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 δ13C’. A response to each of the comments is provided below (in italic text). Specifically, we propose to 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 would be addressed. Additionally, we wish to improve 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

Review of “Evaluating the Biological Pump Efficiency of the Last Glacial Maximum Ocean using d13C” by Moree et al.

The authors discussed about the glacial changes in d13C distribution in the ocean by comparing LGM ocean (NorESM-OC model) simulations with proxy data. The model significantly underestimates the glacial d13C changes compared with the proxy data; for example, negative signal of d13C in the deep Atlantic Ocean inferred from the proxy data is not reproduced in the model. At the same time, the model shows the decrease of the ocean biological pump efficiency in the LGM (33%) compared with the PI (38%), opposite to the fact that this is believed to be increased from the proxy data. The authors discussed the response of d13C by artificially increasing the ocean biological pump efficiency. The authors concluded that an approximate doubling of the global mean biological pump efficiency from 38% (PI) to 75% (LGM) leads to the best-fit of d13C distribution between the model and the proxy. The manuscript deals with an important topic and contains interesting result which contributes to our understanding the glacial changes in the ocean carbon cycle. However, I think that the manuscript needs considerable revision. Followings are my comments about the manuscript, which I think needs to be seriously addressed before its publication.

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 will revise the manuscript as outlined below.

Changes in the manuscript: We will address this comment in two ways. First, we will revise 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 will extend our discussion by including a new discussion section at the end of the paper. Here, a more detailed and structured discussion on the lack of a simulated increase in the biological pump efficiency will be given. Specifically, we will 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: We plan to insert the following explanation and discussion into the discussion and conclusion at the end of our manuscript.

Changes in the manuscript: 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 CO2 (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 CO2 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 CO2 reduction as observed in the atmosphere.

(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/CaCO3. 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.

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 (f.e., 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 δ13C 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 d13C.
Changes in the manuscript: Revise 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 δ13C.' 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 δ13C.' Additionally, revise 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 δ13C.

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: Replace 'theoretical' with 'potential (offline)' on p.1 l.23. Additionally, rephrase other occurrences of the word theoretical throughout the text 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 include this in the abstract. Nevertheless, the discussion of the reasons for this mismatch could indeed be shortened, and we will do so in a revised version of our manuscript.
Changes in the manuscript: Shorten p.1 l. 28-35.

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 PO4 are not entirely clear in the current version of the manuscript. We will be able to include a demonstration figure (in the SM, for the Atlantic) as requested, which shows how the 3 different methods of adding regenerated PO4 will alter the regenerated PO4 distribution (for one biological pump efficiency) relative to the model regenerated PO4 distribution. In addition, we will update p.6 l.35 to p.7 l.5 to improve the clarity of the text.
Changes in the manuscript: We will add a figure to the SM to visualize the differences between the 3 different methods and clarify the explanation of the methods (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 will therefore clarify the explanation of the methods and definitions (p.6 l.28 to p.7 l.5) in the text.
Changes in the manuscript: We will clarify 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 will thicken the line in Fig. 1 and extend 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.

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 will address this by adding a new a new subsection under Methods.
Changes in the manuscript: We will add 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 will add a new subsection 2.5 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 will include 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 think it is worth it to also include the reference in our methods section (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: Include the results of Yamamoto et al. (2019) in our discussion on the LGM-PI O2 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 will do so in a revised version.
Changes in the manuscript: Include the findings of Kobayashi et al. (2015; 2018) in our discussion on the remaining mode-proxy data mismatch.

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.