

Interactive comment on “Evaluating the Biological Pump Efficiency of the Last Glacial Maximum Ocean using $\delta^{13}\text{C}$ ” by Anne L. Morée et al.

Anonymous Referee #2

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General comments

The authors use an ocean-sea-ice model (NorESM-OC) that also includes biogeochemistry, $\delta^{13}\text{C}$ carbon isotopes and radiocarbon, to quantify the role of the efficiency of the LGM biological pump in obtain the best agreement between model simulations and proxy data. Their results indicate that the efficiency should be doubled to obtain the smallest model-proxy mismatch.

The model setup is novel, properly thought through, overall well-described, and can certainly be used to provide useful insight on long-standing questions about the role of ocean circulation and biogeochemistry in driving glacial-interglacial changes in ocean carbon storage. However, the structure and clarity of parts of the manuscript need to be substantially improved. Some additional simulations/sensitivity experiments may also

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need to be included, or at least their potential implications need to be better discussed and compared with the existing literature. A few highly relevant studies, and all very recent, are also missing in the references.

Setting up these simulations must have involved a substantial amount of work and this should be acknowledged and this framework will also be useful to investigate other research questions. This study makes valuable contributions to the topic and definitely deserves to be published, but several issues need to be addressed first, as described in the comments below.

Specific comments

Abstract, page 2, line 33: This statement is a bit too strong. The LGM is indeed a good test case for models and their evaluation and process-based understanding, but it can't be considered a necessary "requirement" for their reliability for future projections. I get the point and I agree, but this need to be rephrased.

Page 2, lines 10 and 23: Add references to Stein et al. (2020) and Marzocchi and Jansen (2019), especially since these studies both address directly the role of physical changes on glacial carbon storage, which is not really done in this manuscript. These also needs to be discussed further with the results – see later comments.

Methods

The simulations are integrated for a long period of time. Nonetheless, it would still be useful to show some of the LGM ocean state equilibrium/drift in the Supplement. Perhaps some timeseries of T and S and/or AMOC and Drake Passage transport, which are already mentioned in the text.

The Bern3D model part of the study needs to be introduced and explained, at least briefly, in this section – with proper reference to the Supplement for the rest of the details.

Results and discussion



This part of the manuscript needs some substantial restructuring and improvements. Parts of it are quite confusing, which takes away from the key findings and the main points that the authors are trying to get across.

Perhaps separate more clearly parts of the results that are more of a “model evaluation” and then for each of these have a subsection that discuss the reasons for the biases, to give some separation between results and discussion, especially where comparisons to observations and other studies are also discussed.

All of this is already in the text, but currently quite mixed up all together, making several parts a little hard to follow. I am not against having results and discussion together, but the structure needs to be clearer and easier to follow.

Section 3.1 is a little hard to follow without any figures...maybe add some in the Supplement?

Page 8

Discuss the radiocarbon ages also with respect to the results of Burke et al. (2015)

Line 31: add references to Jansen (2017) and Marzocchi and Jansen (2019) to support this statement on the importance of atmospheric temperatures for both LGM water masses and biogeochemistry, respectively.

Line 35: this needs to be discussed a little further (i.e. the underestimation of negative buoyancy fluxes) – for instance, compare Klockmann et al. (2018) – this is an example of where I think a separate Discussion section is missing. Alternatively, this could be picked up again in the conclusions as one of the potentially important biases.

The abyssal cell actually looks weaker at the LGM? (Figure S5) This also needs to be discussed, perhaps here.

Page 9

Line 5: add reference to Marzocchi and Jansen (2019) and Stein et al. (2020) where

the link to ocean carbon storage is actually tested.

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Section 3.2.2

Lines 10-26: This result (i.e. reduced LGM biological pump efficiency but lower pCO₂ concentrations) is not dissimilar from what discussed in Marzocchi and Jansen (2019), despite a very different model setup. So this is worth discussing further – perhaps think about this in the context of the carbon pump decomposition. This may mean that there is something we simply don't understand in this part of the mechanism. Can your study clarify this apparent discrepancy further? Can you make this clearer/highlight it better?

Page 10

Lines 10-19: this is another example where this is a discussion part, but it's somewhat "thrown" in the middle of some other text. So again this needs restructuring to make it easier for the reader to follow.

Line 25: here the reference is Marzocchi and Jansen (2019) rather than Jansen (2017).

Page 11

Lines 2-21: This part about the Bern3D ESM comes a bit out of the blue and I can't say that this is explained well enough and entirely clear. Make better reference to the Supplement and better introduce the setup in the Methods (as noted before), where the goals of this additional step need to be better clarified and introduced. Then it will come less out of the blue here in the results.

Page 12

Lines 12-29: this is again a somewhat self-standing discussion part that should perhaps be a subsection.

Here, and/or earlier, you should discuss the results of Odalen et al. (2019) Actually, would it be feasible to test their variable C/P ratio in your simulations?

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Also could you quantify the dependence of your results to your model initial state, as discussed in Odalen et al. (2018)? [this reference is already cited in the manuscript].

Conclusions

Add a reference to Rae et al. (2019) when discussing the importance of southern-sourced waters. This should probably also be discussed earlier in the results/discussion.

Technical corrections

Abstract

Line 17: ocean model state? Do you mean “equilibrium simulations”? Clarify. Ocean model state is not the best term to use here.

Line 23: “we explore the theoretical effects” doesn’t quite make sense. This could just be “we explore/test the effects”.

Line 29: again “theoretical” is not quite the right word. Just say “our approach”. Same in the rest of the manuscript (e.g. page 7, 10, 13). Perhaps do just call it “offline”.

Page 10, line 30: miss-match should be mismatch.

Everywhere: “Southern Source” should really be “southern-sourced”.

References

Burke, A., Stewart, A.L., Adkins, J.F., Ferrari, R., Jansen, M.F. and Thompson, A.F., 2015. The glacial mid-depth radiocarbon bulge and its implications for the overturning circulation. *Paleoceanography*, 30(7), pp.1021-1039.

Klockmann, M., Mikolajewicz, U. and Marotzke, J., 2016. The effect of greenhouse gas concentrations and ice sheets on the glacial AMOC in a coupled climate model. *Climate of the Past*, 12, pp.1829-1846.

Marzocchi, A. and Jansen, M.F., 2019. Global cooling linked to increased glacial car-



bon storage via changes in Antarctic sea ice. *Nature Geoscience*, 12(12), pp.1001.

Ödalen, M., Nycander, J., Ridgwell, A., Oliver, K.I., Peterson, C.D. and Nilsson, J., 2019. Variable C/P composition of organic production and its effect on ocean carbon storage in glacial model simulations. *Biogeosciences Discussions*, pp.1-33. (accepted) DOI: <https://doi.org/10.5194/bg-2019-149>

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