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Climate of the Past Discussions

Interactive comment on "A Last Glacial Maximum World-Ocean simulation at eddy-permitting resolution – Part 2: Confronting the paleo-proxy data" *by* M. Ballarotta et al.

Anonymous Referee #2

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REVIEW OF "A Last Glacial Maximum world ocean simulation at eddy-permitting resolution – Part 2: Confronting the paleo-proxy data" by Ballarotta et al., submitted to Climate of the Past Discussions

The authors compare the sea surface temperatures (SSTs) and the sea-ice extent simulated in the world oceans at the Last Glacial Maximum (LGM) in three different model experiments with reconstructions produced from deep-sea sediments. A first experiment has a horizontal resolution of about 0.25 deg x 0.25 deg and is presented as "eddy-permitting". The other two experiments have a coarser resolution of about 1o x 1o. It is reported that the SSTs in the three experiments compare favourably with

SST reconstructions from the MARGO project between 25S – 50S and between 25N – 50N. Larger discrepancies between model results and reconstructions are found in the tropical regions and at higher latitudes. Furthermore, the "eddy-permitting" experiment produces results that are generally not in better agreement with the paleoceanographic estimates than those of the coarser resolution experiments. Finally, notable discrepancies with the sea-ice extent reconstructed from deep-sea sediments in the northern North Atlantic and in the Southern Ocean are found for the three experiments.

I think that the results from this manuscript should constitute a significant step toward the interpretation of paleoceanographic reconstructions for the LGM on the basis of numerical models of ocean circulation. As summarized in the manuscript, previous studies revealed significant differences between the SST distributions at the LGM simulated by climate models and those reconstructed from sediment cores. The results from this manuscript provide a test to the notion that increased horizontal resolution of the models, which is expected to lead to more accurate simulations of ocean circulation, could improve the consistency with LGM reconstructions. As cautiously stated in the Conclusions section, the present test should not be viewed as definitive and future studies are needed. Nevertheless, the quantitative and careful comparisons reported in this manuscript should provide a useful reference for future studies on the ability of high resolution models to replicate paleoceanographic observations, not only for the LGM but also for other time intervals of the geologic past.

Whereas I am generally enthusiastic about this study, I also think that several comments should be addressed before this manuscript be considered acceptable for publication. Major comments are listed below, followed by minor ones. I hope these will help the authors to improve both the content and the presentation of their interesting manuscript.

MAJOR COMMENTS

As it is, the manuscript is a relatively short and rather dry description of the ability of

different model experiments to reproduce estimates of SST and sea ice cover at the LGM. My major comment concerns therefore the discussion of the results of this study, which I think could be fruitfully extended along different directions. Several of the points below relate to the spatial distribution of the paleoceanographic data and are therefore somewhat connected.

1) A natural question is whether the ocean model used with "eddy-permitting" resolution to simulate LGM conditions (NEMO model) produces accurate results when applied with similar resolution to simulate the circulation and properties in the modern ocean, in particular SST and sea ice cover. In other words, is there evidence that, with the NEMO model, a resolution of about 0.25 deg x 0.25 deg leads to better consistency with modern data than a resolution of about 1 deg x 1 deg? If this is the case, then the present study would demonstrate that the greater ability of a model with "eddy-permitting" resolution to replicate oceanic observations may not necessarily apply to other climate intervals. If this is not the case, then it would demonstrate that use of "eddy-permitting" resolution with NEMO does not improve the simulation of SST distribution, not only for the modern state, but also for the LGM. In my views each of these results would be significant, and I would encourage the authors to elaborate the discussion along these lines.

2) A second question is the extent to which the geographic distribution of the LGM data (very scarce by modern standards) conditions the major result of this study. In other words, could this distribution contribute to the result that the experiment with about 0.25 deg x 0.25 deg resolution does not generally lead to better agreement with these data than the two experiments with about 1o x 1o resolution? This question could in principle be addressed by comparing (i) the modern SST simulated by NEMO (at both resolutions) at the MARGO sediment core locations with (ii) the modern SST at the same locations as documented in a recent hydrographic climatology (in this comparison, annual mean, boreal winter, and boreal summer values could be individually considered). If the experiment with higher resolution better replicates the modern SST

globally but does not better replicate the modern SST at the sediment core locations, then one would be tempted to conclude that the major result of this study may be largely determined by the core locations. This result would also provide further impetus for the collection of additional paleoceanographic data.

3) It is stated in section 1 of the manuscript that, according to the IPCC 4 report, models with high resolution would be needed to properly simulate coastal upwellings and eastern boundary currents, and hence to obtain better agreement with the MARGO SSTs in these regions. The question emerges, therefore, as to whether the experiment with about 0.25 deg x 0.25 deg resolution which is considered in this study produces results that better accord with paleoceanographic reconstructions in these regions than those of the coarser resolution experiments. In the manuscript, emphasis is placed on western boundary regions (Figs. 5-7), supposedly because relatively large differences between results from "eddy-permitting" and non-"eddy-permitting" experiments are expected near western boundaries. However, I think that the results from this study could also be used to explore whether higher resolution could reduce discrepancies with MARGO SSTs in coastal upwellings and along eastern boundaries, as suggested in the IPCC report.

4) Why is the difference with the MARGO SSTs north of 50N so large for the three model experiments compared to the differences in the other latitude bands (Table 2)? Could the authors point to one or several dynamical processes that are not or poorly represented in these experiments and yet which could influence significantly the MARGO SSTs at high latitudes? Alternately, could the large data-model SST differences north of 50N be due, at least partly, to systematic errors in the MARGO SSTs in this region, in particular in the northern North Atlantic where important differences exist between SST estimates obtained from different sediment indicators (Waelbroeck et al., 2009)? Of course, the authors do not have to take a position on this delicate issue, but they could at least mention whether or not the above possibility is consistent with discussions, in the paleoceanographic literature, on the systematic errors in LGM

SSTs at high northern latitudes (cf. in particular a series of papers by de Vernal et al.).

5) Please consider that readers may not all be familiar with the skill score S and with Taylor diagrams. A brief discussion of the pros and cons of S compared to more conventional measures of the association between two random variables, such as the Pearson product-moment correlation coefficient R, would be welcome. Moreover, the Taylor diagrams depicted on Figs. 1-4 are incompletely described: they should also include (1) labels along the vertical axes, with physical units, (2) an extended label along the horizontal axes, with physical units (e.g., "standard deviation in reconstructed SST [deg C]"), and (3) an explanation in the caption of the two oblique black solid lines, of the black dashed line, and of the grey lines with the physical units used for the values being displayed (I had to infer from the text that grey lines show RMSE in SSTs expressed in deg C). Please also clarify that the skill scores S per se are not displayed on the Taylor diagrams, as I was led to conclude.

6) Figs. 5-7 do not appear to be particularly informative: the SST differences between the high and low resolution experiments can be noticed but are generally not conspicuous, and model-data differences are very difficult to evaluate based on these figures. I would recommend that, for each of the three western boundary regions, the SST maps be extended with another figure showing the SST difference between the two experiments. I would also recommend that for each of these regions a scatter plot of simulated SST versus reconstructed SST be displayed. For each scatter plot, the correlation coefficient R should be provided together with its corresponding p-value and sample size n. Of course, these results should be discussed in the text, at least briefly.

MINOR COMMENTS

P. 330, line 4: write "... improving the agreement with the available ..."

P. 330, lines 15-16, "... probably due to the choice of the model equilibrium": it is unclear why this choice is to be blamed from the abstract. Please drop the above part of the text or elaborate shortly.

P. 331, line 12: write "... There it is mentioned ..."

P. 331, line 21: write "... models according to the paleo-proxy reconstructions."

P. 331, line 23: write "... and one eddy-permitting forced ocean simulation) ..."

P. 332, line 13: write "... tropical regions. From these data, reconstructions of ... JAS) have been produced."

P. 332, line 24, "source point": do you mean "sediment core location"?

P. 332, lines 25-26, and P. 333 lines 1-2: write "... The performance of the models is evaluated quantitatively by their skill score S and illustrated using Taylor diagrams (Taylor, 2001). The skill score S is a measure of the correlation between the simulated and reconstructed SSTs, given their respective variability:" Then write equation (1).

P. 333, line 5: write "... where R is the Pearson, product-moment correlation coefficient (please check this) between simulated and reconstructed SSTs, ... and R0 = 1 is the maximum (positive) correlation attainable."

P. 333, lines 8 - 10: write "... outputs. A skill score S = 1 means that ... while a score S = 0 ...".

P. 333, line 15: write "3.1 Sea surface temperature"

P. 333, line 21: write " ... in the North Atlantic from the MARGO reconstruction (Waelbroeck et al., 2009) ..."

P. 334, lines 3-4, "... They capture the variability of the SSTs, but ...": what is the quantitative basis for this statement (\hat{sigma}_f close to 1?) and where is the contradiction implied by "but"?

P. 334, lines 13-14: write "...the correlation coefficient R is near 0.6, ... above 3 oC, and the skill scores S above 0.5".

P. 334, line 15: write "Finally, between 25oS and 50oS, ..."

P. 334, line 19: write “The above analysis shows that . . .”

P. 334, last paragraph: The focus on these three western boundary regions should be rationalized in the text. I suppose the focus is motivated by the fact that these are regions where mesoscale activity is relatively high and where “eddy-permitting” and non-“eddy-permitting” experiments could show particularly large differences.

P. 334, lines 23-25, “The conclusions are drawn . . . are not shown)”: This sentence should be moved to the end of the paragraph, i.e., after these conclusions are stated.

P. 335, lines 1-3: I do not see why meandering of the ACC should result in high meridional SST gradients. This part of the text could perhaps be elaborated or rephrased. “. . . better captured . . .: do you mean according to the MARGO data?

P. 335, line 21: write “supplementary material”

P. 335, line 22: write “. . . in accordance with the reconstruction of . . .”

P. 335, lines 25-27: The sentence is ambiguous: how seasonal changes could appear in a fully ice covered area? Is the seasonality being referred to the simulated seasonality or the observed seasonality? Please rephrase and clarify. Please change “Supplement” to “supplementary material”.

P. 336, line 1: write “. . . isoline of . . .”

P. 336, line 14: write “. . . All three models perform well . . .”. The agreement is not that spectacular for the band 25N-50N (see P. 4, line 4: a R value of 0.7 would imply that 1-(0.7**2) = 51% of the variance in reconstructed SSTs remain unexplained).

P. 336, line 16: write “Moreover, it is obvious . . .”

P. 336, lines 22-23: write “where mesoscale dynamics may play . . .”

P. 336, lines 26-27: write “. . . showed increased sea-ice fraction . . .”. Also please clarify what is a “second equilibrated stage”.

P. 337, line 4, “. . . the choice of model equilibrium”: Please clarify (or remind to the reader) what “model equilibrium” is. Is this a spin-up solution that has reached equilibrium with boundary conditions and from which the LGM experiments are performed? Please also clarify why this choice may matter in the present context.

P. 337, line 8: write “In summary, this investigation . . .”

P. 337, line 13, “productive”: do you mean “computationally efficient”. Please clarify.

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