to capture the complex pattern of the deglaciation periods. Nevertheless, due to linear smoothing in time and space, it is unclear to me whether they really may capture abrupt events. This limitation should be discussed in more details.

As it was discussed before, the aim of this paper is to present a tool that allows for automatic changes of bathymetry and land-sea mask in the ocean component of MPI-ESM. We are not attempting here to analyse the climate response to a changing topography. The way in which the inclusion of this tool affects on the deglaciation will be studied in the future. The transient simulation exposed in section 3 has the purpose of testing the algorithms in a long-term run. By testing the algorithm we mean the evaluation of the tool in terms of model stability and conservation of water properties as it is stated at the beginning of section *3 Transient simulation*:

“This section has the aim of testing the above-described tool in a long-term run with MPI-ESM. The purpose is not to analyse the climate response to a changing bathymetry and land-sea mask, this will be discussed in a consecutive paper. The aim of this experiment is evaluating the performance of the tool in terms of model stability and conservation of mass and tracers. This is a necessary step towards a fully coupled simulation.”

Due to the changes in the model domain, the fields of SSH and tracers from the restart file are modified. It is therefore important to maintain the same amount of water and tracers inside the system. The choice of 10-years is not crucial for such an evaluation. Knowing that the algorithms guarantee the conservation of water properties, the tool can be applied more often if necessary. Beside this, we are discussing the abrupt flooding of the Hudson Bay in our algorithm.

R2 Superimposed to ice sheet melting, a major component of the SLR is the ocean thermal expansion during deglaciation. Therefore it should produce a difference between SLR and cumulative fresh water input. In fig. 8, I suggest to plot, superimposed to the black and red curves, the component relative to the changes of the ocean volume associated with the thermal expansion during deglaciation.

Thermal expansion is, indeed, not included in the model. MPIOM, as many other ocean models, uses the incompressibility assumption. As a consequence of this, tracers are conservative relative to volume and not relative to mass and the model conserves volume and not mass. Including the thermal expansion term in an ocean model as MPIOM is not consistent with the model physics because it would imply to give up the incompressibility assumption. In any case, the relative effect of thermal expansion on SSH is small compared with the signal due to the freshwater input. We include it in the model description in section *1 Introduction*:

“MPIOM is a free-surface ocean general circulation model with the hydrostatic and Boussinesq approximations and incompressibility assumption.”

We also include a sentence in section *3 Transient simulation*:

“The difference between both time series was divided by the ocean area in order to obtain the errors in mean sea level (Fig. 8b). They are of the order of 1×10^{-3} cm and within the computational accuracy. Therefore, the changes in ocean volume match the freshwater input indicating that water is being conserved. Note that MPIOM uses the incompressibility equations and therefore, the contribution of the thermal expansion on SSH is not being considered here. The year when the Black Sea is connected to the Mediterranean Sea, around 10.3 ka BP, is an exception for the conservation.”

Finally, we do not see the advantage of showing a plot of the thermal expansion in the paper. In addition, changes in volume and mean salinity make it rather tedious to calculate. Traditionally, in ocean models with the incompressibility assumption, thermal expansion can be calculated by using the volume integral over the density, thus giving a mass. The difference of the calculated masses between 2 time slices, can be converted into a volume change (in a real ocean we can assume that volume changes and mass doesn't). Then, dividing the change in ocean volume by the ocean area yield the sea level change by thermal expansion (as was done e.g. in Mikolajewicz et al. 1990). However, here we have the problem that the volume within the ocean model is no longer constant, but the changes are substantial. Therefore, we would have to feed in further assumptions how the additional water entering the ocean should affect the reference mass used for calculation of thermal expansion. As changes in volume are quite large (more than 100 meters in sea level during our simulation) and much larger than the expected value of the thermal expansion (probably a few meters), we would expect quite some uncertainty in the estimation of thermal expansion because of the assumption how to deal with the extra water for the calculation of the reference value. This issue would require a discussion about circulation and climate changes in our deglacial simulation, which is not the topic of this paper.

R3 is the model accounting for a possible ice shelf at the beginning of the deglaciation in the northern hemisphere?

No, MPIOM does not include ice shelves. Therefore, the transient simulation we present does not account for ice shelves in any moment of the run. We include it in the model description in section *1 Introduction*:

“MPIOM includes an embedded dynamic/thermodynamic sea-ice model (Notz et al., 2013) with a viscous-plastic rheology following Hibler (1979). Sea-ice is swimming in the water. Ice shelves are not included. In this paper, we use the MPIOM coarse resolution configuration with a curvilinear orthogonal grid (GR30) and two poles (Haak et al., 2003), over Greenland and Antarctica.”

Remarks:

RM1 As the impact on climate due to change in bathymetry is not described in this paper, we can still have in mind many questions concerning the limits of this tool, when applied to non linear processes as those occurring during deglaciation. Indeed, the deglaciation is far to be a linear process. Major abrupt events (MWP and HE) occurred that are associated with large increase of fresh water inputs. It would be interesting that the authors discuss these potential limitations.

This issue, as already explained before, is not a limitation of our tool but of the prescribed topography and freshwater forcing. We are not solving the topography response to abrupt events, actually. The changes in topography associated with the large increase of freshwater inputs should be included in the forcing we are prescribing and our algorithms do not depend on it. This would be a task for either the reconstructions or the ice-sheet model, and not for the ocean model. In the fully coupled model under development this will be a very interesting aspect and the model should be suitable to cope with it.

Final comment:
This study is interesting and novel. Moreover, it corresponds to an awaited development to better simulate the last transient deglaciation. Therefore when the authors will have answered the questions raised above, the manuscript will be worth to be published.