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Interactive comment on "Impact of Southern Ocean surface conditions on deep ocean circulation at the LGM: a model analysis" *by* Fanny Lhardy et al.

Anonymous Referee #2

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In this paper, Lhardy et al. use an intermediate-complexity model to evaluate the response of the simulated glacial ocean circulation to Southern Ocean surface ocean temperature and sea-ice conditions. They achieve differences in these surface conditions by running simulations with various options for glacial climate boundary conditions from the Paleo Model Intercomparison Project (PMIP), and by simulations where they change wind (i.e. sea-ice export) conditions, formation of salty brines, and freshwater input in the Southern Ocean. They find that the sensitivity tests with winds, brines, and freshwater have more potential to influence the simulated surface properties, and particularly the ocean overturning circulation and distribution of water masses, compared to the choice of boundary conditions. This highlights the importance of informed

choices in model parameterizations of processes. In particular, it further clarifies the effect of deep-water formation and convection processes for achieving a realistic representation of the glacial deep ocean and its water masses. This is of importance for our ability to understand, and simulate, glacial ocean storage of CO_2, and to improve our models. As this category of models is commonly used for paleo simulations, this study is particularly educational for modelling groups using models of similar resolution and complexity, especially those participating in PMIP.

The experimental design is overall sound, though adding a PI state that uses the brines parameterization would be beneficial, to test the effect of 'a better representation of deep-water formation' in the modern ocean. As the parameterization does not change the amount of sea ice, only its effect on water mass properties and circulation, the choice of 0.8 as the scaling should not have to change between climate states. The amount of brines, and thus their influence on the ocean properties, should decrease in a warmer climate due to the reduction in sea-ice. It would also be useful to test the brines parameterization together with the PMIP2 boundary conditions, to strengthen some of the conclusions about the role of boundary conditions, but I leave it up to the authors to decide if this is feasible.

Overall, the paper is well written, with a generally clear structure and informative figures. However, some clarifications, motivations of choices, and rephrasings are advisable prior to publication. I therefore suggest the following revisions.

Abstract

P. 1, L. 5 and L. 12: 'proxy data': please specify 'proxy records of . . .'

P. 1, L. 7: 'with respect to data' – data in this context is very unspecific. I would suggest 'paleoproxy data'

P. 1, L. 9: I suggest replacing the vague descriptions 'different modelling choices and/or boundary conditions' for something more specific, e.g. by rephrasing to '[. . .]

different boundary conditions for climate and ice sheets, and choices for sea-ice export, formation of salty brines, and freshwater input [. . .]'

Introduction

P. 2, L. 25: Consider adding a reference to Galbraith and de Lavergne (2019), see also my comment for the Discussion section. Galbraith, E., de Lavergne, C. (2019). Response of a comprehensive climate model to a broad range of external forcings: relevance for deep ocean ventilation and the development of late Cenozoic ice ages. Climate Dynamics, 52(1-2), 653-679.

P. 2, L. 53: 'three sensitivity tests' - Please mention what these are e.g. 'three sensitivity tests of Southern Ocean conditions for sea-ice export, formation of brines, and freshwater input.' (see also suggestion above for the Abstract)

P. 2, L. 36-38: Model representation of Southern Ocean deep-water formation is rather central for the conclusions of this paper. I would suggest clarifying the reasons for why deep water is formed by the wrong process in most models.

Methods

P. 3, L. 67: There is no mention of how this sea-ice component differs from those in other PMIP models, and how the model representation of sea ice potentially impacts the results. As a non-expert on sea-ice modules, I would have liked to see a sentence or two that discusses this.

P. 3. Section 2.2: I feel that it might be clearer if this section is amended to be 'The PMIP boundary conditions and their implementation" and thus to include descriptions of the PMIP2 boundary conditions and how they differ from PMIP4 (see specific comment for L. 73-76 below for an example)

P. 3, L. 73-76: It is clearly stated (much) later in the paper (P. 10, L. 318) that GLAC-

1D and ICE-6G-C are the main recommendations of Kageyama et al. (2017) among multiple options. From the current phrasing here on P. 3, it is not clear why the PMIP3-option is excluded from the present study. It would be clearer if the phrasing were more similar to that on P. 10. In addition, there is no introduction to the ICE-5G option (presented on P. 4, L. 113-115), as it is part of the PMIP2 boundary conditions (see previous comment).

P. 4, Section 2.3: This section is very technical and not necessarily relevant to the average reader. I suggest moving it to an appendix, or include it with the rest of the description of the bathymetry generation method in the SI.

P. 4, Section 2.4: Please add a PI state that uses the brines parameterization, to test the effect of 'a better representation of deep-water formation' in the modern ocean.

P. 5, L. 122: Please specify why P4-I is selected as the reference LGM state over P4-G (see also comment for P. 7, L. 195)

P. 5, L. 125: 'a chosen fraction (here 0.8)' – Please specify how this choice is made and how a different choice might impact the results (see also comment for Discussion P. 11, L. 338-342)

P. 5, lines 136-139. 1) I find this paragraph to be phrased in a confusing way. I suggest separating the descriptions of LGM and PI data. 2) The MARGO Project Members reconstruct the LGM sea-surface temperatures. Please explain briefly why there is a lack of data for the Southern Ocean in austral winter. On P. 8, L. 219, you say that it is due to an extensive sea-ice cover, but coring that is done in the summer will still provide sediments from past winters, so it should be clarified why the winter sea-ice cover is a problem.

P. 5, line 137. You say here that you compare the PI simulation to World Ocean Atlas data. Please specify which version of the WOA data that is used, and if you are indeed using the WOA98, explain why you are not using the most recent version. I suspect it is because the MARGO Project Members are using WOA98, but if so, this needs to be stated clearly. If you are using a more recent version of WOA, please cite the appropriate publications for each variable. Also, according to the figures, the PI simulation is compared to MARGO data (see comment for e.g. Fig. 4)

P. 6, L. 160: Please mention what causes this notable difference in surface area.

P. 6, L. 161-162: 'For the indicative error in the surface extent computed, we kept the respective values of 10

Results

P. 6, L. 178, Section 3: 'Methods'- Should be 'Results'

P. 6, L. 180: 'Cold P2 is too cold' – but it is well comparable to the more recent estimate by Tierney et al. (2020) mentioned later in the paragraph. I feel like this should be mentioned in the discussion of that paper and the fact that iLOVECLIM generally simulates more modest SAT anomalies (P. 7, L. 187-192), as this experiment design is an example of when the model actually achieves a more extreme anomaly.

P. 6-9, Section 3.2-3.3: The model-data analysis in these two sections could gain from a comparison of model skill (M) as described by Watterson (1996). This allows an evaluation of overall model-data agreement (patterns and point-to-point agreement), globally as well as on a basin level, and easier comparison between ensemble members and time periods. It would quantify statements such as those made on P. 11, L. 331-333. Watterson, I. G. (1996). NonâĂŘdimensional measures of climate model performance. International Journal of Climatology: A Journal of the Royal Meteorological Society, 16(4), 379-391.

P. 7, L. 195: 'the reference LGM simulation P4-I' – it is never mentioned in the Methods why this simulation is chosen as the reference over P4-G. This should be clarified. Is this choice likely to influence the results of the sensitivity tests, and if so, how?

P. 7, L. 197-201: In Fig. 2, Southern Ocean SST anomalies in P4-G show a similar pattern as P4-I brines (with the exception of the mid-to-eastern Indian Ocean sector of the Southern Ocean). If you have an idea for why this is, it could be interesting to mention.

P. 8, L. 237-239: Please be clear also on how sea ice area is defined.

P. 8, L. 241: Minimum and maximum sea-ice extent - Is data available to use this method to compute corresponding numbers for the PI/modern day, to evaluate how these numbers compare to other estimates (e.g. Parkinson and Cavalieri, 2012, that is used here)? This could clarify whether the method gives rise to any systematic bias, and how well the model agrees with the different sea-ice extent estimates for the different time periods.

P.8, L. 251: The background for the previous comment is the statement here that 'the sea-ice extent of most simulations falls close to the reconstructed winter sea-ice extent'. To me, this fact seems to suggest that your maximum reconstructed extent might be an underestimation, given that most of the simulations are on the lowest end of the reference interval for glacial cooling by Annan and Hargreaves (2013).

P. 10, L. 294: 'paleotracer data' – It would be helpful to remind the reader of the relevant references.

P. 10, L. 296-298: Do you have an explanation for why the enhancement of the bottom convection cell occurs as a response to the change in ice sheet boundary conditions?

P. 10, L. 298: 'the simulation associated with GLAC-1D (compared to ICE-6G-C)' – Here, it would be helpful to specify which one of P4-G and P4-I uses which ice sheets

P. 10, L. 311-312: 'showing that simulations with a colder Southern Ocean tend to be associated with a stronger Southern Ocean cell, a weaker bottom cell and a more intense NADW cell' - Does stronger/weaker in this sense also refer to the volume occupied by the cell (i.e. depth of the water mass boundary between the bottom cell and the NADW cell)? I get this impression when I read about the results for the 'P4-I brines' simulation. If so, it should be pointed out that proxy records conflict with this result, as they show a colder Southern Ocean simultaneously with a shallower NADW cell and a more expanded bottom cell. This is very well summarized in the Conclusions section.

Discussion

P. 10, Section 4.1: Important aspects of the effect of boundary conditions, modelling choices, and vertical mixing on LGM simulations are all discussed in Galbraith and de Lavergne (2018). I suggest mentioning the findings of this publication somewhere in sections 4.1-4.3.

P. 11, L. 323-324, and 326-327: Based on the remark on P. 9, L. 282, that all the simulations show similar biases in seasonal and regional patterns, could you give examples of sensitivity tests that might show somewhat different biases, or do you think this is too much of a persistent characteristic of the model (if so, why)?

P. 11, L. 338-342: It would be advisable to mention the choice of the fraction 0.8 and how it would potentially affect the results if this was chosen differently (see also comment for Methods P. 5, L. 125)

P. 11, L. 343-344: 'However, we can argue that the open ocean convection in the Southern Ocean is actually hindering the simulation of a realistic water masses distribution.' – This should be shown to be true also for the PI simulation. If it is not, the authors need to argue for why it is reasonable to include it in the LGM when it is not necessary or an improvement to do so for the PI.

P. 11, L. 349-350: 'showed that few progresses have been made by some modelling groups with respect to that aspect.' - I do not quite understand this sentence. Do you want to say "a few modelling groups have made some (minor?) progress in this aspect", or that "few modelling groups have made any progress in this aspect", or maybe that "some modelling groups have made particularly little progress in this aspect"?

P. 13, L. 409-411: 'It would therefore seem that the correct simulation of convection processes is paramount, and far more important than the choices of boundary conditions, such as the ice-sheet reconstruction [. . .]' – The brines parameterization has not been tested with the PMIP2 boundary conditions, as far as I can tell. Hence, it is clear that it is more important than the choice of ice-sheet reconstruction, but I am not sure it is well founded to say that it is more important than the choice of boundary conditions in general.

Figures

P. 6, L. 153: Somewhat confusing that Fig. 6 is mentioned before Fig. 2.

General comment (e.g. figures 3, 4, 7, 8, S2, S3): How are the basins defined (longitudinal and latitudinal limits)? The latitudinal limit for the Southern Ocean seems to be different in different figures (see Fig. 7).

Figure 2: Please specify in the caption what the mean SST of this simulation is, as this is specified for all other simulations in the figure. Also, it would be advisable to add a few longitude and latitude grid ticks, at least in the bottom row and left column respectively, since you have drawn the grid lines.

Figure 4, Panels a-b: In the Methods section 2.5, you say that PI simulations are compared to WOA data, not MARGO data, Caption: Describe the thinner dashed lines surrounding the 1:1 line and how the SSTs are averaged in this figure.

Figure 6, General: I would suggest adding a few longitude and latitude grid ticks, Panel a: Why is the red line in panel a dashed, when no other lines in the figure are?, Caption, L. 1: Please mention how the sea-ice edge relates to the extent and area, Caption, L. 2: The part about the arbitrary index is a bit difficult to read. I suggest "[...] as an arbitrary index on a blue to white scale, where blue denotes no indication of sea ice in proxies, and white denotes agreement of several proxies on the presence of sea ice."

Figure 7: This figure seems to have a different limit for the Southern Ocean compared to other figures (see General comment). I found this colour scale not very gentle on the eyes. I had trouble looking at the figure because the stark contrast and particularly the bright cyan/mint made me feel dizzy/nauseous. Changing it is of course not a requirement for publication, just a suggestion.

Figure 8: In my opinion, it would be preferable to plot these using a standardized grid spacing for each column, as it would make it easier to compare the slopes for the different cells

Minor details (typos and similar)

P. 1, L. 8: 'inaccurate' – replace for 'inaccurately'

P. 1, L. 10: 'data-model' – 'model-data' seems to be the more commonly used term, and is also what you use later in the paper

P. 1, L. 15: 'water masses properties' – replace for 'water mass properties'. Note! This error occurs throughout the manuscript when water mass properties/distributions are mentioned (see e.g. P. 2, L. 24: 'water masses distribution' – should be 'water mass distribution', as on P. 1, L. 1)

P. 2, L. 29: 'shallowing' – replace for 'shoaling'

P. 11, L. 337: 'paleodata' – replace for 'paleoproxy'

P. 13, L. 399 and 400: 'sea ice and SST data', 'proxy data' – replace 'data' by 'reconstructions'

Supplementary information

P. 1, first sentence: 'remplaces. . .' – should be 'replaces'

P. 1, fourth bullet point: 'all the critical traits stay open' – 'traits' should be 'straits'

Fig. S1: I would suggest adding a few latitude and longitude grid ticks

Fig. S2: See comments for Fig. 4

References

Galbraith, E., de Lavergne, C. (2019). Response of a comprehensive climate model to a broad range of external forcings: relevance for deep ocean ventilation and the development of late Cenozoic ice ages. Climate Dynamics, 52(1-2), 653-679.

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