Dear Referee #1,

Thank you for your time to provide constructive feedback on our manuscript 'Evaluating the Biological Pump Efficiency of the Last Glacial Maximum Ocean using $\delta^{13}C$'. A response to each of the comments is provided below (in italic text). Specifically, we include a separate discussion section in a revised version of the manuscript. Here, the concerns of the reviewer on several discussion topics and missing references are addressed. Additionally, we improved the methods section by clarifying our approach to artificially enhance the efficiency of the biological pump (i.e., Sect. 2.4) and the use of the Bern3D model (new section 2.5).

Yours sincerely,

Anne Morée and co-authors

Major comments

(1) The authors artificially increased the efficiency of the carbon pump at the LGM for their discussion. However, the mechanism behind this increase is not discussed enough in the manuscript. In other words, why do the original NorESM-OC model fail to simulate the glacial increase of the efficiency of the carbon pump? This needs to be more seriously discussed in the revised manuscript.

Author response: We revised the manuscript as outlined below.

Changes in the manuscript: We addressed this comment in two ways. First, we revised section 2.4 to clarify how we artificially increased the efficiency of the biological pump (see also our reply to the comment on Sect 2.4). Secondly, we extended our discussion by including a new discussion section at the end of the paper (Sect. 4). Here, a more detailed and structured discussion on the lack of a simulated increase in the biological pump efficiency is given. Specifically, we discuss both physical (e.g., stratification, solubility pump, isolation and strength of abyssal overturning cell) and biogeochemical (e.g., export production, remineralization rate) mechanisms that could contribute to an increased efficiency of the biological pump - and whether NorESM-OC is able to capture these. We want to stress however that identification of the exact mechanisms is beyond the scope of our manuscript. Earth System Models are generally found to incompletely capture the biogeochemistry and strengthening of the biological pump for the LGM ocean, and identification of the exact processes that are missing in these models is a major challenge in modelling the LGM ocean (e.g., Galbraith and Skinner, 2020).

(2) Related to the above comment, the authors' conclusion "an approximate doubling of the global mean biological pump efficiency from 38% (PI) to 75% (LGM) reduces model-proxy biases the most" appears to depend highly on the reproducibility of their original LGM simulation. For

example, the strength of the AMOC in the LGM simulation appears to significantly affect this number: the weaker AMOC tends to increase the efficiency whereas the stronger AMOC tends to decrease it. I request the authors to discuss about the robustness of their conclusion.

Author response: Changes between preindustrial and LGM ocean circulation fields as simulated by ocean models generally fail to account for the 100-120 ppm drawdown in atmospheric pCO_2 (taken the outgassing by the land biosphere into account) when used in global ocean carbon cycle models (Heinze et al., 1991; Brovkin et al., 2007). The induced change is usually too small. Correspondingly, also the vertical d13C gradient (Deltadelta13C) is often not fully reproduced to its full extent. If we assume that the simulated circulation changes are realistic, this indicates that one needs to employ additional biogeochemical or ecological processes to enhance the atmospheric pCO_2 drawdown by the ocean and to enhance the biological pump. This can be done either by artificially enhancing the pump efficiency (which we explore in our theoretical framework) or by changing the nutrient cycling, e.g. by adjusting the stoichiometric ratio of elements N:P:C away from the Redfield ratio values or by adding nutrients to the ocean. Changing the pump efficiency is an easy way to implement the effect needed, leaving open the exact process that leads to this effect. A more sluggish ocean circulation, already leads to a partial increase in pump efficiency, because smaller amounts of nutrients are brought to the ocean surface and get exported in a more slowly overturning ocean, while the particle flux still operates with unchanged gravity acceleration. This leads to partial carbon and nutrient fractionation between upper and deep ocean, but not enough to explain the full pCO_2 reduction as observed in the atmosphere.

Changes in the manuscript: We included the above discussion in the new discussion section 4.

(3) I think that discussion about the effect on glacial changes in pCO2 is important. The authors stated that only 21 ppm lowering is found in their original LGM simulation. How much lowering of pCO2 is expected after the efficiency of the carbon pump is doubled in the LGM simulation?

Author response: The additional carbon inventory in the ocean corresponding to a doubling of the efficiency of the biological pump is quantified at ~1850 Gt C (p. 11, l.16). Where this additional carbon would have come from (the land, ocean sediments or atmosphere) is something we can not distinguish in our model setup or our offline exploration of the potential effects of changes in the efficiency of the biological pump. Nevertheless, the magnitude of this estimated change in marine DIC (i.e., ~1850 Gt C) allows for full (~80 ppm more than simulated, which is ~ 170 Gt C) draw-down to LGM atmospheric carbon concentrations, a profound decrease in land carbon (which could be ~850 Gt C as estimated by Jeltsch-Thömmes et al., 2019) as well as a source of DIC from the deep ocean sediments/$CaCO_3$. We see it would be of interest to discuss this in the manuscript, and will include this in a revised version.

Changes in the manuscript: Extension of the discussion to include information on the potential effects of a doubling of the efficiency of the biological pump on atmospheric pCO2 as discussed above.

Specific comments

Line15-26 (Abstract): In my reading, I think that “relative roles of physical and biological changes” is not clearly evaluated in the manuscript.

Author response: This sentence is meant to describe that we explored the net effect of physical changes (e.g., circulation, temperature, atmospheric forcing, land-sea mask) and biogeochemical changes (different dust field, offline exploration of the potential effects of an increased efficiency of the biological pump) in shaping the LGM ocean (and specifically its $\delta^{13}C$ distribution). As we do not present a range of different physical ocean states, we see that rephrasing of this sentence is appropriate. Related to this, we would rephrase p.2 l. 12-13 and p.12 l. 31-33 to clarify that we simulated LGM-PI changes in both the physical and biogeochemical state of the ocean and study its cumulative effect on $\delta^{13}C$.

Changes in the manuscript: We revised sentence ‘This modelling study explores the relative roles of physical and biological changes in the ocean needed to simulate an LGM ocean in satisfactory agreement with proxy data, and here especially $\delta^{13}C$.’ to ‘This modelling study presents a realization of the physical and biological changes in the ocean needed to simulate an LGM ocean in satisfactory agreement with proxy data, and here especially $\delta^{13}C$.’ Additionally, we revised p.2 l. 12-13 and p.12 l. 31-33 to clarify that we simulated LGM-PI changes in both the physical and biogeochemical state of the ocean and study its cumulative effect on $\delta^{13}C$.

Line23 (Abstract): The word “theoretical” appears not appropriate. (“potential” might be better)

Author response: We think that ‘potential (offline)’ would best summarize that we explored the potential effects of different efficiencies of the biological pump without doing additional modelling experiments. Similarly we would revise the other occurrences of the word ‘theoretical’ to clarify we mean exploring the potential (and offline) effects when we describe our approach.

Changes in the manuscript: We clarified the use of the word ‘theoretical’ throughout the text by adding the word ‘(offline)’ or ‘potential’ to clarify our intention to explore the potential (offline) effects whenever we describe our approach.

Line26-35 (Abstract): I think that this sentence (which describes remaining issue and future work rather than the direct conclusion of the study) should be removed or shortened.

Author response: As the model-proxy data mismatch is one of the central results of the study, we do wish to mention this in the abstract, but move the detailed discussion out of the abstract.

Changes in the manuscript: The discussion of the reasons for the model- proxy data mismatch is moved to the new discussion section 4.

Section2.4: This is key section for understanding how the authors control the efficiency of the ocean carbon pump, but I feel that its description is not very clear and difficult to fully understand.

For the demonstration, I request the authors to show the Figure of PO4_new after the adjustment by methods 1, 2, and 3, together with PO4_model.

Author response: Thank you for making us aware that the different methods of distributing additional regenerated PO_4 are not entirely clear in the current version of the manuscript. We were able to include a demonstration figure as requested (Fig S4, for the Atlantic and an increase in BP_eff to 75%), which shows how the 3 different methods of adding regenerated PO_4 will alter the regenerated PO_4 distribution. In addition, we updated p.6 l.35 to p.7 l.5 to improve the clarity of this section of text. Note that the total PO_4 concentration is kept constant (only redistributions between regenerated and preformed PO_4 are considered).

Changes in the manuscript: We added a new figure to the SM to visualize the differences between the 3 different methods for regenerated PO_4, and clarify the explanation of the methods in the main text (p.6 l.35 to p.7 l.5).

Line28 (page 6): Definition of deltaPCO4(reg) is given at lines 1-4 on page7 but should be described before eqns. (2)-(3).

Author response: Lines 1-4 on p.7 describe how the total global change in deltaPO4(reg) is distributed over the grid for the 3 different methods, while p.6 l.28 defines deltaPO4(reg) for a specific grid-cell which is relevant for the updated fields of O2, DIC and d13C. We understand the current description is confusing, and therefore clarified the explanation of the methods and definitions (p.6 l.28 to p.7 l.5) in the text (see also our response to the previous comment).

Changes in the manuscript: We clarified the explanation of the methods and definitions (p.6 l.28 to p.7 l.5) in the text.

Line20-26 (page8): The discussion here is not clear for me. What do the authors mean by “the transition line in the PO tracer in Fig.1”?

Author response: We note that the line in Fig. 1, which is the mean SSW PO value, is too thin. Besides that, we see that a more thorough introduction of the PO tracer and how it was used here will help the reader to understand Fig. 1.

Changes in the manuscript: We thickened the transition line in Fig. 1 and extended the caption of Fig. 1 as well as the text in section 3.2.1 (l.20-26) to clarify our use and interpretation of the PO tracer. We also thickened the line in the corresponding Pacific figures in Fig. S12.

Line2-28 (page11): The discussions made here are difficult to understand because the information on Bern3D is not given to readers at all.

Author response: The Bern3D model is mentioned in SM3 and in Sect. 3.3, and we see there is a need for a clearer introduction of the Bern3D model in the main text and how it was used in our *study (see also our reply to the next comment), and we addressed this by adding a new subsection under Methods.*

Changes in the manuscript: We added a new subsection 2.5 to describe the purpose and technical details of the Bern3D model and how it is used to estimate ΔDIC.

Line16 (page11): What does deltaDIC stand for? Its definition is missing.

Author response: ΔDIC is defined at its first occurrence on p. 11, l. 3 as 'the LGM-PI change in marine DIC'. Here, LGM for ΔDIC is the mean over 21 kyr BP to 19 kyr BP and PI is the mean of 500 to 200 yr BP. We see that this definition, together with the technical information on the Bern3D model (In the SM 3 and Sect. 3.3) could be lifted to a new subsection (Sect. 2.5) under Methods for clarity, which also addresses the previous comment.

Changes in the manuscript: We added a new subsection 2.5 as well as Appendix A to describe the purpose and technical details of the Bern3D model and how it is used to estimate ΔDIC.

Line29-38 (page11): For the authors' reference, as for the discussion about O2, Yamamoto et al. (2019, Climate of the Past) discuss the role of glaciogenic dust in glacial O2 changes.

Author response: Thank you for making us aware of this interesting paper. We included its results in our discussion on O2. This paper also highlights the importance of using a glacial dust field when looking at the biogeochemistry of the LGM ocean. As changing the dust field in the LGM simulation is the only change to the model which can directly affect the biogeochemical model through relief of iron limitation, we included the reference in our methods section as well (original p.5 l.24) to explain the interest of using the Lambert et al. (2015) dust dataset to force our model.

Changes in the manuscript: We included the results of Yamamoto et al. (2019) in our discussion on the LGM-PI O_2 changes as well as to argue for the use of a glacial dust field in our methods section.

Line12-29 (page12): For the authors' reference, as for deep water formation processes in the Southern Ocean, Kobayashi et al. (2015, 2018; Paleoceangraphy) discuss about its representation in the OGCM and its potential role in glacial water mass age and ocean carbon cycle. This study appears closely related to the discussion the authors made here.

Author response: Thank you for making us aware of these Kobayashi et al. studies from 2015 and 2018. We agree that including their findings in our discussion would improve this part of the manuscript, and we have done so in the revised version of our manuscript. Specifically, we mention now in Sect. 3.2.1 our simulation of the slowdown of the abyssal overturning cell as well as salinification of SSW while referring to the results of Kobayashi et al. (2015). We also mention the Kobayashi et al. (2015) study when underlining that both physical and biological changes must have occurred between the LGM and PI oceans. Note that we are not able to compare our *study directly to the study by Kobayashi and Oka (2018) as we excluded our sediment model and riverine fluxes in our simulations due to computational costs.*

Changes in the manuscript: We included the findings of Kobayashi et al. (2015; 2018) in our discussion on the remaining model-proxy data mismatch. The references are added to the reference list.

References of the response

Brovkin, V., Ganopolski, A., Archer, D., and Rahmstorf, S.: Lowering of glacial atmospheric CO2 in response to changes in oceanic circulation and marine biogeochemistry, Paleoceanography, 22, 10.1029/2006PA001380, 2007.

Galbraith, E. D., and Skinner, L. C.: The Biological Pump During the Last Glacial Maximum, Annual Review of Marine Science, 12, 559-586, 10.1146/annurev-marine-010419-010906, 2020.

Heinze, C., Maier-Reimer, E., and Winn, K.: Glacial pCO2 Reduction by the World Ocean: Experiments With the Hamburg Carbon Cycle Model, Paleoceanography, 6, 395-430, 10.1029/91PA00489, 1991.

Jeltsch-Thömmes, A., Battaglia, G., Cartapanis, O., Jaccard, S. L., and Joos, F.: Low terrestrial carbon storage at the Last Glacial Maximum: constraints from multi-proxy data, Climate of the Past, 15, 849-879, 10.5194/cp-15-849-2019, 2019.

Kobayashi, H., A. Abe-Ouchi, and A. Oka: Role of SouthernOcean stratification in glacial atmospheric CO2 reduction evaluated by a three-dimensional ocean general circulation model, Paleoceanography, 30, 1202–1216, 10.1002/2015PA002786, 2015.

Kobayashi, H., & Oka, A.: Response of atmospheric pCO2 to glacial changes in the Southern Ocean amplified by carbonate compensation, Paleoceanography and Paleoclimatology, 33, 1206–1229, 10.1029/2018PA003360, 2018.

Yamamoto, A., Abe-Ouchi, A., Ohgaito, R., Ito, A., and Oka, A.: Glacial CO2 decrease and deep-water deoxygenation by iron fertilization from glaciogenic dust, Clim. Past, 15, 981–996, 10.5194/cp-15-981-2019, 2019.

Dear Referee #2,

Thank you for your time and effort to provide constructive feedback on our manuscript. We have replied to each of your comments and concerns below (in italic text). Specifically, we included a separate discussion section in the revised version of our manuscript. In this section, the missing references that the reviewer pointed out are included. We hope the reviewer agrees with us that we addressed the reviewers' concerns about the clarity and structure of the text, as well as improved the discussion of potential implications of our results and the comparison with existing literature.

Yours sincerely,

Anne Morée and co-authors

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.

Author response: We think the reviewer refers to p. 1 l. 33 here. We will change the statement as well as shorten this part of the abstract (see also our response to reviewer (#1, response to comment on Line 26-35). Our results underline that only those coupled climate models that contain the processes and/or components that realistically change both ocean circulation and biogeochemistry will be able to simulate an LGM ocean in satisfactory agreement with proxy data. Such a simulation is also a test for Earth system models for their ability to reproduce natural climate variations adequately as a basis for reliable future projections, including human-induced forcing. I.e., a satisfactory fidelity of Earth System Models in reproducing orbitally forced climate variations will increase our confidence in these models as tools for projecting future anthropogenic climate change.

Changes in the manuscript: We shortened abstract lines 26-35 and moved part of text to the new discussion section.

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.

Author response: Agreed. We added the references to the original p2, lines 10&23 and extended our discussion on the physical changes in the new discussion Sect. 4.

Changes in the manuscript: The references Stein et al. (2020) and Marzocchi and Jansen (2019) are added to original p2, lines 10&23 and discussed in the new discussion Sect . 4. The references are added to the reference list.

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.

Author response: We included a time series over the last 1000 years of the LGM and PI simulations in the supplement (for S, T, AMOC and Drake Passage transport, Fig. S5 and S6) to give a visual impression of the equilibration/drift. Referral to this new figure is made in the beginning of Sect. 3 as well as in 3.2.1 where the LGM physical ocean state is presented. Regarding the Bern3D model, we see that besides the information on the Bern3D model in Sect. 3.3 and SM3, the model and its application in the context of our study could be introduced in the methods section as well, for which we included a new section and appendix.

Changes in the manuscript: Addition of a new equilibration time series figure in the supplement and referral to this in the main text, as well as a new methods Section (2.5) and Appendix A to describe the setup and use of the Bern3D model in this study.

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.

Author response: In order to improve the clarity and structure of section 3, we can agree that the inclusion of a separate discussion section would help and include this in the revised version of our manuscript. We were able to lift some of the model-data comparison discussion points that are currently spread throughout the Sect. 3 text into the new section, as well as provide a *dedicated section for the discussion of the remaining model-data mismatch after adjustment of the efficiency of the biological pump.*

Changes in the manuscript: We included a separate discussion section at the end of the manuscript that focuses on the discussion around model-proxy data mismatches. This section consists of two parts, one discussing the model-data mismatch of the original simulation, and a second dedicated to the remaining model-data mismatch after adjustment of the efficiency of the biological pump (i.e., p. 12 l. 12-29).

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

Author response: We see that no reference is made in Sect. 3.1 to Fig. S5, which shows the PI physical state and could already be referred to here (currently done in Sect 3.2). The biogeochemical state of the PI simulation is described in detail for the C isotopes in Tjiputra et al. (2020). Otherwise, our focus is on the change in the biogeochemical marine state (LGM-PI), which is shown in Fig. 2. To address the reviewer's comment further, we now provide supplementary figures of PI temperature (section), PP (vertically integrated), and regenerated phosphate (section) compared to observational estimates, since these are mainly discussed in Sect. 3.1.

Changes in the manuscript: We included reference to the original Fig. S5 in Sect 3.1 as well as new supplementary figures of temperature, PP and regenerated PO4 compared to observations.

Page 8

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

Author response: We now include the Burke et al. (2015) reference in our discussion.

Changes in the manuscript: We added and discussed Burke et al. (2015) when describing our simulation in Sect. 3.2.1.

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.

Author response: We agree these references should be cited here and did so in the revised version of the manuscript.

Changes in the manuscript: We added the Jansen (2017) and Marzocchi and Jansen (2019) references.

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.

Author response: As described in our response to the general comment on the result and discussions section, we now include a separate discussion section in the revised version of the manuscript. In the first part of this new section, where we discuss the model-proxy data mismatch of the original simulation we discuss among other things our results in more detail regarding buoyancy fluxes and the abyssal cell strength (which indeed weakens). We assume the reviewer means Klockmann et al. (2016) here (as in their reference list) and included the findings of Klockmann et al. (2016) in this discussion.

Changes in the manuscript: We now discuss our simulation with regard to (Southern Ocean) buoyancy fluxes and the strength of the abyssal cell in the new discussion section. Specifically, we also included and discussed Klockmann et al. (2016).

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.

Author response: See our response to p. 2, l 10 and 23 above.

Changes in the manuscript: We included these references here and in other appropriate locations where their findings are useful for our discussion. The references are added to the reference list.

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?

Author response: The lowered atmospheric pCO_2 is expected from the combined effect of the ocean volume decrease and increased CO_2 solubility due to decreased ocean temperatures (p. 9 l. 19-22). That is, mostly the physical C pump is represented in our study and driving down atmospheric pCO_2 (as evidenced by increases in DICpref and DICsat, see new discussion section). The lack (and actually decreased efficiency) of a soft tissue pump strengthening is also discussed in the original Sect. 3.3. The inability of our model to simulate the strengthening of the soft tissue pump is expected from earlier results for ESMs and our model setup (e.g. summary point 5 in Galbraith and Skinner, 2020; p. 9 l. 22-26) - and indeed indicates that some biogeochemical processes/mechanisms are lacking in these models. Pinning down the exact processes of this strengthening is an ongoing challenge, and beyond the scope of our study. Nevertheless, we decomposed the LGM-PI change in DIC into DICsoft, DICpref, DICsat, DICbio, DICcarb and DICdiss (Bernardello et al., 2014) and added a figure of this to the supplement in *order to visualize their individual contributions. We highlight this result and its discussion in the new discussion section.*

Changes in the manuscript: We clarify the atmospheric pCO_2 drawdown despite the decrease in BPeff in the context of the LGM-PI changes in the different C pump components (DICsoft, DICpref, DICsat, DICbio, DICcarb and DICdiss) in the new discussion section 4, and included in a new supplement figure Fig. S17.

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.

Author response: As described in our response to the general comment on the result and discussions section, we included a separate discussion section in the revised version of the manuscript. The discussion on p. 10 l. 10-19 is moved to the new discussion section to improve the structure of Sect. 3.

Changes in the manuscript: We included p. 10 l. 10-19 in the new discussion section.

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

Author response: Thank you for noting this, we see that Marzocchi and Jansen (2019) is more appropriate here than Jansen (2017) and will adjust the manuscript accordingly

Changes in the manuscript: As suggested

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.

Author response: As described above, we added a new methods section 2.5 and Appendix A on the Bern3D model. Here we paid specific attention to clarifying why and how the Bern3D model was used.

Changes in the manuscript: Addition of a new methods section 2.5 and Appendix A on the Bern3D model.

Page 12

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

Author response: As described in our response to the general comment on the result and discussions section, we included a separate discussion section in the revised version of the manuscript. The second part of this new section is dedicated to discussing the remaining model-proxy data error after adjustment of the efficiency of the biological pump, which is base on the original p. 12 l. 12-29.

Changes in the manuscript: We restructured the text to include a new discussion section, where p. 12 l. 12-29 forms a paragraph that discusses the remaining model-proxy data error after adjustment of the efficiency of the biological pump.

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?

Author response: We assume that the reviewer refers to the paper Ödalen et al. (2020). Their results, which showed decreased $\delta^{13}C$ while keeping (regenerated) PO_4 constant, could indeed be included in our discussion on the remaining proxy-data mismatch. With regard to the feasibility to test variable C:P ratios in our model setup: Our model is computationally more demanding than the cGENIE model as employed by Ödalen et al (2020) and we do not have the resources for repeating long runs for this currently.

Changes in the manuscript: Discuss the results on variations in the C:P ratio by Ödalen et al. (2020) and their potential implications for our remaining model-data mismatch.

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].

Author response: We did not carry out experiments with vastly different initial states. It is long known that different initial conditions for temperature and salinity can result in different circulation modes. However, in our case we assume that the initial conditions for the glacial ocean circulation would not be too different from preindustrial conditions and not fully different. Due to the high computational demand of our model, we cannot carry out multiple spin-ups (as 10,000 years done in the cGENIE model) with different initial conditions or tunings. This would not be feasible given currently available computational resources.

Changes in the manuscript: No changes were made.

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.

Author response: We assume that the reviewer refers to the paper Rae et al. (2018). As our paper does not deal with pH changes, we do not specifically discuss this article, but can include this reference as it highlights the central role of SSW as the reviewer points out.

Changes in the manuscript: Rae et al. (2018) is now cited on page 2 where the importance of SSW is mentioned, and is added to the reference list accordingly.

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.

Author response: We adjusted the text as described below

Changes in the manuscript: We replaced the sentence 'We prepared a PI and LGM ocean model state (NorESM-OC) with full biogeochemistry (including the carbon isotopes δ13C and radiocarbon) and dynamic sea ice.' with 'We prepared a PI and LGM equilibrium simulation using the ocean model NorESM-OC with full biogeochemistry (including the carbon isotopes δ13C and radiocarbon) and dynamic sea ice.'

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

Author response: We think clarifying that our approach is exploring the potential effects only (i.e. it is an approximation as no actual simulation is done) is important here, and we therefore replace 'theoretical' with 'potential (offline)' in the abstract.

Changes in the manuscript: Replaced 'theoretical' with 'potential (offline)' on p.1 l.23.

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".

Author response: See our response to the previous comment.

Changes in the manuscript: We clarified the use of the word 'theoretical' throughout the text by adding the word '(offline)' or 'potential' to clarify our intention to explore the potential (offline) effects whenever we describe our approach.

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

Author response: Thank you for noting this mistake, we will adjust the manuscript as suggested.

Changes in the manuscript: change p. 10, l. 30 miss-match to mismatch.

Everywhere: "Southern Source" should really be "southern-sourced".

Author response: We revisited the literature and see that both southern source water (e.g., Adkins, 2013; Curry and Oppo, 2005; Roberts et al., 2010) and southern-sourced water (Howe et al., 2016; Pöppelmeier et al., 2018) are commonly used. We therefore feel the current use of Southern Source Water (SSW) throughout the manuscript can be maintained.

Changes in the manuscript: None.

References

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

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