[Reply to Reviewer #2]

We appreciate the constructive and beneficial comments by the reviewer.

Major points

Q1: In the introduction the authors emphasize the importance of simulations covering the entire glacial cycle to also capture the effects of slow processes. While it is clear that this is currently not possible due to the prohibitive computational costs, it would be interesting to discuss what effects and impacts would be expected from these slow processes and whether they could bias the results of the present study.

A1: In a revised manuscript, we will reorganize the introduction to a large extent to include the description about the timescale of various processes. We will thereby make the concept of this study clearer.

Q2: While by design of the experiments the largest changes are expected to be in the Atlantic, there are surely also important differences in the physical ocean states and biogeochemistry of the rest of the ocean. However, this is neither discussed nor shown in any of the figures.

A2: We will add plots for a Pacific section for various tracers, and add discussion about them accordingly.

Q3: In the two shallow LGM runs (LGMsw and LGMss) large changes in phosphate and carbonate ion concentrations and AOU exist. The authors argue that this is either related to the more sluggish deep ocean ventilation or the biological carbon pump. Yet, the ideal age tracer distributions, that in fact indicate younger bottom water in the entire Atlantic, and the stronger stream function in the deep Atlantic strongly suggest that this was only caused by the more efficient biological carbon pump in the Southern Ocean. Due to the importance of this process and the far-reaching effects I would like to see a more in-depth discussion and analysis of this matter.

A3: We appreciate the beneficial suggestion. We should have discussed the depths of 2000-3500 m and the deeper (>3500 m) depths separately. In the former depth range, expLGMws and expLGMss had older ideal ages, which contributed to the more efficient storage of remineralized matter. However, in the latter depths, we agree with the reviewer that the effect of the increased biological pump in the mid-to-high latitudes of the Southern hemisphere and the northward transport of remineralized nutrients by the bottom circulation prevailed over the effect of the younger age of the corresponding water. We will add these description to a revised manuscript. (Please see also A26 and A27)

Q4: In section 4.1 the authors mention that the applied alkalinity changes are in good agreement with previous estimates for carbonate deposition during the deglaciation. However, in section 4.2 it is then mentioned that the [CaCO3] are systematically too high most likely due to the uniformly increased alkalinity. How can this be reconciled?

A4: This discussion would have two aspects. First, to manage the compatibility of the 190 ppm and more reasonable carbonate ion concentrations, one needs to realize the low pCO_2 with the smaller amount of appended alkalinity. This would require the help of other mechanisms to reduce pCO_2: for example, higher solubility given by lower SST, a larger vertical contrast of DIC concentration by the even more stratified ocean (e.g. Kobayashi et al. 2021), and/or larger carbon storage in the deep water by stronger biological pump. The more efficient carbon storage given by these processes would relax the problem of too-high carbonate ion concentrations. Second, the compatibility with the post-glacial shallow water deposition of $CaCO_3$ would need to be satisfied, too. In expLGMss that needed the smallest amount of appended alkalinity of the three LGM experiments, the applied alkalinity corresponded to 2.5e16 mol of $CaCO_3$. This value is already close to the lower limit of the independently-estimated amounts of the shallow water deposition (i.e. 2.2e16 mol) that would have removed alkalinity from the ocean. Therefore, to incorporate the likely post-glacial deposition of $CaCO_3$ and accompanying reduction of alkalinity inventory into the evolution of the climate from the LGM to the modern, another source of alkalinity might be required. More dissolution of $CaCO_3$ in the deep-ocean sediments or more input of alkalinity from the land weathering would be able to serve as the alkalinity source. Considering that the amount of deep-sea carbonate burial is estimated to be rather higher during the last 20krys (Cartapanis et al. 2018), the increased input by the land weathering might be a more plausible explanation. Future studies to deal with the transient evolution from the LGM are expected to give more insights into this issue. In a revised manuscript, we will extend a discussion section by including these discussion.

Q5: Section 4.2 encompasses many comparisons of the LGM experiments to reconstructions of various parameters. However, I feel that there is a missed opportunity by not visualizing these results to a greater extent (currently only the CaCO3 MAR model-data comparison is shown). One could for instance show the LGM d13C and nutrient data by Oppo et al. (2018) in Figure 3. Further, the [CaCO3] gradients discussed in the text could be plotted against the reconstructions. This would surely help to better demonstrate where the model performs well and where there are biases.

A5: As in A2, we will significantly update the plots, some of which will include visual model-data comparisons as the reviewer suggested. For $\delta^{13}C_{DIC}$, we will overlap dots showing data by Peterson et al. (2014) and Yu et al. (2020). As in the current manuscript, we will refer to Oppo et al. (2018) in the main text because they do not provide point data for Holocene. For the carbonate ion, we will use the data by Yu et al. (2020). As to phosphate, we will not make overlaid plots because Oppo et al. (2018) only provide estimated distributions of phosphate given by inversion and do not have point data. Moreover, for these tracers, we will add plots for a Pacific section.

Q6: The authors try to assess the validity of the DIC – ventilation age relationship used by Sarnthein et al. (2013) and Skinner et al. (2015) and come to the conclusion that it does not hold for the LGM. However, one has to note that the previous studies by Sarnthein et al. (2013) and Skinner et al. (2015) used radiocarbon ventilation ages while in the present study the ideal age of the model was used for the assessment. In this context it is noteworthy that the ideal age and the radiocarbon ventilation age behave quite differently in the (model) ocean, mostly due to the additional effect of limited air-sea gas exchange under sea-ice for radiocarbon that the ideal age tracer does not see. This effect should be much stronger for the LGM simulations than the PI due to the colder temperatures and hence larger sea ice extent. It is therefore possible (or even rather likely) that the radiocarbon ventilation age is much older in the LGM simulations than in PI while the ideal age is younger in the global mean and the previously proposed relationship still holds. The DIC – age relationship should therefore be reassessed with respect to this issue.

A6: Thank you very much for this insightful comment. We will update the relevant figures and discussion by taking account of the reservoir ages of modern surface water depending on ocean basins based on Matsumoto (2007), and will further modify the LGM counterpart by considering the estimated increase in surface reservoir ages given by Skinner et al. (2017). These modification will alter the intercept of each regression line but will not affect the slope of them. As a result, the fact that the LGM model oceans have a different structure of the DIC–age relationship, where the Atlantic branches are separated from the others, is still valid. Although it is a discussion in a somewhat idealized framework, we consider that the basic ideas behind it (the mixing of northern-sourced water and the DIC-enriched southern-sourced water, and the effect of depth domain for the regression) would be useful for future more detailed examination.

Minor points

Q7: P1, L3-5: This sentence is slightly confusing, considering that you simulated time slices but here argue with the evolution of the reservoirs.

A7: In a revised manuscript, we will modify the initial sentences in the abstract.

Q8: P2, L2: The penultimate interglacial was MIS 7. Do you instead mean the last interglacial (i.e., the Eemian)?

A8: Thank you for pointing out. As the reviewer rightly assumed, we meant the last interglacial. We will modify it in a revised manuscript.

Q9: P2, L5: Orbital configuration not orbital elements.

A9: We will modify the manuscript following this comment.

Q10: P2, L24: As far as I’m aware there are no reconstructed concentrations of DIC.

A10: There we meant the estimate by Sarnthein et al. (2013) that this study had used as a constraint. We will rephrase the part concerned into "the estimated rise of mean DIC concentration in the deep ocean".

Q11: P3, L20: Do you mean that POM was fully remineralized in the bottommost cells?

A11: Yes. We will rephrase "dissolved in the bottom layer" into "remineralized in the bottommost cells".

Q12: P4, L8: Can you give a percentage for the adjusted mean salinity and nutrient concentrations.

A12: We increased the salinity by 1 psu and the other tracers' concentrations by 3%. We will add this information to a revised manuscript.

Q13: P4, L10: Was the 2.5 kyr spinup enough to reach equilibrium?

A13: The pCO_2 in the atmosphere was superbly equilibrated that the drift in the last 500 model years was 0.6 ppm or less (depending on the simulation). For the other tracers, if we select $\delta^{13}C_{DIC}$ as an example that should give the most strict criteria because of a uniform initial condition, the drift of $\delta^{13}C_{DIC}$ in the deep Northern Pacific (at 30N, 150W, 2900m) was 0.06 permil or less, which was less than a typical magnitude of data uncertainty. Therefore, we judge that the modelled states are reasonably equilibrated.

Q14: P4, L14: The Ruddiman Belt is defined by the deposition of ice rafted debris during Heinrich Events. The reference to this could therefore lead to confusion. Instead, better simply refer to the latitudinal band where the freshwater was applied.

A14: Thank you for pointing out. We will delete the relevant part "so-called 'Ruddiman belt'". The latitudinal band has been already defined in the current version of the manuscript.

Q15: P4, L14: Since the freshwater addition was compensated for, I would try to avoid the word "hosing".

A15: We will replace the word "hosing" with "freshwater forcing".

Q16: P5, L1: Here you mention that the atmospheric d13C signature was prescribed. Isn't this in conflict with freely evolving atmospheric CO2 concentrations in terms of the 13C budget?

A16: Although the treatment of ^{13}C in the current model configuration is not theoretically self-consistent as the reviewer pointed out, we took the approach for three practical reasons. 1. The prescribed atmospheric $\delta^{13}C$ that was fixed to a reliable value was expected to contribute to a better model representation of $\delta^{13}C_{DIC}$ in the ocean to be compared with observation-based data. 2. The deviation of the atmospheric pCO_2 from the required 190 ppm was minimum, so that the simulated state would reasonably approximate the consistent (with the also-required atmospheric $\delta^{13}C$) LGM state. 3. As far as we recognize, the available carbon isotope package provided by Jahn et al. (2015) does not deal with atmospheric $\delta^{13}C$ that evolves interactively and self-consistently with the air-sea gas exchange in the model, and therefore, further substantial model-development will be needed to implement it, which is beyond the scope of this study.

We will add similar descriptions to these to a revised manuscript.

Q17: P5, L15: It's Atlantic Meridional Overturning Circulation not ocean circulation.

A17: We will correct the error.

Q18: P5, L17-18: The zero isoline of the stream function is not equivalent to the separation of AABW and NADW as can be seen from dye experiments (e.g., for CESM: Gu et al., 2020).

A18: We realized that those two elements are not equivalent to each other, and that is why we used the word "associate". However, we admit the current expression is misleading, and will delete "which can be associated with the North Atlantic Deep Water (NADW) and Antarctic Bottom Water (AABW) masses" in a revised manuscript.

Q19: P5, L21: To me, it appears from Figure 2 that LGMws does not have a stronger bottom circulation than LGM or PI.

A19: We will enhance the current Fig.2 by including common stream-function plots, which will show that expLGMws has a stronger penetration of bottom water up to 20N, which is not resolved by the current plots.

Q20: P5. L26: Directly inferring from roughly correct SST changes the correct atmosphere-ocean partitioning of CO2 is quite a stretch. This completely ignores the other carbon pumps that also play a role in the partitioning.

A20: We only meant the air-sea gas exchange with the "carbon distribution between the atmosphere and the ocean", but we admit that the current expression is misleading. We will delete "hence the carbon distribution between the atmosphere and the ocean" because it is sufficient to mention the reasonable solubility at that point.

Q21: P5, L27: This is in conflict with Figure 3c. However, I suspect that something went wrong in Figure 3c.

A21: We re-plotted the Atlantic sections of salinity and added new horizontal plots as well to confirm the current Fig.3c is correct. Although the current expression "the vertical gradient of salinity is larger in most regions" is still valid theoretically because that is the case in the other oceans than the Atlantic (and in a small part of Atlantic), we will rephrase the sentence because it is misleading and inconsistent with the Atlantic section plot for expLGMws. Instead, we will extend the description of modeled salinity in a revised manuscript by introducing new plots to show comparisons with reconstructed paleo-salinity.

Q22: P5, L31: “In expPI, we obtained 276 ppm”. Please rephrase and expand this sentence.

A22: Combined with the next question, we will rephrase the relevant part.

Q23: P6, L1: The model is surely tuned to this PI pCO2, I therefore think that this is not necessarily an indication of the models "excellent ability" to predict pCO2.

A23: Agreeing with the reviewer, we will rephrase the sentences.

Q24: P6, L11: Typo "relfected".

A24: We will correct it.

Q25: P6, L18: Please try to avoid the word "observed" when talking about model results, as it suggests that the finding is derived from observations.

A25: We will modify the relevant parts.

Q26: P6, L25: But the very deep is younger than PI not older and the stream function (Fig. 2) indicates stronger or equal advection in the deep of all LGM runs compared to PI. How does this fit together? Was the longer-lasting storage of organic matter only at the mid-depth between 2 and 3 km? Why is the phosphate concentration also elevated below 3 km? Have you diagnosed the remineralized phosphate fraction from the model?

A26: We appreciate this beneficial remark. We have diagnosed the fraction of remineralized phosphate by estimating the remineralized amount based on AOU and the Redfield ratio defined in the model. The fraction is indeed higher in the all depth ranges deeper than 2000 m in the LGM experiments than in expPI, which gives another support for the fact that the increased phosphate concentration were mainly caused by an increase in remineralized phosphate as mentioned in the current version of manuscript. However, as the reviewer pointed out, we found that the reasons for the increased remineralized phosphate described in the current manuscript is insufficient. In the very deep water below 3km, the effect of the increased biological pump in the mid-to-high latitudes of the Southern hemisphere and the northward transport of remineralized nutrients by the bottom circulation overwhelmed the counter-effect of the younger age of the very deep water. We will add these description to a revised manuscript.

Q27: P9, L13-14: As mentioned before, from the ideal age it is clear that the bottom water was in fact not more stagnant for all LGM simulations.

A27: Similarly to A26, we will add words to describe the effect of increased biological pump.

Q28: Figure 3c: The distribution looks rather strange compared to the other experiments and other tracers. Please double-check.

A28: Please see A21. We will add discussion about the influence of the freshwater forcing on the simulated salinity.

Q29: Figure 6: Most of the map is white. Does that mean that in ~80% of the grid cells the 1°x1° bathymetry is outside the POP2 depth domain and no CaCO3 MAR can be calculated? If yes, can this be improved to show a continuous map?

A29: Motivated also by a comment from the other reviewer, for a revised manuscript we coupled MEDUSA simply or directly to every bottom grid cell of POP2 as in the preceding study (Kurahashi-Nakamura et al., 2020) to involve the whole ocean floors and carried out similar experiments again. This new method has enabled us to provide global continuous maps of MAR covering the entire ocean. Moreover, the global burial amount of $CaCO_3$ will be available with the new method.

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