

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 #1

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The manuscript by Ballarotta et al. describes results from an LGM ocean simulation using an eddy-permitting (0.25 degree resolution) global OGCM. As defined in the introduction, the goal of the study is "to evaluate whether the eddy-permitting oceanic simulations improve the results with regard to coarse-resolution models and paleo-proxy reconstructions" (page 300, lines 20-22). The latter goal (model-data comparison) seems to be the topic of a second manuscript (Part II), so I expected that this manuscript (Part I) would provide insight into the potential of eddy-permitting OGCMs in improving simulations of the glacial ocean. It turned out, however, that this study neither provides new insight into the state of the glacial ocean nor does it evaluate the advantages of high-resolution eddy-permitting ocean modelling against coarse-resolution

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simulations. In the end, the authors conclude "This comparison shows that the simulated glacial ocean is globally colder and has a higher salinity than in the present-day experiment" (page 311, lines 10-11). But do we really need an eddy-permitting ocean model to come to this conclusion?

Given the little scientific progress of this study, I do not recommend publication of the manuscript in its present form. However, there is a lot of potential in the use of the model output when focussing on specific issues that can only be addressed by an eddy-permitting model. There are a lot of unresolved questions surrounding the state of the eddying glacial ocean, e.g. regarding Southern Ocean/ACC dynamics, Agulhas leakage, transports through "bottlenecks" like the Indonesian throughflow, etc. It is unclear to me, why the authors did not address such important points to provide substantial scientific progress using their sophisticated and computationally expensive OGCM simulation.

Specific points:

1) The comparison of the LGM simulation with the present-day (PD) simulation is flawed, since the two OGCM runs were driven by different methods. The PD run is forced by observational data (including surface restoring!), whereas the LGM run is forced by raw model output from a CCSM3 coupled LGM simulation. This approach leads to wrong conclusions, like the LGM warming of coastal upwelling areas which can be attributed to CCSM3's well-known warm bias over these regions (incl. biases in radiative forcing associated with stratus clouds) but has no scientific meaning. Also the statement that "the mid-latitude winds are stronger and shifted equatorwards during the glacial period" (page 304, lines 24-25) is based on the comparison between modern observations and LGM model results. However, CCSM3 is well known to have an equatorward bias of the southern westerlies, while the same model does not simulate an additional equatorward shift of the westerlies for the LGM (Rojas et al., 2009, Clim. Dyn.). A better approach would be to either force both simulations with CCSM3 model output (present-day CCSM3 control runs already exist) or to use simulated LGM

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anomalies (in combination with the modern observations) to force the glacial OGCM run. The approach chosen in this study, however, is not acceptable.

2) The integration time of 150 years is by far too short to draw any firm conclusions on deep ocean T/S, the MOC or the meridional heat fluxes. After 150 years, what we see in the deep ocean is strongly determined by the initial state which is taken from the coupled low-resolution CCSM3 simulation. It is therefore not surprising that "the results reported here are consistent with those from the source CCSM3 quasi-equilibrated simulation" (page 311, lines 19-20). For a reasonable study of deep ocean quantities, overturning and heat transports, a much longer integration is needed. Alternatively, the authors could remove those points from the manuscript which are related to the deep ocean when focussing on eddy-related upper-ocean issues (see above).

3) In order to demonstrate improvement of the glacial ocean simulation by using an eddy-permitting model, the authors should compare their high-resolution model results with an analog simulation using a non-eddy-permitting OGCM forced with identical glacial boundary conditions. A brief look at Part II revealed that such a simulation already exists (NEMO-ORCA1) and it's hard to understand why the authors didn't make use of it in Part I of the study.

4) While the atmospheric forcing fields are listed in Table 1, it is unclear how evaporation was treated. Was it prescribed or calculated by a bulk formula? If the latter approach has been chosen, the global P-E+R would probably not be zero and a drift in global salinity would occur. This might explain why the global LGM salinity is too high (sea-ice alone cannot satisfactorily explain the high salinity).

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