

## ***Interactive comment on “A Last Glacial Maximum world-ocean simulation at eddy-permitting resolution – Part 1: Experimental design and basic evaluation” by M. Ballarotta et al.***

### **Anonymous Referee #2**

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In this paper the authors describe the basic design of an ocean simulation in the eddy-permitting regime of the Last Glacial Maximum. They show some basic comparisons with a present-day simulation of the same model, deferring a comparison with paleo-oceanographic data to Part 2 of this series.

Overall, I like the approach taken by the authors, and appreciate the efforts that they have made for this study. However, I am not sure if the separation of the manuscript in 2 parts does this study justice. In particular, the comparison in the current manuscript between the eddy-permitting LGM and PD simulations does not contribute at all to the stated aim of this study, “. . .to investigate the improvements due to taking into account

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these higher spatial scales”. A clean comparison between the 1 and 0.25 degree simulations of the LGM, and an interpretation of the differences in terms of explicitly resolving finer scales would be a more valuable contribution than the comparison presented here. I would therefore recommend to the authors to extend the current comparison by including the low-resolution simulations as well.

Minor comments:

p.301, section 2.1: some more description of the mixing parameterizations (Gent-McWilliams, KPP, etc) might be in order here, especially since the aim is to study the impact of higher resolution on LGM simulations.

p.301: It is essential to say something about the equilibration of the run. Is there still a significant model drift after 150 yrs?

p.302, l.19: provide these Levitus references.

p.302, l.21: Is the difference in sea ice volume between PD and LGM really large enough to significantly increase the mean salinity of the global ocean?

p.302, ll.22-26: A bit more detail about the calculation of the heat and freshwater fluxes might be useful here. Also, what is used for runoff during the LGM? Is ice sheet melt included (per p.306, l.3)?

p.302, l.28: Please provide reference here (Brandefelt and Otto-Bliesner?)

p.303, ll.23-24: It is disappointing that surface relaxation is applied for the PD run. This disqualifies the argument of ll.7-9 that “the main goal of our experiment is to investigate the impact. . .”. It seems to me that such a fair comparison is no longer possible.

p.303, l.26: Figure is called out of order.

p.304: This section seems to describe model results, while they really describe characteristics of the external forcing. This might be emphasized a little better.

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p.304, l.15: "Therefore. . ." Logic is not clear.

p.306, l.7; p.309, l.12, ll.22-27: I still don't understand this (it may be my ignorance), since in a steady seasonal cycle the amount of freshwater that freezes up during winter is released in summer, so in the net this should not affect surface salinity, right? Unless significant sea ice export takes place. But you would think that the net salt content is conserved.

p.306, l.24: Do you mean Antarctic Intermediate Water (AAIW)?

p.307 and beyond: The Brandefelt and Otto-Bliesner reference should be called in-line.

p.307, ll.6-19: It may be clearer to discuss NH and SH sea ice separately. It is a bit confusing (and meaningless) to compare sea ice extents in both regions during boreal winter, for instance. The entire description of the sea ice is poorly organized.

p.307, l.21: What is the second equilibrated stage?

p.308, l.7: Title should be "zonally-averaged structure". The zonal structure is exactly what you average out. . .

p.308, l.16: ". . . a glacial state. . .". A "permanently ice-covered state" might be clearer.

p.310: A plot of the global MOC might be of interest as well.

p.310, l.25: Is the causality obvious here?

p.311, ll.16-18: Causality?

p.312, ll.9-11: I don't remember these stages being discussed in the model approach section.

p.319, and beyond: Figures are quite small, you might be able to increase their size if stacked vertically.

p.323: Scale on the ordinate is a bit awkward.

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p.326, Fig. 8: Are we missing negative contours here? Where is the LGM AABW cell?

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Interactive comment on Clim. Past Discuss., 9, 297, 2013.

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