I appreciate the authors for carefully considering the previous comments and making substantial improvements to the manuscript. I just have a few additional minor comments that could further improve the manuscript before final acceptance.

Dear Referee,

Thank you very much for your positive comments and suggestions. We listed our responses to all of your points below and hope the manuscript is now satisfactory.

General comments:

Please plot land boundaries (coastlines) for Fig. S5.

Reply: Thank you for pointing this out. Coastlines have been added to all panels in Figure S5 in the updated Supplementary Information.

Line 111: The previous comment regarding "how the wind pattern is different in the LGM" referred to describing the actual changes in the wind field (e.g., changes in the position and/or strength of the westerlies). If Fig. S1 is included, it would be helpful to briefly describe what the wind anomalies represent, as this is currently not explained. While it is evident that the wind patterns differ, the nature of these changes is not discussed.

Reply: We have expanded Section 2.3 to briefly describe the main features of the LGM wind stress anomalies shown in Fig. S1 (from PMIP3 models). The new sentence is "Compared to pre-industrial conditions, the LGM wind field features an northward shift and intensification of the Southern Ocean westerlies, as well as enhanced trade winds in the tropics and pronounced wind stress anomalies in the North Atlantic (Fig. S1)." on lines 116-118.

Line 137: resulted "in" a shut-down?

Reply: Corrected.

Line 148: So, the analysis is based on only a single year? Typically, averages over the final 50–100 years of the spin-up are used to minimise the influence of internal variability. I am a bit skeptical about using only a single year.

Reply: We understand the referee's concern regarding internal variability. In the Uvic-ESCM, atmospheric forcing is prescribed using a seasonally repeating annual climatology without internal interannual physical variability. Therefore, once a simulation reaches a steady state, the annual cycle repeats identically year after year. We have added a few sentences on lines 151–153 in Section 2.4 to clarify this aspect of the model setup. "All simulations in the ensemble were spun up for more than 10,000 years under their respective boundary conditions until the marine biogeochemistry approached a steady state. The interannual variability at steady state is negligible because the model simulates a repeating annual cycle. Therefore, we use the results from the final year of the spin-up simulations for our analysis."

How does figure S7 support the statement "We find that, within the first 10 years of the spin-up, NPP and POC export under LGM biogeochemical conditions already decrease and approach their equilibrium levels. The lower atmospheric pCO2 in the ∗_LGMallbgc simulations compared to ∗_PIallbgc is therefore primarily driven by the redistribution of surface DIC and the associated changes in global air–sea CO2 fluxes before equilibrium is re-established."???

A time series spanning the spin-up would more clearly demonstrate the transition from the initial state to equilibrium. I do note that this sentence has been revised (Lines 377–379) compared to the original manuscript, and no longer explicitly discusses the temporal evolution. However, the statement "just before equilibrium is re-established" still does not appear to be supported by Fig. S7.

Reply: We agree with the referee that Fig. S7 shows steady-state spatial flux anomalies rather than the transient trajectory during spin-up. To avoid confusion and ensure strict support from the presented figures, we have rephrased this statement to focus on how spatial differences in air-sea CO_2 fluxes sustain the lowered atmospheric CO_2 at equilibrium. The sentence now is "The atmospheric pCO_2 in the $*$_LGMallbgc simulations is lower compared to $*$_PIallbgc, and is primarily driven by a spatial redistribution of surface DIC and the associated changes in global air–sea CO_2 fluxes (Fig. S7)." on lines 387–389.

Line 427:Is the term "nutrient robbing effect" an established term in the literature?

Reply: Yes, "nutrient robbing" (or "nutrient stealing") is an established term in the ocean iron fertilization and marine carbon dioxide removal (CDR) literature. We have added a key reference here. The revised sentence now is "... This 'nutrient robbing' effect, whereby stimulated productivity in one area depletes macronutrients in downstream regions, is a primary criticism of Ocean Iron Fertilisation as a marine CDR strategy, as it could limit long-term global net carbon sequestration (Shepherd, 2009)." on lines 443–445.

Line 431: What is meant by "model solutions"?

Reply: By "model solutions," we refer to the simulated ecosystem behavior under our variable stoichiometry framework, which allows net primary production (NPP) to be sustained even when surface macronutrient concentrations decline. We have rephrased this sentence to explicitly refer to the variable stoichiometry mechanism. Now it reads "By increasing pC:P and pC:N in response to nutrient stress, the variable stoichiometry formulation in our model sustains NPP even when macronutrient concentrations decline." on lines 446–448.

Section 4.3: (Section 4.2 in the original draft). I appreciate the authors for expanding this section. However, it still reads largely as an extension of the Results section, with substantial quantitative detail that makes the discussion difficult to follow conceptually. It would improve readability if the discussion focused more on interpretation and synthesis, with quantitative comparisons included primarily where they are directly compared with previous studies (percentage differences may be sufficient where appropriate).

Reply: We thank the referee for this suggestion. We have removed non-essential quantitative numbers and have rewrote the text to emphasise physical-biogeochemical mechanisms and conceptual synthesis in this section.

Line 509: Please include a reference for the observational data.

Reply: There is no observational data on line 509. Does this refer to the observed 90 ppm drawdown in pCO_2 during the LGM on line 495? We have added a reference for it. Now it is "... are only about 50 % of the observed ($\approx$90 ppm; Monnin et al., 2001)." On line 509.

References

Monnin, Eric et al. (2001). "Atmospheric CO2 Concentrations over the Last Glacial Termination". In: *Science* 291.5501, pp. 112–114. DOI: 10.1126/science.291.5501.112. eprint: https://www.science.org/doi/pdf/10.1126/science.291.5501.112. URL: https://www.science.org/doi/abs/10.1126/science.291.5501.112.

Review round 2 – egusphere-2025-4907 This manuscript demonstrates that uncertainties in model parameters can substantially impact the model's carbon system response to a change in climate. The results are valuable, in particular for the understanding of challenges with modelling palaeoclimate states such as the LGM and the models' ability to reproduce processes involved in ocean drawdown of atmospheric CO2. They can also inform evaluations of marine CDR methods and their potential. Thus, the manuscript be considered for publication, subject to minor revisions. The updated version of the manuscript takes into account both reviewers' concerns from the previous round regarding the clarity of the text and too high information density, and the authors have successfully worked to increase readability of the manuscript. They have improved the included figures, and moved figures with high information density, that include the full range of results, to supplementary material. A minor remaining question is whether the PI-to-LGM reduction in sedimentary Fe input is too substantial. The phrasing in the previous version of the manuscript lacked in clarity, but now that the phrasing is clearer (L. 384-390), I find that some further motivation would be helpful for the argument that the reduction of sedimentary Fe input, and the consequences of it, is a lower-range estimate. Specifically, it could be more clearly stated whether it is assumed that the lowered sea level would lead to the same amount of sedimentary input of iron in the LGM regardless of how large the PI input is, or if the loss of input when going from the PI to the LGM scale with the PI input. In the former case, the authors' argumentation that the estimate is in the lower range holds. In the latter case, one could argue that a higher level of PI input that is reduced by 1/5 would lead to a higher amount of available dFe in the LGM c.f. the current experiments, and thus different biogeochemical responses. The paragraph that follows (starting on L. 391) partly addresses effects of different dFe availability, so the two paragraphs could be linked by a clarification of how a higher PI sedimentary Fe release would impact dFe availability in the LGM.

Dear Referee,

We would like to express our appreciation for the careful review and constructive suggestions you provide. We have revised the manuscript accordingly.

Reply: In our model, sedimentary Fe release is parameterised as a function of organic carbon flux reaching the seafloor in grid cells shallower than the sea level threshold. When the sea level drops by 120 m during the LGM, continental shelf areas shallower than 120 m are exposed, removing $\approx$ 80% of the pre-industrial sedimentary Fe release surface area. Because the sedimentary Fe flux scales directly with pre-industrial sedimentary Fe release, a model setup with a higher pre-industrial sedimentary Fe inventory would experience a proportionally larger absolute loss of Fe during the LGM transition. Thus, our pre-industrial sedimentary Fe flux (12.6 $\mathrm{Gmol\,Fe\,yr^{-1}}$) is at the lower end of published estimates (0.6 –194 $\mathrm{Gmol\,Fe\,yr^{-1}}$), meaning that our simulated LGM reduction (-10.3 $\mathrm{Gmol\,Fe\,yr^{-1}}$) represents a conservative estimate of sedimentary Fe loss. We have updated Section 4.2 to explain this scaling mechanism explicitly and connect the two paragraphs as suggested.
The revised paragraphs are now (lines 390–406):
"Glacial-interglacial changes in sedimentary Fe input were not considered in several earlier studies (Somes, Schmittner, et al., 2017; Ödalen et al., 2020; Matsumoto, Rickaby, and Tanioka, 2020; Matsumoto, Tanioka, and Rickaby, 2020; Vollmer, Ito, and Lynch-Stieglitz, 2022). Nevertheless, the strong sensitivity of the biogeochemical inventories and fluxes to changes in sedimentary influx of Fe demonstrates its importance. In our model, the lower glacial sea level leads to a proportional reduction in sedimentary Fe supply from ∗_PIallbgc to ∗_LGMFesed ($\approx$80 %, corresponding to a drop of 10.3 $\mathrm{Gmol\,Fe\,yr^{-1}}$). This reduction is larger than the increase in atmospheric Fe deposition from ∗_PIallbgc to ∗_LGMFedep (4.7 $\mathrm{Gmol\,Fe\,yr^{-1}}$), resulting in smaller dFe inventories in ∗_LGMallbgc.
At first glance, one might expect that the magnitude of sedimentary Fe input in the model is relatively large and that its reduction could therefore be overestimated. However, the simulated sedimentary Fe flux in PIctl_PIallbgc (12.6 $\mathrm{Gmol\,Fe\,yr^{-1}}$) falls close to the lower end of estimates from other models (0.6–194 $\mathrm{Gmol\,Fe\,yr^{-1}}$, Tagliabue et al., 2016). A recent sensitivity study also shows that a much higher present-day sedimentary Fe release

(117 Gmol Fe yr^{-1}) yields a better model-data fit in global and surface dFe distributions (Somes, Dale, et al., 2021). Assuming that the glacial loss of sedimentary Fe input scales proportionally with the PI baseline due to shelf exposure, our low PI baseline implies that the absolute drop in sedimentary Fe supply (10.3 Gmol Fe yr^{-1}) is a conservative estimate; a higher PI baseline would yield an even larger net Fe deficit relative to the atmospheric supply. On the other hand, a higher PI baseline would also leave a larger absolute amount of sedimentary Fe available during the LGM, which would mitigate the severe ocean-wide iron limitation observed in our simulations. This highlights the critical need to better constrain both the baseline magnitude and bathymetric scaling of sedimentary Fe release when evaluating glacial-interglacial biogeochemical cycles."

Minor comments Abstract L. 14 – Typo. 20x20 does not equal 480. I assume that this should say 24 simulations (i.e. the number of boundary conditions) per parameter set.

Reply: Thank you for bringing this to our attention. The total of 480 simulations comes from running each of the 24 combinations of boundary conditions across 20 selected parameter sets ($24 \times 20 = 480$). To ensure this calculation is clear and to avoid confusion, we have updated the phrasing in the abstract to explicitly mention "For each of the 24 combinations". The revised sentence now is "For each of the 24 combinations, we perform 20 simulations using the 20 biogeochemical parameter sets selected out of 600 [...] resulting in a total of 480 simulations." on lines 13–15.

Introduction L. 69: "may have varied during the LGM" – I think you mean "may have differed in the LGM compared to PI" because you are not studying the variability that was occurring during the LGM. L. 79: "estimating pCO2 changes in the LGM" – Similarly to L. 69, I think you mean difference between PI and LGM.

Reply: We have updated both instances accordingly.

Materials and Methods L. 92-97: The new schematic that details the experimental design is very helpful for understanding every step of the modelling approach. The figure is currently mentioned in general terms on L. 149, but it would be more efficient to mention it before describing the details of the design in the text, so that the reader can follow each step. Therefore, it should ideally be referenced already here in the first section that details the performed simulations (Section 2.2), with an explanatory sentence, e.g. "All steps of the experimental design are illustrated in the schematic Figure X, with the model calibration and selection of the best parameter sets described in the left hand column." or similar.

Reply: Many thanks for your suggestion, we have updated the explanatory sentence at the beginning of Section 2.2 accordingly from line 92 to 93.

Results L. 215: "The average NO3- concentration in the ∗_LGMFedep simulations is slightly higher than in ∗_PIallbgc." – Based on Fig. S3c, it looks like only the LGMatmpi gives a higher concentration for ∗_LGMFedep. Is the average for the whole set really higher than ∗_PIallbgc?

Reply: We apologies for the mistake here. The nitrate concentration in the *LGMFedep is slightly "lower" than in ∗_PIallbgc. We have corrected this in the manuscript, now on line 220.

L. 219-220: "The enhanced N_2 fixation due to the greater phosphate availability in ∗_LGMPO4 and the reduced benthic denitrification in ∗_LGMbdeni [...]" - N_2 fixation and denitrification are discussed explicitly in Section 3.3.2, so it would be good to cross-reference it here → add "(see also Section 3.3.2)

Reply: We have added a cross-reference here and it is now on line 225.

L. 222-223: "dFe supply from atmospheric deposition in ∗_LGMallbgc seems to compensate that effect." – Given that ∗_LGMPO4 and ∗_LGMbdeni give much more clearly positive effects on the average NO_3^- concentration than ∗_LGMFedep, I would expect that these are the ones counteracting the negative effect of ∗_LGMFesed. If this is not the case, this needs to be explained and motivated.

Reply: We agree that LGMPO4 and LGMbdeni give much more positive effects on NO_3^- concentration than LGMFedep. Nevertheless, because the decline in NO_3^- in ∗_LGMFesed is much larger than the sum of the positive effects of ∗_LGMPO4 and ∗_LGMbdeni, the additional Fe supply from LGMFedep is crucial. Together, these three positive drivers overcompensate for the sedimentary Fe reduction, ultimately producing the net increase in NO_3^- observed in ∗_LGMallbgc relative to ∗_PIallbgc. We have updated the text to clarify that ∗_LGMPO4 and ∗_LGMbdeni also contribute to this net increase in NO_3^-. The new sentences now read "Although the NO_3^- inventories in ∗_LGMFesed are substantially reduced due to a lower sedimentary flux of dFe, this negative effect is compensated in ∗_LGMallbgc by the combined contributions of increased atmospheric Fe deposition (∗_LGMFedep), higher PO_4^{3-} inventories (∗_LGMPO4), and reduced benthic denitrification (∗_LGMbdeni)." on lines 227–229.

L. 240-241: "The average sPO_4^{3-} in the ∗_LGMFesed simulations is about 62% higher than in ∗_PIallbgc [...]" – It would be helpful to remind the reader that sedimentary input of Fe is substantially reduced in the LGM. → "The average sPO_4^{3-} in the ∗_LGMFesed simulations, where sedimentary input of Fe is reduced to 1/5 compared to the PI, is about 62% higher than in ∗_PIallbgc" This is currently mentioned on L. 248 but would be helpful already in this subsection.

L. 241: This is further discussed in Section 3.3.3, specifically on L. 350, so it would be good to add a cross-reference to that section → (See also Section 3.3.3)

Reply: We have include the information regarding sedimentary input of Fe and the cross-reference in the sentence. Now it is "The average sPO_4^{3-} in the ∗_LGMFesed simulations, where the sedimentary input of Fe is reduced to about one-fifth compared to ∗_PIallbgc, is about 62 % higher than in ∗_PIallbgc, i.e., even higher than in ∗_LGMPO$_4$ (Fig. S3e and Table S1, see also Section 3.3.3)." on lines 246–248.

L. 264-266: "The ∗_LGMPO4 and ∗_LGMbdeni simulations yield DIC concentrations similar to those of the pre-industrial control. Our LGM simulations yield higher DIC concentrations than the PIctl_∗ simulations throughout the different biogeochemical configurations (Fig. S3o)." – Phrased like this, these two sentences seem somewhat contradictory, first saying that some of the LGM simulations are similar to the pre-industrial control and then saying that they should all yield consistently higher values. Based on Figs. S3g-o, I think the issue is in that you mix talking about the PIallbgc and PIctl physical conditions, but do so in an unclear way by just referring to "pre-industrial control" when you in fact mean PIallbgc. It would be good to clarify this, for example as follows: "The ∗_LGMPO4 and ∗_LGMbdeni simulations yield DIC concentrations similar to those of the ∗_PIallbgc (Fig. S3g). Our physical LGM configurations yield higher DIC concentrations than the PIctl_∗ simulations throughout the different biogeochemical configurations (Fig. S3o)."

Reply: We appreciate the suggestion, which improves the information delivered. The sentences now are "The ∗_LGMPO$_4$ and ∗_LGMbdeni simulations yield DIC concentrations similar to those of ∗_PIallbgc (Fig. S3g). Our physical LGM configurations yield higher DIC concentrations than the PIctl_∗ simulations throughout the different biogeochemical configurations (Fig. S3o)." on lines 271–273.

L. 269: "The glacial-interglacial changes in the strength of the solubility pump caused by a reduced temperature [...]" - Glacial-interglacial is from LGM to PI, i.e. it would mean an increased temperature → rephrase to interglacial-glacial

Reply: Corrected. Now it is on line 276.

L. 274-276: "Switching to LGM Fe fluxes has a relatively strong effect on pCO2, when compared to the effects of the LGM PO43– inventory or benthic denitrification." – This sentence seems redundant, as the sentence that follows essentially repeats this information but in more detail.

Reply: We have removed the sentence.

L. 301: “When combined with the biogeochemical conditions” – I think you perhaps mean “With all biogeochemical conditions combined”

Reply: Thank you, we have revised that phrase and it is now on line 306.

L. 305: It could be helpful to add a sentence of the style “In summary, only ∗_LGMFesed and LGMatmpi_∗ affect NPP in a substantial way”.

Reply: We have added the sentence at the end of the paragraph. Now it is on lines 310–311.

L. 311-312: “Proxy-based studies indicate that changes in export production during the LGM [...]” - I think you mean LGM export production compared to PI, not changes during the LGM itself.

Reply: We have revised the sentence to ”Proxy-based studies indicate that changes in export production during the LGM relative to the pre-industrial were spatially heterogeneous ...” on lines 317–318.

L. 317: First use of the acronym WC for water column, so it needs to be defined.

Reply: We have spelled out WC and it is now ”... WC denitrification (water-column N-loss)...” on lines 323–324.

L. 321-322: “In the model, N2 fixation is the counterpart of WC and benthic denitrification, thus N2 fixation equals the sum of WC and benthic denitrification at equilibrium.” – To demonstrate that this holds, it might be good to give the actual numbers in Tg N yr-1 in parenthesis behind the percentages in the sentences that follow after this one, or give one example.

Reply: We have added the actual number of rates behind the percentages as suggested. The paragraph now is on lines 327–338.

Discussion

L. 360: “The UVic-ESCM responds to LGM wind stress changes” – Clarify that you mean change from PI to LGM wind stress, not wind stress changes that happened during the LGM itself.

Reply: We have revised the sentence and now it is ”The UVic-ESCM responds to the transition from pre-industrial to LGM wind stress ...” on line 368.

L. 360-365: You only mention the individual effects of wind stress and atmospheric moisture diffusivity on the AMOC. What is the combined effect of all the LGM physical changes on the AMOC?

Reply: We have added ”When all LGM physical boundary conditions are applied simultaneously (LGMatmctl_PIallbgc), the maximum AMOC strength at 26.5°N shows a 29% reduction, and the depth remains similar relative to pre-industrial conditions (PIctl_PIallbgc).” at the end of the paragraph. Now it is on lines 373–375.

L. 372 “glacial-interglacial” → interglacial-glacial

Reply: Corrected. Now it is on line 382.

L. 384-390: See general comments in the beginning of the review.

Reply: Thank you. We have revised the paragraph accordingly.

Figure 7: The squares with dashes (LGMbdeni) are not visible anywhere in the two subplots, unless they have accidentally been filled with colour. There seems to be a cluster of filled squares close to the origin point that could be these ones, as it is stated in the caption that they should be located around there. It is also possible, based on looking at fig. 8., that they are just clustered so closely that they end up looking as filled squares. In that case, maybe consider changing to a different symbol, e.g. diamonds, and then changing Fig. 8 accordingly, to keep the symbols consistent.

Reply: Thank you for the suggestion. We have replaced the dashed quares with open diamonds for LGMbdeni in both Fig. 7 and 8.

Figure 9: The use of the rainbow colour palette results in an unfortunate combination of red and green dots (no blue) that is inaccessible to people with impaired colour vision. Consider changing to a different colour scheme (e.g. based on red, yellow, blue).

Reply: We have changed the color palette to a colorblind friendly one.

References

Matsumoto, Katsumi, Rosalind Rickaby, and Tatsuro Tanioka (2020). "Carbon Export Buffering and CO_2 Drawdown by Flexible Phytoplankton C:N:P Under Glacial Conditions". In: *Paleoceanography and Paleoclimatology* 35.7, e2019PA003823. DOI: 10.1029/2019PA003823.

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