

Clim. Past Discuss., referee comment RC2
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Comment on cp-2022-8

Anonymous Referee #2

Referee comment on "Glacial state of the global carbon cycle: time-slice simulations for the last glacial maximum with an Earth-system model" by Takasumi Kurahashi-Nakamura et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2022-8-RC2>, 2022

Kurahashi-Nakamura et al. present three time slice simulations of the LGM performed with the carbon isotope-enabled CESM1.2 fully-coupled GCM, that are used to investigate the global carbon cycle. The authors further investigate the sedimentary carbonate chemistry with an offline sediment model. The three experiments have in common that the artificial DIC and alkalinity additions have been tuned to achieve pCO₂ concentrations and global mean DIC concentrations in agreement with reconstructions. These changes in inventories are then discussed in the context of potential processes that could have provided/removed the required budgets during the deglaciation. Yet, the three LGM runs differ in their physical ocean state which has been achieved through additions of idealized freshwater/salt fluxes in the North Atlantic and Southern Ocean. The authors discuss the effects of the different AMOC states on the distributions of biogeochemical tracers and compare their results to previous studies. The manuscript is generally well-written and illustrated, but I feel that the analysis could go into more depth with additional figures better showing for instance the model-data comparisons. As such, I believe the manuscript would benefit from some revisions that I outline below in detail.

Major points

1. 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.

2. 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.

3. 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.

4. 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 $[CaCO_3]$ are systematically too high most likely due to the uniformly increased alkalinity. How can this be reconciled?

5. 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 $CaCO_3$ MAR model-data comparison is shown). One could for instance show the LGM $\delta^{13}C$ and nutrient data by Oppo et al. (2018) in Figure 3. Further, the $[CaCO_3]$ 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.

6. 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.

Minor points

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

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

P2, L5: Orbital configuration not orbital elements.

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

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

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

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

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.

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

P5, L1: Here you mention that the atmospheric $\delta^{13}\text{C}$ signature was prescribed. Isn't this in conflict with freely evolving atmospheric CO_2 concentrations in terms of the ^{13}C budget?

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

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).

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

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.

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

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

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.

P6, L11: Typo "relfected".

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

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?

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.

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

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?

References:

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