

Review, Chien et al. 2025, egusphere-2025-4907

“Ensemble simulation of the Last Glacial Maximum marine biogeochemistry and atmospheric pCO₂ drawdown due to the soft-tissue biological carbon pump”

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 CO₂ 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. P1ctrl-P1allbgc, 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: P1ctrl_P1allbgc.

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.

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.

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

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.

1. 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.

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

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

2. 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.

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.

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>

L. 105-106: “reduced sedimentary 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.

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.

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.

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).

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

3. 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.

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.

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.

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

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.

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?

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.

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.

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.

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?

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.

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

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.

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

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.

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 be considered negligible, or am I missing something?

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

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.

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.

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.

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?

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.

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

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.

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.

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.

L. 471: “from PI_{ctl}-PI_{allbc} to LGM_{atmctl}-LGM_{Fedep}” – It is not clear to me why you choose LGM_{atmctl}-LGM_{Fedep} specifically for the comparison here.

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.

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

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

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.

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.

Minor comments

L. 183 “has a great impact” – how large?

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

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

L. 249: “changes in NO₃ cycling” – Shouldn’t this be PO₄ ?

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

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.

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

L. 417: “affected differently” – differently how?

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

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

L. 511: “also have opposite effects on the ΔpCO₂, changes in” --> “also have opposite effects on the ΔpCO₂, opposing changes in”