In this paper, Chien et al. take on the question of the effect of biogeochemical parameter uncertainty on nutrient distributions, atmospheric pCO2 drawdown and more in model simulations of the Last Glacial Maximum (LGM). This question is omnipresent in palaeomodelling and, thus, the study is scientifically valuable. They explore a set of biogeochemical and physical boundary conditions under which they test the effect of parameter uncertainty, running ensembles with the same boundary conditions but different combinations of parameter settings that yield similarly representative control states in comparison to present-day observations. By doing this, they allow themselves to answer two different sets of questions. On the one hand, they give an overview of how large the variability caused by parameter uncertainty may be in modelling studies of LGM ocean biogeochemistry and carbon storage. On the other hand, they explore this variability by producing their own estimates of glacial-interglacial changes in ocean biogeochemistry and carbon storage with more robust uncertainty ranges compared to studies with a fixed set of parameter values. They explore general results of the various boundary conditions, finding that changes to iron input both from atmospheric deposition and sedimentary sources have the most profound impacts on the ocean biogeochemistry among the biogeochemical boundary conditions. The range in drawdown of atmospheric CO_2 between the preindustrial control and their full LGM simulation is as large as 50% of the average.

While the science in itself is sound, and the conclusions interesting, the information density of the results section makes it nearly impenetrable. An overall reduction in detail (recital of numbers) and amount of figure panels would make the material more accessible to the reader. In addition, the text would benefit from some rewriting, on the one hand to make sure the story does not get lost in the details, and also some checks for redundancy and restructuring in terms of what information belongs in what part of the manuscript. I therefore recommend major revisions.

General comments

"While the science itself is sound, and the conclusions interesting, the information density of the results section makes it nearly impenetrable." – This is a clear case of "less is more". I would strongly advice the authors to decrease the amount of information in the results section, because in the current form it is very difficult for the reader to follow and make sense of the results. I understand the urge of wanting to include all the information from this vast dataset, but if that prevents people from reading and understanding the data, it is only counterproductive. Having figures with 24 sub-panels, each presenting results of 20-24 sets of simulations, with max-min and three quartile indicators, sums to as much as 2880 data points in one figure where the reader has to search for numbers that are mentioned in the text. The whole right-most column in Fig. 8 is never referred to, thus including those panels in the main manuscript (or at all) is not helpful but instead adds to the overwhelm. Some can be moved to supplementary material, but some of the information in figures 6-8 is simply redundant and different ways of presenting the same thing. This can surely be done more efficiently. The asterisks are another "added information" that seems to serve little purpose and that I struggled to understand what they are actually showing. This is not well described in the figure captions. In the text, referencing to specific figure panels can also be improved. Also, it would be helpful for the reader if the text mentioned the sets of simulations in the same order as they are listed in the sub-panels (from left to right), because it reduces the time that is spent searching for the correct one.

In the introduction and methods sections, I would like to see more motivation for the choice of which physical boundary conditions are tested. There are many others that may be equally, or more, relevant. For example, ocean diffusivity and mixing (e.g., tidal), sea ice physics and so on. For some reason, only atmospheric physics are changed, despite this being an ocean study.
Because of the complex nature of the experimental design, I would recommend adding a flow chart that shows all steps and groups of simulations.

Referring to combined sets of simulations, e.g. PIctrl-PIallbgc, with a dash as the link is confusing. Normally, this would indicate that it is a computed anomaly between two different ensembles, not one set of simulations that use both, and this tripped me up many times while reading. It may seem like a minor detail, but it hampers the readability of the paper. I would suggest to switch to an underscore: Pictrl_Piallbgc.

In terms of scientific interpretations, the only thing I am not entirely convinced about is that the changes in strength of the ocean overturning circulation are entirely unimportant. The AMOC is not the only component of the ocean overturning, and what matters is the overall deep ocean residence time, where the AABW cell also plays a role.

Reply: We thank the referee for these comments. We agree that further improvement of the manuscript was required. We have revised the manuscript according to the suggestions as follows:

1. reduce results:

Reply: We have followed the suggestions and moved Fig. 6 and right panels in Fig. 8 to the supplementary. We have also tried to improve the clarity of the text and figure, and we believe that also helps the readers to follow the information provided.

2. text mentioned the sets of simulations in the same order as they are listed in the sub-panels (from left to right)

Reply: We have reordered the presentation of simulation results as they are listed in the panels throughout Sections 3.2.3 to 3.3.1 to improve the readability of the text.

3. more motivation for the choice of which physical boundary conditions are tested.

Reply: We agree that the selection of physical boundary conditions warrants clearer motivation. We chose wind stress and reduced moisture diffusivity across the Southern Ocean because their effects on the AMOC in the same model (UVic-ESCM) were investigated previously in Juan Muglia and Andreas Schmittner (2015) and Juan Muglia, Skinner, and Andreas Schmittner (2018), respectively, although the physical and biological configurations used there are not identical to those in this study. We have included the references in the boundary conditions section and revised the text as: "Besides the different orbital parameters, LGM conditions, including the strength of the AMOC, likely also deviated from the pre-industrial era by a lower moisture diffusivity over the Southern Ocean and a different wind pattern (Juan Muglia and Andreas Schmittner, 2015; Juan Muglia, Skinner, and Andreas Schmittner, 2018). We adopt these forcings from Somes, Andreas Schmittner, et al. (2017) and Juan Muglia and Andreas Schmittner (2015) to investigate their influence on the marine biogeochemistry and carbon cycle. Specifically, meridional moisture diffusivity over the Southern Ocean was reduced by a factor of 2, and the wind stress patterns are from monthly averages of models which have participated in the PMIP3 (Fig. S1)."

4. a flow chart

Reply: We have added a flow chart (Fig. 2) to show the procedures and configurations in the experiments.

5. change hyphen to underscore for the naming.

Reply: We have changed hyphen to underscore for naming of the simulations.

6. Concerns regarding ocean overturning circulation are entirely unimportant

Reply: In the original manuscript around lines 278–279 we only compared the global average DIC concentrations and found that the values are similar among the LGM simulations. We added a supplement figure (Fig. S2) for the comparison of DIC profiles, and we found that deep-water concentrations are indeed different for different circulations, and it is highest under the LGMatmctl condition, which has the most sluggish AMOC among the four conditions we tested.

We have revised the manuscript to clarify that our results do not imply that overturning circulation changes are

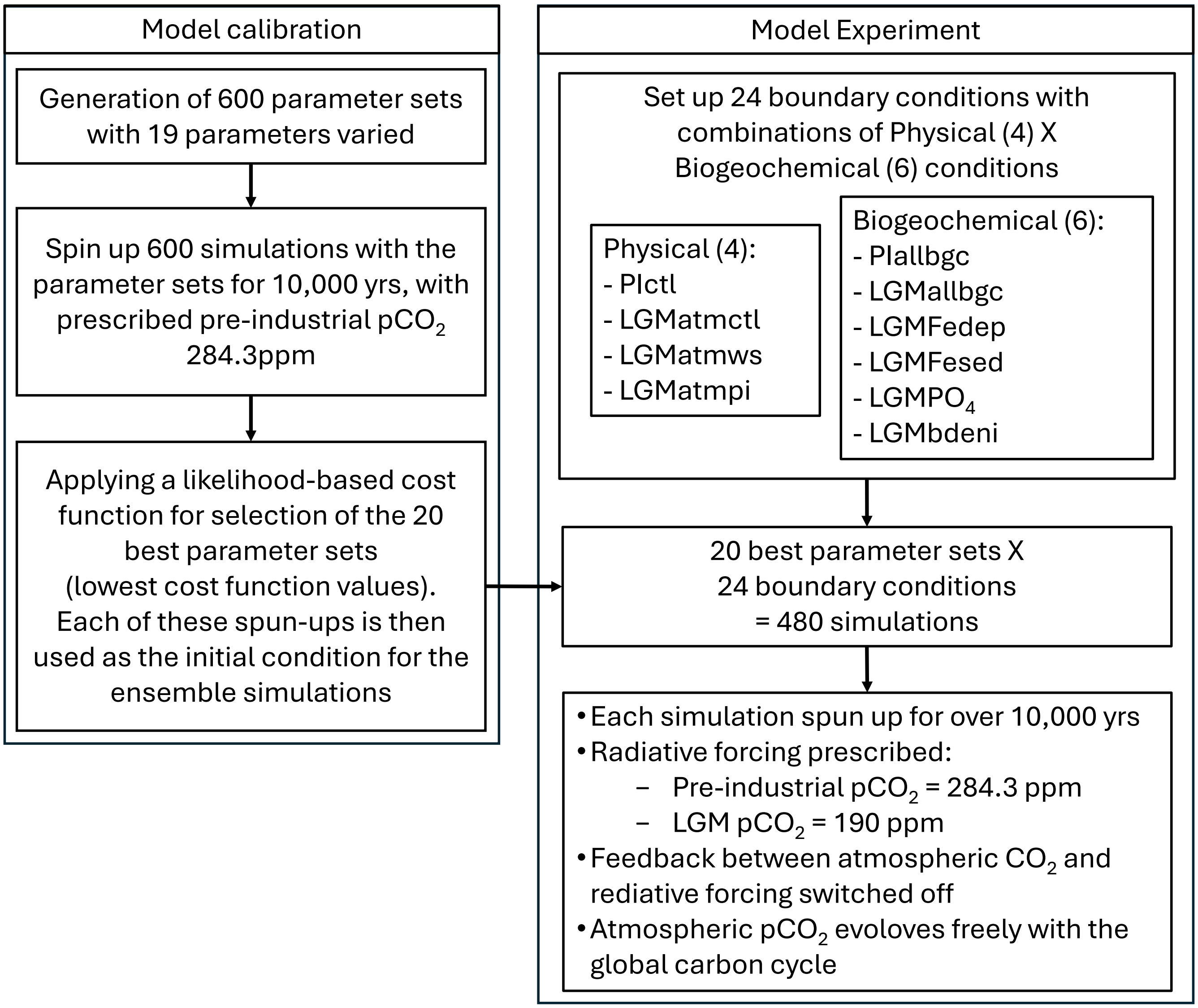

Figure 2: Schematic representation of the experimental workflow, including selection of the parameter sets and the configuration of model experiment.

unimportant, but rather that the relationship between circulation strength and DIC storage is not straightforward and likely involves additional factors such as circulation geometry and deep ocean residence time. We have added a paragraph: "While global average DIC concentrations appear similar across the LGM physical configurations, the vertical distribution tells a more complex story (Fig. S2). Specifically, deep-water DIC concentrations vary with the circulation state; the LGMctl configuration, which exhibits the most sluggish AMOC in our ensemble, shows the highest deep-water DIC inventory. This suggests that the relationship between circulation strength and DIC storage is not straightforward and likely involves additional factors such as circulation geometry and deep ocean residence time."

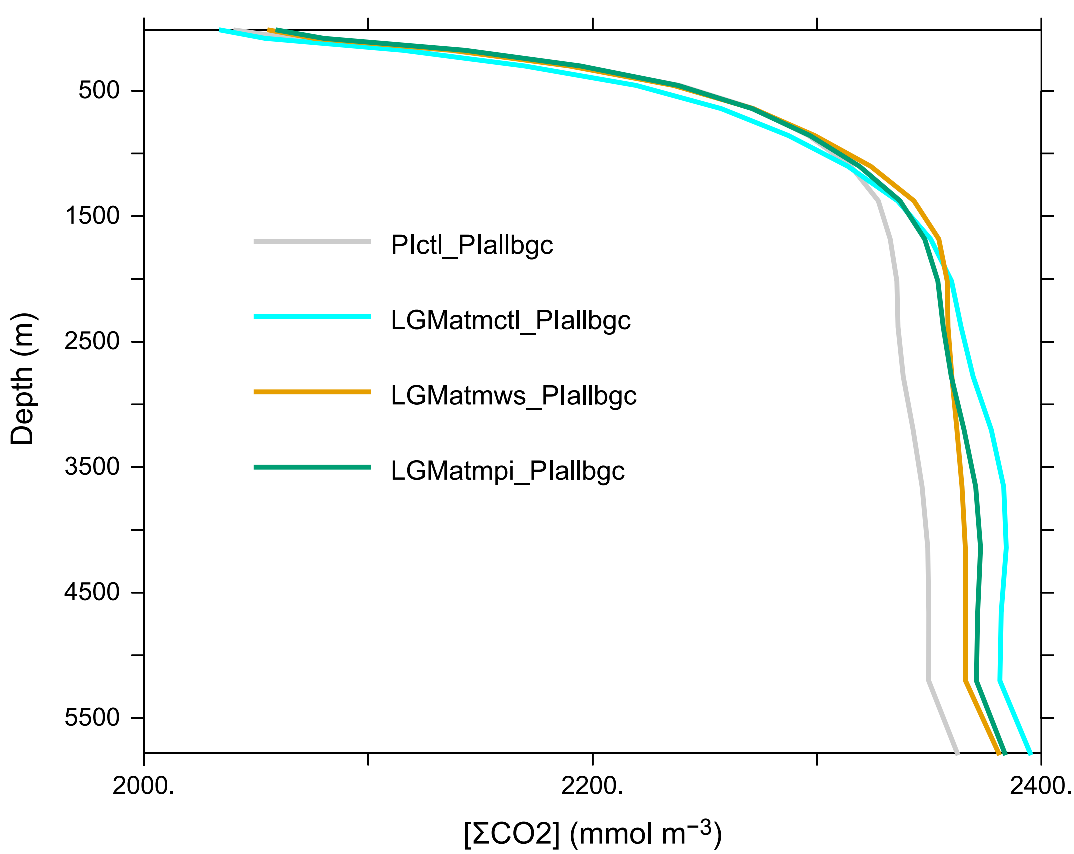

Figure 2: Globally-averaged vertical profiles of DIC (ΣCO_2) in PIctl_PIallbgc, LGMatmctl_PIallbgc, LGMatmws_PI-allbgc, and LGMatmpi_PIallbgc simulations.

Specific comments

Abstract

The abstract does not make it entirely clear what the sensitivity study is aiming to test. Add a sentence, or rewrite to clarify.

Reply: We have added ”We aim to quantify the uncertainty in simulated LGM marine biogeochemistry and atmospheric pCO_2 arising from uncertainties in parameter settings and boundary conditions.”

L. 3-4: Changes in sea-ice which affects gas exchange with the atmosphere should also be mentioned.

Reply: We added ”...reduced air-sea gas exchange due to changes in sea-ice coverage, ...” and also in the introduction ”reduce air-sea gas exchange due to changes in Antarctic sea-ice coverage (Stephens and Keeling, 2000),”.

L. 22-23: “ The difference between the maximum and minimum glacial pCO2 decreases amounts to 50% of the 43 ppm average decrease” – I had to read this sentence three times to understand it. You almost need a figure to see what is being compared to what. Consider rephrasing to clarify.

Reply: Thank you for pointing this out. We have rephrased the sentence to improve clarity and reduce ambiguity. Now it is ”The spread between the maximum and minimum decrease in simulated glacial pCO_2 is approximately 50% of the mean decrease (43 ppm)”.

Introduction

L. 25-40: Changes in disequilibrium carbon are too important to not be mentioned here. Cite e.g. Khatiwala et al. (2019), https://doi.org/10.1126/sciadv.aaw4981 that you refer to in the discussion.

Reply: We added ”..., changes in air-sea disequilibrium of carbon (Khatiwala, A. Schmittner, and J. Muglia, 2019), ...”

L. 68-69: The only physical changes that are imposed are in atmospheric physics. Please motivate further (see General comments)

Reply: Please see our responses for the General comments above.

L. 75-77: Please clarify how/why the results may provide insights for CDR. This is not clear from the current text.

Reply: We thank the referee for pointing out this lack of clarity. We revised this part as: ”The biogeochemical mechanisms driving glacial carbon drawdown serve as critical natural analogs for evaluating ocean-based carbon dioxide removal (CDR) strategies. For instance, uncertainties in how the glacial ocean responded to increased dust deposition mirror the uncertainties in assessing the efficacy of ocean iron fertilization. Our results may provide insights not only regarding the interpretation of past pCO_2 variations, but also into the effectiveness of potential CDR approaches.”

Materials and Methods

L. 86: : “values assigned to 19 model parameters” – Since the title of the subsection says “the best 20 parameter sets” and suddenly you talk about 19 model parameters, it is very likely that the reader confuses the number of sets of parameters with the number of parameters and think that you just made a typo here. Consider phrasing this more carefully so that there is no risk of misinterpretation.

Reply: Thank you for pointing this out. We have revised the sentence to clearly distinguish between the number of parameter sets (20) and the number of model parameters (19), in order to avoid potential confusion. Now it is ”We constructed 600 parameter sets, each representing a unique combination of values assigned to 19 model parameters, ...”

L. 98: “currently available observations” – How, if at all, do you account for the fact that the observations are not obtained during pre-industrial conditions, but in a state that is increasingly anthropogenically forced? This is an issue that we all face, but it should to the very least be discussed in the context of how it might affect what sets of parameters ARE actually the 20 best, given that your control state aims to be pre-industrial.

Reply: We agree that this is an important point. We have to make the assumption that the effects of those anthropogenic signals are small relative to spatial variability: "Modern observations may include anthropogenic signals, which could bias parameter selection. Thus, we assume here that these effects are small relative to spatial variability and note this as a source of uncertainty we cannot quantify."

L. 104-105: "we configure a 120 m lower sea level compared to PI [...] (ocean bathymetry and volume remain the same as in the PI) – How can you keep ocean bathymetry and volume if sea level is 120 m lower? This is not clearly explained. Also, if ocean volume is the same as in PI, thereby allowing for a larger ocean carbon inventory than LGM ocean volume would have held with the same DIC concentration, this is a source of error/bias in your pCO2 drawdown, see Lhardy et al. (2021) https://doi.org/10.1029/2021PA004302

Reply: The 120 m lower sea level was not implemented through an explicit modification of model bathymetry. Instead, it was represented diagnostically in the parameterizations of benthic processes. Specifically, we excluded benthic denitrification and sedimentary Fe input in regions shallower than 120 m relative to the pre-industrial sea level, thereby mimicking the exposure of continental shelves under LGM conditions. All other physical and biogeochemical processes were computed using the same bathymetry as in the pre-industrial control simulation. This approach allows us to isolate the impact of shelf exposure on benthic fluxes without introducing additional changes to ocean circulation or ecosystem structure that would arise from a fully modified LGM bathymetry. We have included this information in the methods section.

L. 105-106: "reduced sedumentary input of Fe" – It would be more helpful to the reader to mention what is actually changed and how, not just refer to these papers.

Reply: We have amended the explanation in the methods section: "...mimicking the exposure of continental shelves under LGM conditions. This results in reduced benthic denitrification (Somes, Andreas Schmittner, et al., 2017), reduced sedimentary input of Fe (Alessandro Tagliabue, Bopp, et al., 2010; Juan Muglia, Somes, et al., 2017), ...

L. 120-121: "With each of the 24 conditions, we restarted the 20 simulations [...] from the calibration state" - This sentence is not clear. It would be beneficial to tie it better to what was actually done in the calibration stage.

Reply: We thank the referee pointing out the unclear method description. We agree that the connection to the calibration stage was not sufficiently clear. In the revised manuscript, we now explicitly state that the selected 20 parameter sets originate from the calibration stage and correspond to fully spun-up pre-industrial simulations that best reproduce present-day observations. Each of these simulations is then used as the initial condition for the 24 combinations of physical and biogeochemical boundary conditions. The new sentences are "From the calibration stage, we selected the 20 parameter sets that best reproduce present-day observations, each corresponding to a fully spun-up pre-industrial simulation. These simulations serve as initial conditions for the 24 combinations of physical and biogeochemical boundary conditions." We also added a flow chart for a better description of our model calibration and ensemble experiment stages.

L. 121: "The spin-up were performed with fix the radiative forcing [...] but let the atmospheric pCO2 evolve freely" – The beginning of the sentence (before the part that I left out here) reads strangely, so it is a bit difficult to work out what it means, but I think you are saying that the feedback between atmospheric CO2 and radiative forcing is switched off. Please clarify this.

Reply: We agree that the original wording was unclear. What we intended to describe is that the feedback between atmospheric CO_2 and radiative forcing is switched off during the spin-up. We have revised the sentence as "For each of the 24 configurations, we restarted simulations using the 20 selected parameter sets, resulting in an ensemble of

480 simulations. During the spin-up, radiative forcing was prescribed according to a fixed pCO_2 level (284.3 ppm for PI and 190 ppm for LGM), i.e., the feedback between atmospheric CO_2 and radiative forcing was switched off, while atmospheric pCO_2 was allowed to evolve freely in response to changes in the global carbon cycle."

L 125: "All simulations were spun up [...]" – Please, specify again which simulations you are referring to here. This section gets confusing because it is not described clearly enough how these simulations are tied to the calibration stage. A flow chart might help with this (see General comments).

Reply: We thank the reviewer for pointing this out. As addressed above, we have added clarifications to better link these simulations to the calibration stage. We believe that the added flow chart now clearly illustrates the workflow.

Table 2: Why is there no experiment LGMatmMD, with LGM moisture diffusivity and PI wind stress?

Reply: The combination (LGM moisture diffusivity and PI wind stress) resulted a shut-down of the AMOC and we did not include it. We have added the information "The combination of LGM moisture diffusivity and PI wind stress resulted a shut-down of the AMOC in the UVic_ESCM and hence is not discussed here."

Results

L. 163: "surface air temperature in LGMatmctl-Piallbgc is 4.3°C lower than in Pictl-Piallbgc" – it should be noted that this is likely not the full extent of glacial cooling c.f. studies based on proxy records.

Reply: We thank the reviewer for this important comment. We agree that the simulated global cooling of 4.3°C is likely lower than proxy-based estimates. We have revised the manuscript to acknowledge this limitation and added references to proxy-based reconstructions indicating a larger cooling of approximately 5–6°C (Tierney et al., 2020; Seltzer et al., 2021). This discrepancy is consistent with known limitations of intermediate-complexity models and does not affect the relative differences analysed in this study. We have revised the sentences as "The simulated global surface air temperature in LGMatmctl_PIallbgc is 4.3°C lower than in PIctl_PIallbgc. Although studies based on proxy records indicate a larger cooling of approximately 5–6 °C (Tierney et al., 2020; Seltzer et al., 2021), the discrepancy is consistent with known limitations of intermediate-complexity models and does not affect the relative differences analysed in this study."

L. 167-168: "The LGM wind stress intensifies and deepens the AMOC, while the reduced moisture diffusivity across the Southern Ocean makes it weaker and shallower (Somes et al. 2017)" – It would be beneficial to briefly explain why the LGM wind stress and moisture diffusivity have these opposing effects on the overturning.

Reply: We thank the reviewer for this helpful suggestion. We added "The UVic-ESCM responds to LGM wind stress changes with an increase in northward salt transport in the North Atlantic, which enhances surface salinity and density, thereby strengthening North Atlantic Deep Water formation and intensifying and deepening the AMOC. In contrast, reduced atmospheric moisture diffusivity decreases meridional freshwater transport to the Southern Ocean, leading to changes in surface buoyancy and enhanced vertical stratification. This suppresses deep water formation, particularly Antarctic Bottom Water, and weakens the deep return flow, resulting in a weaker and shallower global overturning circulation, including the AMOC (Sigman et al., 2007; Somes et al., 2017)."

Figure 3. This figure is difficult to decode for several reasons. First, it lacks labels for what the upper and lower row show. Second, it is very difficult to see the coloured lines in the top row. Ideally use a lighter colour (not bold black) for the many model lines. Also, it is not advisable to use red and green in the same panel, to avoid issues for colour vision impaired readers. The figure lacks panel letters.

Reply: We agree with the points. We have revised Fig. 3 according to the suggestions.

L. 169-170: It would be good to put these numbers into context with some proxy record results, despite them having

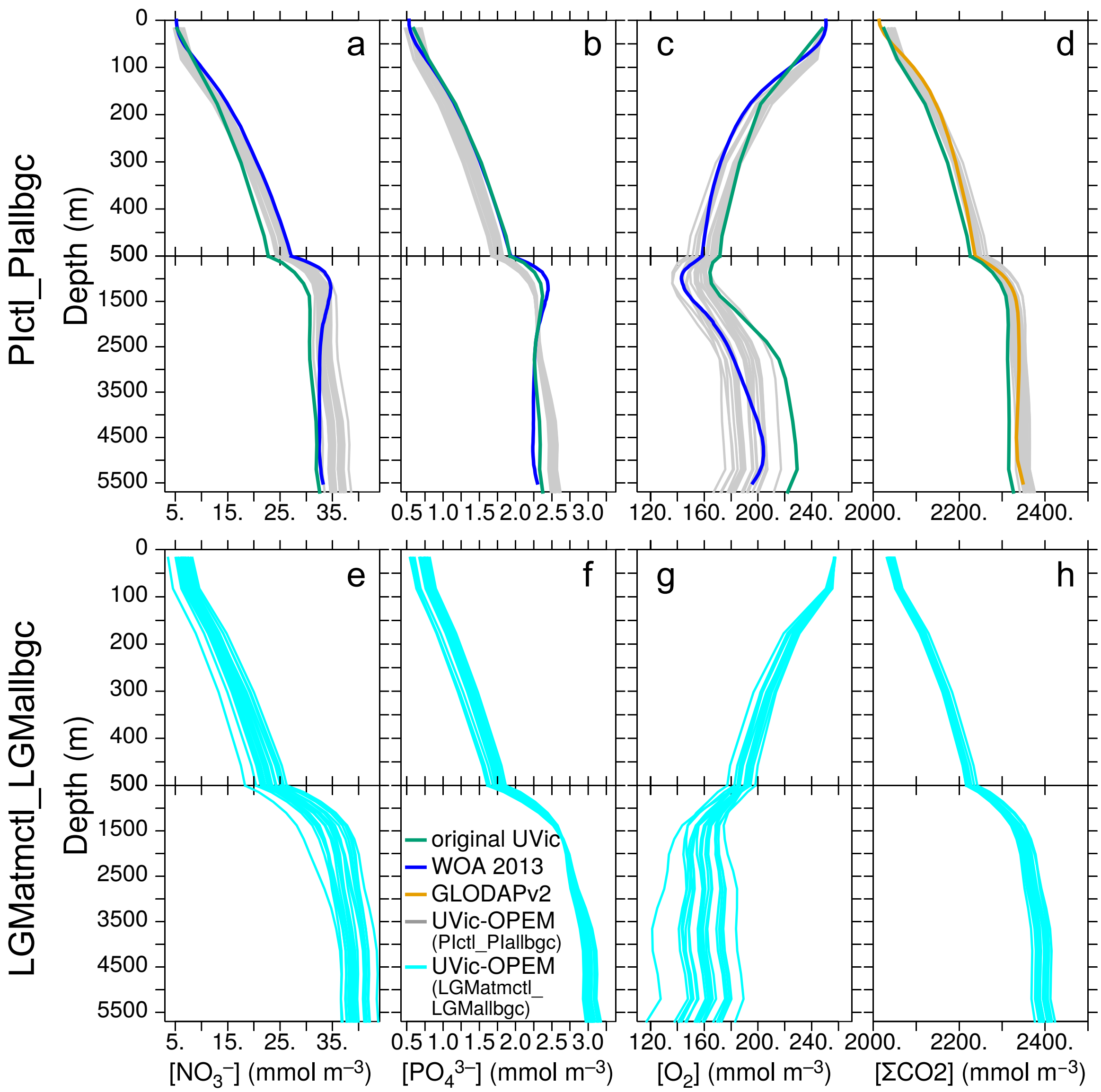

Figure 3: This one will be Fig. 4.

uncertainties of their own.

Reply: We added a reconstruction of the AMOC strength using multi-proxy constraints, which shows that the AMOC was ≈ 36% weaker during the LGM. We added the information: "The strength and depth of the maximum AMOC at 26.5°N in LGMatmctl-PIallbgc, LGMatmws-PIallbgc, and LGMatmpi-PIallbgc are 12.66 ± 0.01 Sv at 1400 m, 17.33 ± 0.04 Sv at 1700 m, and 14.16 ± 0.03 Sv at 1100 m, respectively (Fig. 5). In LGMatmctl_PIallbgc, the simulated in AMOC strength is reduced by 29% relative to PIctl-PIallbgc, which is comparable to multi-proxy reconstructions suggesting an approximately 36% weaker AMOC during the LGM (11.2 Sv, Pöppelmeier et al., 2023).".

L. 200-201: It currently says that 31.4 mmol/m3 is lower than 22.1 mmol/m3, but based on the figure, it seems like the numbers for *-LGMFesed and *-PIallbgc have been switched in the text. Hence, the *-LGMFesed concentration is indeed lower, it should just have a different value.

Reply: Thank you for pointing this out. The values for *_LGMFesed and *_PIallbgc were mistakenly switched in the original draft. We have corrected this in the text.

Figure 6: Why are the concentrations given in µmol/m3 when the text uses nmol/m3? Consistency would be helpful. The information in this figure is essentially repeated in Fig. 7 a-h, 8a-h. Could this one be entirely moved to supplementary to reduce the number of information density?

Reply: We have change the $\mu mol\, m^{-3}$ to $nmol\, m^{-3}$ in the figure for consistency. We also moved the figure to the supplementary now.

Figure 7: The information density is overwhelming. Also, it is not clear to me what the asterisks represent. This needs to be clarified in the caption, or removed entirely.

Reply: We thank the reviewer for this comment. The asterisks originally indicated statistically significant differences between the groups being compared (P-value < 0.05, Student's t-test). However, we agree that this adds unnecessary visual complexity, especially since these differences are already discussed in the text. We have therefore removed the asterisks from the figure. We consider the remaining information important for interpreting the results and have retained the figure panels accordingly.

L. 241-243: "In the LGMPO4 simulations [...]" - This simulation is not the first in the subpanel, so why start with talking about this one specifically? It means the reader cannot follow the information in the subpanel chronologically, but has to search even more for the information. This is just an example of how you can facilitate for the reader in handling this enormous amount of information. This sentence is also confusing because I think it is referring to some information that is given by the asterisks in the figure, but this is not clearly stated in the text, so I am not entirely sure.

Reply: We thank the reviewer for this helpful comment. We agree that the presentation order in the original text may not have been intuitive when compared to the layout of the figure. Our intention was to structure the discussion by biogeochemical variable rather than by the order of panels in the figure. In this subsection, we focus specifically on surface PO_4^{3-}, and therefore begin with the $LGMPO_4$ simulations, which directly represent changes in phosphate boundary conditions. To improve clarity, we have revised the text to better guide the reader and avoid confusion with the figure layout. In addition, we have removed the asterisks from the figure, as suggested, which also eliminates ambiguity regarding their interpretation. The additional description is "Since the global PO_4^{3-} inventory is fixed in the model, the higher PO_4^{3-} levels in the $LGMPO_4$ configuration directly translate into higher surface concentrations. In the $*\_LGMPO_4$ simulations, ...". The $*\_$ stands for all four physical conditions combined with the biogeochemical condition, we add this information when it first appears: "In the $*$_LGMFedep (All four physical conditions combined with LGMFedep) configurations ..."

L. 244: "between sPO4 and its global inventory" – NO3 inventory is shown in Figure 7 but not PO4 inventory, and the same for Table S1, so there is no way to check this statement. I am not saying you should add the PO4 inventory to Fig. 7, because it already has too many panels, but I do think you should add it to Table S1. Also, I am wondering why it was not treated the same as the NO3 inventory.

Reply: We thank the reviewer for this helpful comment. In our model setup, the global PO_4^{3-} inventory is conserved, and therefore does not vary across simulations in the same way as NO_3^-, which is affected by processes such as N_2 fixation and denitrification. For this reason, we did not include the PO_4^{3-} inventory in Fig. 7 or Table S1. To clarify this point, we have revised the manuscript to explicitly state that the PO_4^{3-} inventory is fixed in the model, and therefore the differences in surface PO_4^{3-} reflect redistribution rather than changes in the total inventory. The revised sentence is "...indicating a non-linear relationship between $sPO_4{}^{3-}$ and its global distribution, as the total $PO_4{}^{3-}$ inventory is fixed in the model ...". We also explicitly mention that the inventory is conserved in the model in the boundary conditions section.

L. 256 : " consistent with reduced water column denitrification and benthic denitrification" – My first instinct would have been to think about reduced remineralisation of soft tissue carbon consuming less O2, but this is not mentioned. Is this not what is happening?

Reply: Yes the reduced remineralisation of soft tissue carbon consuming less O_2 does happen. We included the information and revised the paragraph: "The ∗-LGMFesed configurations, which feature reduced iron input, result in a remarkable increase of $62\,\mathrm{mmol\,m^{-3}}$ in O_2 concentrations (Fig. 7f). This increase is primarily driven by lower oxygen consumption due to the reduced remineralisation of particulate organic carbon (POC), a shift that is also consistent with decreased water column and benthic denitrification resulting from much lower NPP.".

L. 260-261: " PO4 and NO3 availability alone have limited effects on the O2 inventory" – So, they are not controlling NPP and POC export, and are thus not the main limiting nutrients, but Fe is? This is the overall impression I get from the importance of Fe in these results, that the biogeochemistry in the model is predominantly limited by Fe thanks to the flexible stoichiometry for the macronutrients. This is somewhat discussed later in the manuscript, but could be emphasised more, as it is an interesting result which raises thoughts about how nutrient limitation works in the real ocean.

Reply: We thank the referee for the constructive comment. To emphasise the importance of Fe, in the discussion section we added some paragraphs such as "As a consequence, macronutrient availability alone exerts only a limited control on export production and associated oxygen consumption in our simulations. Instead, iron availability emerges as a more influential limiting factor in many regions, particularly where Fe supply constrains productivity. Changes in Fe supply therefore have a stronger impact on particulate organic carbon export, remineralisation, and ultimately the O_2 inventory." We also link this to Fe fertilisation as a potential CDR method that we evaluated in this study in the discussion section.

L. 262-265: I would expect the depletion of O2 in the LGM deep water to be tied to a weaker overturning and thus a longer residence time leading to less ventilation and more O2 consumption, while the upper ocean concentrations would be higher due to higher solubility in colder conditions. Is this what you observe? See also my comment for L. 278-279

Reply: Yes that is what we observed, as shown in Fig. 3g, O_2 concentration was higher in the upper ocean but lower in the deep water. We have included the information and revised the sentences as "Among the LGM physical configurations, O_2 levels are higher in the upper ocean due to a higher solubility under colder conditions, but are lower in the deep water due to a more sluggish circulation (Fig. 3), and the globally-averaged concentrations generally are lower...".

L. 278-279: "DIC concentrations [...] are similar, despite different strength of the overturning circulation" – What do you mean by overturning circulation here? Just the AMOC strength, or are you also assessing the strength of the AABW cell? Both are important for the storage of carbon, and just the AMOC strength is not enough. The overall residence time of the deep ocean will need to be considered in some way.

Reply: We thank the reviewer for this important comment. We agree that changes in ocean overturning circulation cannot be fully characterised by AMOC strength alone, and that the overall deep ocean residence time, including contributions from Antarctic Bottom Water (AABW), plays a key role in regulating carbon storage. Nevertheless, the AMOC showed the most prominent changes from pre-industrial to the LGM conditions in our study, and we focused primarily on differences in AMOC strength as a diagnostic of circulation changes. We acknowledge that this provides only a partial view of the overturning circulation. In the original manuscript we only compared the global average DIC concentrations and found the values are similar among the LGM simulations. We added a supplement figure (Fig. S2) for the comparison of DIC profiles, and we found the concentration in the deep water are indeed different with different circulations, and it is the highest with the LGMatmctl condition, which has the most sluggish AMOC among the four conditions we tested. We have revised the manuscript to clarify that our results do not imply that overturning circulation changes are unimportant, but rather that the relationship between circulation strength and DIC storage is not straightforward and likely involves additional factors such as circulation geometry and deep ocean residence time. We have added a paragraph: "While global average DIC concentrations appear similar across the LGM physical configurations, the vertical distribution tells a more complex story (Fig. S2). Specifically, deep-water DIC concentrations vary with the circulation state; the LGMctl configuration, which exhibits the most sluggish AMOC in our ensemble, shows the highest deep-water DIC inventory. TThis suggests that the relationship between circulation strength and DIC storage is not straightforward and likely involves additional factors such as circulation geometry and deep ocean residence time."

L. 291-294: It should be noted that these values are far from actual LGM pCO2

Reply: We added "It is important to note that these simulated values remain significantly higher than the actual LGM atmospheric pCO_2 of approximately 190 ppm recorded in ice cores (Monnin et al., 2001)".

L. 304: "changes in atmospheric carbon account for" – Since all the atmospheric values are of course given in ppm, it would be helpful to know what conversion factor you use to get PgC.

Reply: In UVic, 1 ppm CO_2 equals 2.123 PgC. We added this factor as "...changes in atmospheric carbon (1 ppm = 2.123 PgC) account for ..."

L. 315-316: "Switching to LGM moisture diffusivity and wind pattern has no noticeable effect on NPP." – It looks to me as if they are of about the same order as the effects of LGMPO4 and LGMdeni, in that case, could also considered negligible, or am I missing something?

Reply: The original sentence was trying to say that the LGM wind and moisture diffusivity forcings have much weaker effects on NPP than the lower temperature does. We rewrite the sentence and explicitly mention their effects on the NPP as "Compared with the effect of lower temperature, switching to LGM moisture diffusivity and wind patterns has relatively small effects on NPP. Compared with LGMatmpi-∗, NPP increases by $1.3\,\mathrm{Pg\,C\,yr^{-1}}$ (3 %) and $0.4\,\mathrm{Pg\,C\,yr^{-1}}$ (1 %) in LGMatmws-∗ and LGMatmctl-∗ simulations, respectively."

L. 320-321: "The reduction in POC export due to a cooler climate [...]" - How does this compare to proxy records?

Reply: We thank the reviewer for this suggestion. Since the boundary conditions such as the supply of Fe was likely very different, it is difficult to isolate the effect of a cooler climate in proxy records. Nevertheless, proxy-based reconstructions indicate that changes in particulate organic carbon (POC) export during the LGM were spatially heterogeneous, with decreases in some low-latitude regions and increases in regions influenced by enhanced iron supply, such as the Southern Ocean (e.g., Cartapanis et al., 2011; Kofeld et al., 2005; Pichevin et al., 2009; Toyos et al., 2022). We incorporated the information into this paragraph as: "...The reduction in POC export due to a cooler climate is $0.5\,\mathrm{Pg\,C\,yr^{-1}}$ (5 %) among the LGM physical configurations. This temperature-driven reduction reflects the close coupling between global NPP and export production in the model, while regional changes in iron supply and nutrient utilisation can modulate export production. Proxy-based studies indicate that changes in export production during the LGM were spatially heterogeneous with decreases in some low-latitude regions but increases in areas affected by enhanced iron supply, such as the Southern Ocean (Cartapanis et al., 2018; Kohfeld et al., 2005; Toyos et al., 2022).

L. 333-334: "and WC denitrification shuts down entirely." – Do you know why this happens? Also, please refer to Fig. 6 where this is visible (even if this moves to the supplementary). This is one of the few times where I felt like I really needed Fig. 6 and was surprised it was not used.

Reply: The WC denitrification shuts down entirely in *_Fesed simulation because the strong decline in the productivity and the consumption of oxygen, leading to the disappearance of oxygen deficient zones where WC denitrification occurs in the model. We added information of the panel in Fig. 6 as "...and WC denitrification (WC N-loss) shuts down entirely (Fig. 6l)."

Figure 8: The information density is overwhelming. The right-hand column is not referenced in the manuscript, so it can be deleted. Also, it is not clear to me what the asterisks represent. This needs to be clarified in the caption, or removed entirely.

Reply: We agree the information density is high, and we removed the right-hand column and the asterisks as suggested.

L. 358-362: These sentences are an example of something that could overall be improved in the text. They are difficult to read because decreases and increases are compared to each other in an unnecessarily confusing way. As an example, it is clearer if you say "the first increases by 31% while the second increases by 62%" instead of saying "this concentration is 31% higher, which is smaller than the 62% increase in the other concentration". Saying that something is higher, while it is also smaller is not ideal. Try to improve this overall in the manuscript.

Reply: We thank the reviewer for this helpful comment. We have revised the sentences to improve clarity and ensure consistent comparisons: "Compared with $*$_PIallbgc, the average sPO_4^{3-} increases by 31% in $*$_LGMPO$_4$and by 62% in $*$_LGMFesed. Moreover, the effects of Fe limitation on carbon fixation and nitrogen assimilation are weaker in $*$_LGMPO$_4$ than in $*$_LGMFesed. As a result, pC:P and pN:P decrease by 10% and 6%, respectively, in $*$_LGMPO$_4$, which is substantially smaller than the reductions found in $*$_LGMFesed.". We have also revised similar phrasing throughout the manuscript to improve readability.

Discussion

L. 379-380: "One might argue that the sedimentary input is overestimated [...]" - In the following sentences, you establish that the estimate is rather quite conservative, so why would one argue that it is overestimated?

Reply: We thank the reviewer for this comment and for pointing out that the original phrasing could be misleading. Our intention was not to suggest that the sedimentary Fe input is overestimated in our simulations, but rather to acknowledge that, in the absence of context, readers might perceive the imposed reduction as large. We therefore aimed to provide a benchmark by comparing our simulated sedimentary Fe flux to the range reported in other models. We have revised the text to clarify that our estimate lies near the lower end of published values and is therefore relatively conservative, while still retaining this contextual explanation.
The revised sentences are "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 Gmol Fe yr^{-1}) falls close to the lower end of estimates from other models (0.6 –194 Gmol Fe yr^{-1}, Alessandro Tagliabue, Aumont, et al., 2016). A recent sensitivity study also shows that a much higher present-day sedimentary Fe release (117 Gmol Fe yr^{-1}) than assumed previously (15 Gmol Fe yr^{-1}) yields better model-data misfit in global and surface dFe distribution (Somes, Dale, et al., 2021). This indicates that our representation is relatively conservative, and does not impose an unrealistically large reduction in sedimentary Fe supply."

L. 385-390: I find this argumentation somewhat flawed. Low NPP and POC export can still lead to increased ocean carbon storage if the deep ocean residence time is increased. This does not necessarily have anything to do with what happens in the spin-up phase.

Reply: The boundary conditions compared here are pre-industrial and LGM biogeochemical configurations and therefore do not involve changes in ocean circulation. However, we recognise that our original argumentation was not sufficiently accurate. 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 resulting lower atmospheric pCO_2 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 CO_2 fluxes before equilibrium is re-established. To clarify this point, we have added a figure (Fig. S4) showing the air–sea CO_2 flux in PIctl_LGMallbgc, PIctl_PIallbgc, and their difference.

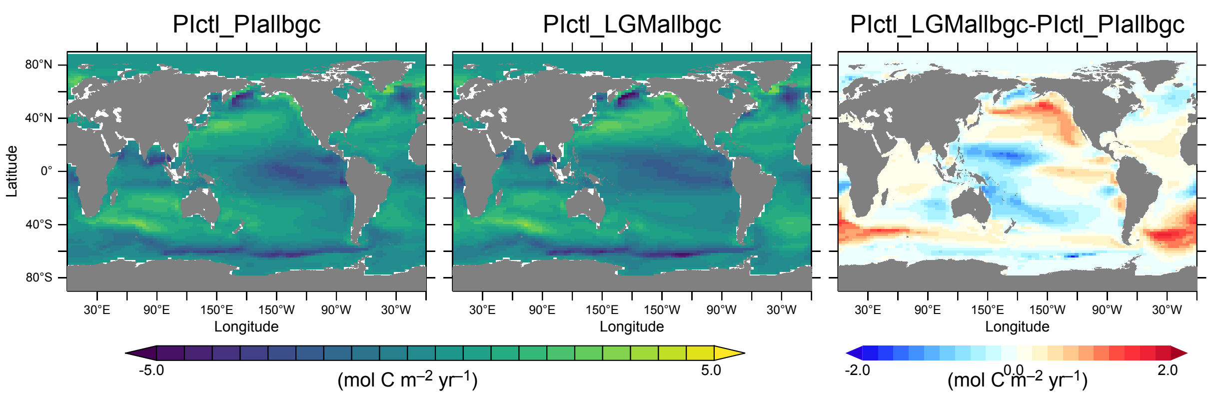

Figure S4: Air-sea CO_2 flux (positive downwards) in PIctl_PIallbgc (a), PIctl_LGMallbgc (b), and the difference (c).

L. 391-400: On the contrary, I find this part of the subsection great. Nicely explained, and very interesting result that deserves more attention.

Reply: We thank the reviewer for this very positive comment. We have extended the discussion regarding the effects of the variable stoichiometry and macronutrient co-limitation on ocean iron fertilisation as a marine CDR method. In the section we added "A stronger Fe supply from atmospheric deposition, such as in the LGMFedep condition, could have enhanced the utilisation of ${PO_4}^{3-}$ and ${NO_3}^{-}$, leading to an expansion of the ocean regions depleted in those major nutrients. This 'nutrient robbing' effect, whereby stimulated productivity in one area depletes macronutrients available for downstream regions, is a primary criticism of Ocean Iron Fertilisation (OIF) as a marine CDR strategy, as it could limit the long-term global net carbon sequestration. Nevertheless, our results suggest that the variable stoichiometry implemented in our model may partially mitigate this effect. By increasing pC:P and pC:N in response to nutrient stress, the model sustains NPP even when macronutrient concentrations decline. This negative feedback mechanism does not exist in models that assume fixed stoichiometry, and warrants further investigation to determine its global significance in CDR scenarios."

L. 410-411: Some of the studies you have previously mentioned in the paper also do this. You should refer to them again here. As a general note, no other literature is discussed in this subsection 4.2 at all. It mostly feels like a repetition of results, with little actual discussion.

Reply: We thank the reviewer for this helpful comment. We agree that the original version of Section 4.2 focused too strongly on describing model results and did not sufficiently place them in the context of previous studies. In the revised manuscript, we have added references to relevant literature (e.g., Somes et al., 2017, and other studies discussed earlier) and expanded the discussion to better interpret the interactions between physical and biogeochemical boundary conditions. The revised paragraph is "Lower temperatures, different wind patterns, and reduced moisture diffusivity over the Southern Ocean in the three LGM physical configurations result in different general circulation patterns, which affect biogeochemical cycles, as demonstrated in previous modelling studies (Somes, Andreas Schmittner, et al., 2017; Juan Muglia and Andreas Schmittner, 2015). The LGM boundary conditions affect inventories and fluxes differently. For example, DIC are $14.7\,\mathrm{mmol\,m^{-3}}$ higher for LGMatmpi-$*$ than for PIctl-$*$, while the differences between LGMatmws-$*$ and LGMatmpi-$*$, and between LGMatmctl-$*$ and LGMatmpi-$*$ are only $-0.3\,\mathrm{mmol\,m^{-3}}$ and $0.8\,\mathrm{mmol\,m^{-3}}$, respectively (Fig. 7o). On the other hand, N_2 fixation shows contrasting responses across the simulations. Relative to PIctl-$*$, it decreases by $19\,\mathrm{Tg\,N\,yr^{-1}}$ in LGMatmpi-$*$, but increases by 20 and $18\,\mathrm{Tg\,N\,yr^{-1}}$ in LGMatmws-$*$ and LGMatmctl-$*$, respectively. This indicates that, in our experiments, the glacial-interglacial changes in DIC inventory are dominated by the lower temperature, while the N_2 fixation is also sensitive to the changes in the physical conditions. Across all biogeochemical conditions, our LGM configurations have lower pCO_2 by -35, -21, and -19 ppm in LGMatmctl-$*$, LGMatmws-$*$, and LGMatmpi-$*$, respectively. Thus, the LGM wind pattern and moisture diffusivity over the Southern Ocean contribute about 16 ppm, similar to the 19 ppm drawdown of pCO_2 owing to the lower temperature. We also notice that the effects of the biogeochemical configurations depend on which physical configuration they are combined with and vice versa. For example, pCO_2 decreases by 0.8 ppm from PIctl-PIallbgc to PIctl-LGMallbgc, by 9.3 ppm from LGMatmctl-PIallbgc to LGMatmctl-LGMallbgc, by 34.2 ppm from PIctl-PIallbgc to LGMatmctl-PIallbgc, and by 42.7 ppm from PIctl-LGMallbgc to LGMatmctl-LGMallbgc. This non-linear interaction between physical circulation and biogeochemical processes is consistent with previous studies showing that glacial pCO_2 drawdown arises from the combined and often synergistic effects of ocean circulation changes and biological processes such as Fe fertilisation (A. Tagliabue et al., 2009; Buchanan et al., 2016)."

L. 436: "an increase in pCO2 due to reduced primary production and higher O2" – Also inconsistent with proxy records in the same way as the NO3 decrease described on L. 433.

Reply: We thank the reviewer for pointing this out. We added a sentence "This increase in O_2 is also inconsistent with proxy-based reconstructions." to explicitly acknowledge this.

L. 459-460: "Further, different parameter combinations also affect the changes in pCO2" – As the different parameter combinations are supposedly a big part of the motivation for the study, I feel like they do not quite get the attention they deserve in the discussion. They are barely mentioned in passing here.

Reply: We thank the reviewer for this important comment. We agree that the role of parameter variations is a key aspect of this study and deserves stronger emphasis in the discussion. While we included a dedicated subsection on the effects of parameter variations, we acknowledge that their broader implications were not sufficiently highlighted or connected to the main findings. To clarify the importance of parameter choices alongside physical and biogeochemical boundary conditions, we added "Parameter uncertainty is a key source of variability in simulated LGM carbon cycle responses and constitutes a central motivation of this study. This variability arises because the response of biogeochemical tracers and fluxes to changes in boundary conditions depends strongly on the underlying parameter choices." in the beginning of the subsection 4.5.

L. 471: "from PIctl-PIallbgc to LGMatmctl-LGMFedep" – It is not clear to me why you choose LGMatmctl-LGMFedep specifically for the comparison here.

Reply: We thank the reviewer for pointing out that this choice was not sufficiently explained. The studies cited here primarily investigate the effect of enhanced atmospheric iron deposition during the LGM, without accounting for the concurrent reduction in sedimentary Fe supply and changes in macronutrient supplies, which could have impacts on the elemental ratios of POM. To enable a more direct comparison with these studies, we therefore focus on the LGMatmctl_LGMFedep configuration, which isolates the effect of increased atmospheric Fe deposition while keeping other biogeochemical boundary conditions at their pre-industrial values. We revised the sentence to "To facilitate comparison with studies neglecting the changes in sedimentary Fe flux, benthic N-loss, and PO_4^{3-} inventory, we calculate the changes in pC:N (26%) and pC:P (28%) from PIctl_PIallbgc to LGMatmctl_LGMFedep, which isolates the effect of enhanced atmospheric Fe deposition in our simulations. Multiplying the relative changes by the total pCO_2 decline yields an estimated additional drawdown of approximately 16–17 ppm attributable to variable stoichiometry."

L. 477-482: In the elemental rations, it may be that regional changes, e.g. in the Southern Ocean, are more important than the global average changes.

Reply: We thank the reviewer for this insightful comment. We agree that regional variations can play a role in controlling elemental ratios and their impact on the carbon cycle. In this section, we focus on global mean changes to provide an overall assessment of the system response. However, we acknowledge that regional changes may be more pronounced and influential locally. We have added: "We note, however, that regional variations, particularly in regions such as the Southern Ocean, may differ from the global mean response and could play a more important role locally." at the end of the subsection.

L. 500-502: "The variation in the difference [...] in each model simulation. – The meaning of this sentence is unclear.

Reply: We thank the reviewer for pointing out that this sentence was unclear. We have revised it to more clearly describe that the spread in biogeochemical responses across the ensemble arises from differences in how the selected parameter sets affect model sensitivity under different LGM boundary conditions as "The spread in simulated changes relative to pre-industrial conditions (PIctl, PIallbgc, and PIctl_PIallbgc) across the 20 ensemble members arises from differences in how the selected parameter sets control model sensitivity to LGM boundary conditions in each configuration.".

L. 502: "cost values" – It would be helpful for the reader if you remind them what this is.

Reply: We revised the sentence and offer information about the cost values as "Interestingly, the ΔpCO_2 appears unrelated to cost values derived based on the likelihood-based cost function we employed for the parameter selection (Fig. 10t)."

L. 519: " We find that persistent changes in Fe supply are the most critical factor" – One major reason for this is that this model, compared to many others, has flexible stoichiometric ratios, as has been previously established and is also discussed in the lines below, but do you think that it could be model dependent in any other way? I am just asking out of curiosity. You do not need to add this to the manuscript per se.

Reply: We thank the reviewer for this thoughtful comment. We agree that, in addition to the flexible stoichiometry, other aspects of the model formulation may contribute to the prominent role of Fe in our simulations. These include the representation of iron cycling, the parameterisation of co-limitation, and the coupling between nutrient limitation and export production. In particular, the relatively strong sensitivity of productivity to Fe supply in our model may reflect both the implemented co-limitation framework and the absence of additional processes such as more complex ligand dynamics. We therefore acknowledge that the magnitude of Fe dominance found here is likely model-dependent, although the qualitative importance of iron as a limiting nutrient is consistent with previous studies. While a full assessment of structural model uncertainty is beyond the scope of this study, we agree that this is an important avenue for future work.

L. 524-525: " Due to the decline in sedimentary Fe input, surface [macronutrients] are higher in the LGM than the pre-industrial simulations" – This connection needs to be briefly explained. I suggest "Due to the decline in sedimentary Fe input, productivity decreases and leads to higher surface macronutrient concentrations" or similar.

Reply: We thank the referee for the suggestion, and revised the sentence as "Due to the decline in sedimentary Fe input, productivity decreases and leads to higher surface NO_3^- and PO_4^{3-} in the LGM than the pre-industrial simulations. "

minor comments

L. 183 "has a great impact" – how large?

Reply: We have added the magnitude of the impact, now the sentence is "...the lower Fe availability has a strong impact on productivity (-44 %) and export of particulate organic carbon (POC, -39 %)."

L. 212: "Whenever NO3 concentrations are lower" – lower compared to what?

Reply: We have added the information, now the sentence is "Whenever NO_3^- concentrations are lower in a simulation in ∗-LGMFedep than in ∗-PIallbgc, ..."

L. 214-215 "the other limiting nutrient, PO4" – the other limiting macronutrient, PO4

Reply: Corrected.

L. 249: "changes in NO3 cycling" – Shouldn't this be PO4?

Reply: Thank you for this comment. Here, "changes in NO_3^- cycling" refers to the ∗-LGMbdeni simulations, in which benthic denitrification alters the nitrogen cycle. We agree that this was not sufficiently clear in the original text. We have revised the sentence as "These results indicate that Fe supply is a critical factor that affects sPO_4^{3-}, whereas changes in NO_3^- cycling through altered benthic denitrification in the ∗-LGMbdeni simulations alone have relatively minor effects."

L. 296: "maximum and minimum pCO2 is largest" – This should say pCO2 anomaly, which is also what the figure shows

Reply: Thank you for this comment. We apologise for the confusion caused by the incorrect figure reference and unclear wording. The values discussed here refer to the spread in absolute pCO_2 across simulations (i.e., the difference between maximum and minimum values), rather than pCO_2 anomalies. We have revised the text and corrected the figure reference to clarify this point. "...The spread in pCO_2 (i.e., the difference between the maximum and minimum values across simulations) is largest for ∗-LGMFesed, ranging from 26 ppm in LGMatmctl-LGMFesed to 31 ppm in PIctl-LGMFesed (Fig. 7h)."

L. 298-299: Note in the text that the reduction in terrestrial carbon and the differences in surface temperature distributions are not shown in the manuscript.

Reply: We have added the information and now the sentences are "While all LGM physical configurations lead to reduced terrestrial carbon (2,700 PgC, 35% lower than in PIctl condition) due to changes in radiative forcing, the three LGM atmospheric configurations result in different surface temperature distributions (Fig. S3), which in turn affect the amounts of carbon released to the atmosphere and ocean."

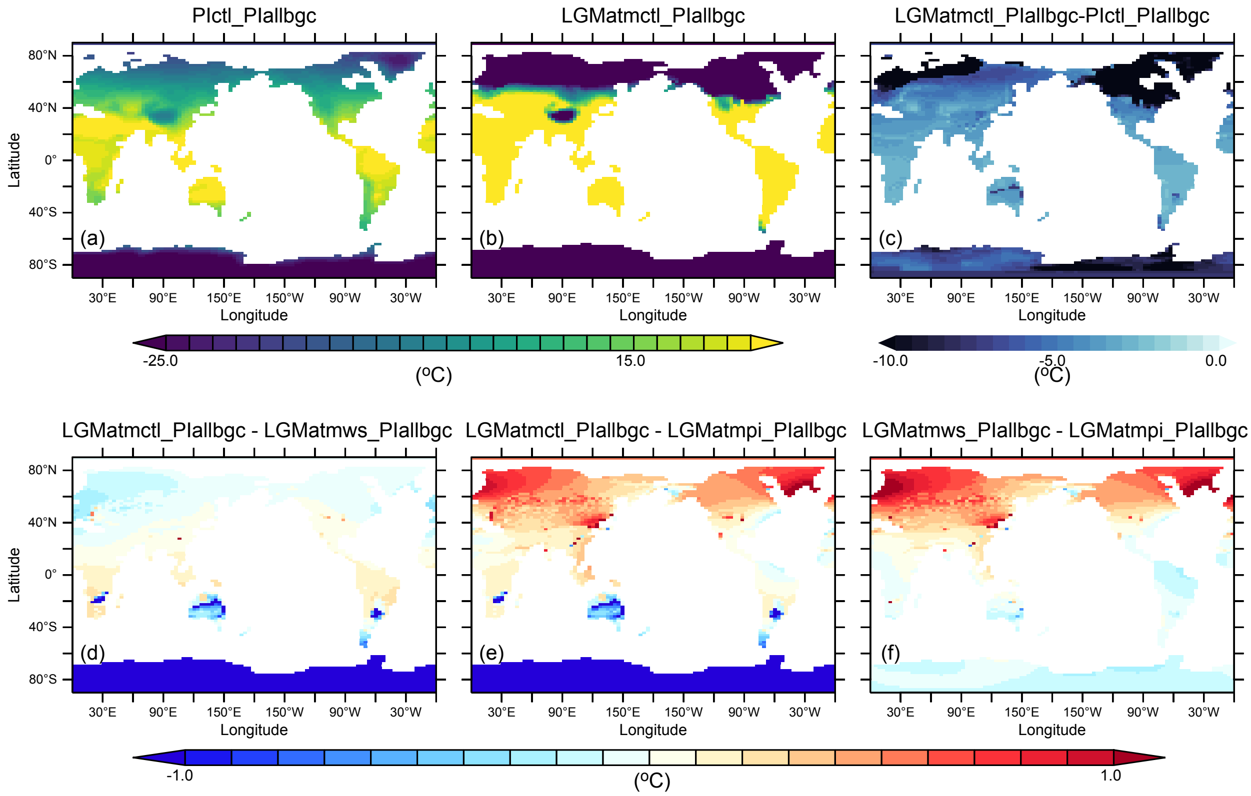

Figure S3: Land temperature in PIctl_PIallbgc (a), LGMatmctl_PIallbgc (b), and the difference between LGMatmctl_PIallbgc and PIctl_PIallbgc (c), between LGMatmctl_PIallbgc and LGMatmws_PIallbgc (d), between LGMatmctl_PIallbgc and LGMatmpi_PIallbgc (e), and between LGMatmws_PIallbgc and LGMatmpi_PIallbgc (f).

L. 344: "without biomass wighting in the ocean gird cells" – typos: without biomass weighting in the ocean grid cells

Reply: Corrected.

L. 417: "affected differently" – differently how?

Reply: What we were trying to say is that DIC and N_2 fixation responded to the physical conditions differently. To clarify, we rephrased the paragraph to "On the other hand, N_2 fixation shows contrasting responses across the simulations. Relative to PIctl_∗, it decreases by 19 Tg N yr^{-1} in LGMatmpi_∗, but increases by 20 and 18 Tg N yr^{-1} in LGMatmws_∗ and LGMatmctl_∗, respectively. This indicates that, in our experiments, the glacial-interglacial changes in DIC inventory are dominated by the lower temperature, while the N_2 fixation is also sensitive to the changes in the physical conditions."

L. 465: "the decline in pCO2 increases" – a decline cannot increase, please rephrase!

Reply: We have corrected this, now the sentence is "...the magnitude of the pCO_2 decline increases ..."

L. 470: "leads to an additional drawdown of 20 ppm" – compared to what?

Reply: It is compared to a experiment with fixed C:N:P ratio using the Redfield ratio 106: 16: 1. We have changed the sentence to "...leads to an additional drawdown of 20 ppm compared to a fixed stoichiometry scheme using the Redfield ratio C:N:P = 106:16:1."

L. 511: "also have opposite effects on the ΔpCO2, changes in" –> "also have opposite effects on the ΔpCO2, opposing changes in"

Reply: Corrected.

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