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Climate of the Past Discussions

Interactive comment on "Coupled climate-carbon cycle simulation of the Last Glacial Maximum atmospheric CO_2 decrease using a large ensemble of modern plausible parameter sets" *by* Krista M. S. Kemppinen et al.

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article

1 General comments

This study provides some unique perspectives on the glacial drawdown of atmospheric pCO_2. This problem has been at the forefront of climate research for many decades. So far, many mechanisms have been proposed, but a recipe of changes that is physical and biogeochemically consistent with proxy records and known mechanisms is still elusive. The authors of this study set out to try and achieve this difficult task.

The authors employ a unique set of methods to attack their exploration of what might *plausibly* reduce atmospheric pCO_2 under glacial conditions. The method involves simulating 315 individual parameter sets using the same Earth System Model (ESM) of intermediate complexity in four stages. From what I can tell, each stage involves the full 315-member ensemble, unless numerical instability problems were encountered. Each ensemble member was therefore independent from another at all stages through the study. Each stage was initialised from the final year solution of its previous stage, except stage 1 which I am unsure about what fields have been used for intialisation. The only boundary conditions that were prescribed to the Earth System Model that I can tell were orbital parameters, aeolian iron deposition rates, detrital flux rate to ocean sediments, and ice-sheet fraction and their orography. The 4 stages are as follows:

1. Stage 1 (PI 10,000 years): relaxed pCO_2 to 278 ppmv; no interaction between carbon reservoirs; conserved alkalinity in the ocean.
2. Stage 2 (PI 10,000 years): freely evolving pCO_2; interacting reservoirs; freely evolving ocean alkalinity.
3. Stage 3 (LGM 10,000 years) freely evolving pCO_2; interacting reservoirs; freely evolving ocean alkalinity; ice-sheet growth and corresponding sea level loss

years 0-1000.

4. Stage 4 (LGM 10,000 years) freely evolving pCO_2; interacting reservoirs; freely evolving ocean alkalinity.

Following on from this methodology, the authors lead the reader through the results in a methodical manner, taking on changes in some key environmental variables. In each address of a variable, the authors undergo a detailed assessment of PI versus LGM changes in their model and in proxy records. This is done both in a global sense and a regional sense, sometimes being highly specific. The ground-truthing and comparison to observations and other studies is commendable. It one of the largest comparisons that I have come across in a modelling paper. However, the sheer size of comparison makes the paper unwieldy, and loses the major findings amongst the details. It is also unexpected that the authors concentrate on highly specific regional comparisons because they use an ESM of intermediate complexity that will simply not perform well in many ways. Rather, the authors should focus on the sheer size of their parameter spread and in diagnosing the effect that certain changes have on carbon and climate. I therefore suggest that the authors make an effort to reduce the emphasis on comparison, and focus on how their interesting results might explain the LGM carbon cycle in only a broad sense.

It occurs to me that the authors seem to forget some obvious strengths of their study. Their parameter space is enormous, includes an interactive carbon cycle with carbon-climate feedbacks, as well as many other processes (sea level) that have not been included in other model studies of the LGM. This is of interest to the LGM pCO_2 drop problem. I suggest that the authors try and convey these strengths more clearly in their abstract and the final paragraph of the introduction.

A particularly interesting result of this study is the increase in terrestrial carbon under LGM conditions. The authors more often than not find that LGM conditions

are associated with greater carbon held in soils because (1) ice-sheet growth covers previously fertile regions and traps the carbon in the soil, (2) soil respiration rates decrease more than primary production, and (3) terrestrial carbon in exposed continental shelves due to sea level drop increases. The authors do not simulate point 3, and there is a possible methodological problem with point 1, in that the authors grow the LGM ice sheets from pre-industrial to LGM state in only 1,000 years. It is unclear in the manuscript presented here whether this rapid growth of ice sheets that is *prescribed* overtakes highly productive regions that have not yet evolved to become arid tundra land, which have lower carbon content in the soils. A rapid trapping of soil carbon by the unrealistic growth of the ice sheets (almost 100x faster than in reality) would likely overestimate the terrestrial carbon reservoir at the LGM. The authors state in the methods that "Sensitivity simulations were performed to verify the simulated equilibrium state is insensitive to the choice of timescale of ice-sheet growth", and yet the authors do not present any evidence of this sensitivity. I am therefore sceptical of the validity of this result, which I would say is their most important and interesting contribution to the field.

Finally, the conclusion needs complete re-writing. It reads as a re-stating of the methods and then the results, which is simply not useful for the reader. The authors do discuss carbon isotope changes, which are appropriate records to discuss given the main result of an increase terrestrial carbon reservoir. I strongly suggest that the authors re-assess their strengths and major findings, present them concisely, and provide some comments regarding how a higher terrestrial carbon reservoir and low ocean reservoir could have occurred despite most studies indicating the opposite. If this is possible, then this would be a useful contribution to the field.

Major revisions are needed. If the authors can provide some evidence that changes in the timescale over which the ice-sheets are grown do not play a large role in determining how much carbon is present in the terrestrial reservoir, and the manuscript

is rewritten addressing the points above and below, then I advocate publication.

Some other general points:

- Regarding the figures, I strongly suggest that the authors overlay the red (PGACF-16) over the yellow (PGACF) bars in the histograms. This would reduce a lot of unnecessary replication in the figures. The authors could use a transparency setting to ensure that the red and yellow are easily seen if they have the same number of experiments (frequency).
- The writing is generally okay, but this manuscript definitely requires re-writing in some places. There are too many adjectives and unnecessarily difficult acronyms.
- The presentation of results suffers from the use of opaque acronyms that are easily forgotten after reading the methods section. I strongly suggest that the references to EFPC2, PGACF and PGACF-16 are changed to be more interpretable. PI_{315}, LGM_{104}, LGM_{16}... or equivalent, would be much more helpful.

2 Specific comments

Abstract

The abstract is quite long. The message that this study delivers could be made considerably more poignant if it were condensed for the reader. The authors make some very interesting findings, such as the increase in non-burial carbon in the terrestrail reservoir due to the slow-down in respiration" and "there are 5 different ways to achieve an atmospheric pCO_2 drop". These findings (mostly in para 2) should be

the focus of the abstract, and I advocate for the more technical aspects (para 1) and comparisons with observations (para 3) be removed. In fact, the entire paragraph 3 of the abstract could be reduced to one important sentence without affecting the findings of this study.

Introduction

- Page 3, Line 13 - "dissolve organic carbon inventory" of what? the ocean? soils?
- Page 3, Paragraph 2 - This entire paragraph simply lists the changes that may be assocaited with a glacial ocean. If these mechanisms are to be called upon by the authors, they should be accompanied by at least a brief discussion of why they influence atmospheric pCO_2. For any non-specialist of palaeoceanographic literature specifically relating to the LGM, this paragraph is totally opaque. I would either expand on these mechanisms or remove entirely when accounting for them in the discussion of the results.
- Page 3, Line 19 - "utilises an ensemble of sets of parameters" a bit clunky. What about "uses a large ensemble (471 parameter sets)"?
- Page 3, Line 23 - probably no need to mention Holden (2010a) or cite other literature. Just state philosophy.
- Page 3, Line 26 - move this to methodology. Not necessary here.

Methods

- Table 2 - define acronym SAT in caption. What is EFPC2 and EFPC? Must define these here or introduce the Table later on.

- Page 5, Line 5 - please clarify what a closed biogeochemistry system is. Does it mean no interaction between land-ocean-atmosphere-lithorsphere reservoirs? how are these reservoirs initiated in Stage 1? Using the fields from Holden 2013a?
- Page 7, Lines 1-3 - I don't think it's useful to mention this.

Preindustrial simulations

- Page 7, Line 5 - First of all, it would be helpful to change the title of section 3 to "Results: Preindustrial simulations".
- Page 7, Lines 7-12 - Why present results and talk about EFPC ensemble when the authors only use the EFPC2 315-member ensemble? It seems to be an unnecessary inclusion that confuses the reader, rather than helps understanding. I think given the length of this study that it would be helpful to simply cut any inclusion of EFPC 471-member ensemble and simply present the results of the 315-member ensemble.
- Table 2 - change "31/12" to 31st Dec.
- Table 2 - change "wt% $CaCO_3$" to "wt% ocean $CaCO_3$"
- page 7, Lines 15-23 - Tables 2 and 3 could be moved to supplementary material. Table 2 discussion could be reduced to a single sentence saying that the preindustrial simulations of the 315-member ensemble reproduced all aspects of the Holden 2013a simulations. Table 3 discussion could be reduced to note that there was good agreement with observations of ocean carbon inventory, SSTs and sea ice extent relative to known values.

- Based on what I've said above, I suppose that this section could actually be reduced to one paragraph, or completely removed if the authors wished to use address PI conditions via comparisons with LGM conditions in the next section.

LGM ensemble simulations

- Page 9, Line 7 - "104 ensemble members". Are these presented in yellow in Figure 1? If so, mention it here in the text.
- Page 9, Lines 7-16 - These sentences are confusing for the reader. I understood once reading further on in the paragraph that you do not include these processes in the model, and you are saying that their inclusion would push LGM pCO_2 decrease even further, which justifies your choice of a -30 ppmv threshold to define a successful solution of LGM conditions. However, this is not clear. Please re-write.
- Page 9, Lines 16-26 - This needs to be moved to the methods section. From how I understand your thinking: lines 5-16 justify your choice of -30 ppmv threshold; lines 16-26 justify your methodology in treating the LGM simulations.
- Page 9, Lines 22-25 - The lower threshold of -60 ppmv should belong with your choice of the -30 ppmv upper limit. discuss these together, not separated by other sentences and concepts.
- Page 9, Lines 28-33 - This is a very interesting results. Why can't you define the mechanisms that lead or do not lead to the snowball Earth scenario? Surely if you can define a plausible set of mechanisms need to achieve glacial conditions, you can do the same by comparing the 471 PIs, 16 LGMs, and 47 snowballs??? This would mark a significant contribution to the field.
- Figure 1 - Overlay red bars on the yellow bars to make 1 panel.

- Page 11, Line 11 - define SAT
- Figure 2 - Again, overlay red on yellow to reduce unnecessary replication. Use transparency perhaps to show where yellow and red are both present at the same frequency.
- Page 12, Lines 4-16 - Comparisons with obs not necessary at this detail given the focus of the work and the fact that you use an ESM of intermediate complexity. I would expect a discussion of global temperature changes, with perhaps a little bit of basin-wide, regional discussion **if** those are important points for later on. Please reduce this paragraph and combine with the next.
- Page 15, Line 5 - I'd say there is no relationship.
- Page 15, Line 10 - too many adjectives.
- Figure 4 - overlay red on yellow.
- Page 18, Lines 11-13 - confusing sentence please re-write.
- Page 19, Lines 11-15 - when talking about AABW formation rates, it is better to present this in positive units. Oceanographers are familiar that the units are negative in the calculation of the streamfunction. It is less confusing for the reader to present your changes as negatives if the AABW formation rate declines. THis also removes the need to explain that a positive anomaly is actually a decrease.
- Page 20, Line 2-3 - Don't understand. I thought you said that weaker AMOC and stronger AABW was coincident with the glacial runs, these being PGACF-16? Please make this clearer. WHat are you comparing?
- Page 20, Lines 3-8 - These relationships are made more confusing for the reader because you define AABW formation rates as being stronger when they are negative. It owuld be more helpful if strong = more positive.

- Page 20, Line 15 - Please see study of Yu et al (2014) Deep ocean carbonate chemistry and glacial-interglacial atmospheric CO_2 changes.
- Page 20, final sentence - This has already been covered above? Also, see Ferrari et al (2014) Antarctic sea ice control on ocean circulation in present and glacial climates. PNAS.
- Figure 9 - please ensure that your colour bars are the same scale! I initially thought that your LGM simulation had strong AMOCs, despite the discussion of weaker AMOCs in the text.
- Page 23 - Very interesting result. I think that this is a unique and interesting contribution to the field and should be a focus of this study.
- Table 5 - why does the order change? Please clarify in caption of correct. Also, add the subheadings "Total counts" and "% of total" or their equivalent to the other columns.
- Table 5 - Please quantify the increases and decreases in this table that accompany the scenarios (i.e. mean).
- Page 25, Line 6 - Is table 6 mentioned previously? This introduction of table 6 is jarring.
- Table 6 - Great Table. Could the acronyms for studies you cite be organised into alphabetical order?
- Page 25, Lines 9-10 - I'm (and probably many readers) not sure what "Carbonate compensation of the increased terrestrial carbon storage" means. It's not clear whether you refer to carbon compensation mechanism in the land or the ocean. Do you mean a loss in oceanic DIC due to terrestrial carbon storage, causing an increase in alkalinity that increases $CaCO_3$ burial? If changing terrestrial carbon

reservoir does have a direct effect on ocean alkalinity and $CaCO_3$ burial, maybe by weathering changes you account for, then please explain more fully.

- Page 25, Line 17 - I find this hard to believe.
- Page 25, Line 22 - and yet present day continental shelves that are inundated are also regions of effective carbon burial through marine export production.
- Tables 7 and 8 - Please make "% Total Land" and its relation to Ice-Sheet carbon more clear in the caption. THis takes time to figure out from the reader.
- Tables 7 and 8 - I think that these tables could be combined to solely show the LGM changes.
- Page 27, Lines 1-16 - The relationship between ice-sheet carbon, non-ice sheet carbon and soil burial carbon needs to be made clearer.
- Page 31, Line 7 - re-write sentence please.
- Page 31, Lines 14-15 - Also the relationship with AABW production and decreased AMOC, which you have just discussed.
- Page 33, Section 4.5 - This tight description of the main effects is what the other sections should emulate, and would tighten up the manuscript considerably.
- Page 35, Line 3 - Again, this sentence is awful to read. Too many adjectives.
- Page 36, Line 7 - I don't see a decline in the figure, just no change.

Conclusion

- Page 38, Line 26 - A decrease in ocean POC export is not necessarily associated with an increase in atmospheric pCO_2. Please see Sigman et al (2010) The polar ocean and glacial cycles in atmospheric CO_2 concentration. Nature. for an explanation. Briefly: it is not total POC export, but the efficiency of carbon fixation relative to outgassing that matters.
- Page 39, Line 13 - Why are you now talking about terrestrial carbon here?
- Page 40, Lines 1-11 - Please see Menviel et al (2017) Poorly ventilated deep ocean at the Last Glacial Maximum inferred from carbon isotopes: A data-model comparison study. Paleoceanography.
- Page 40, Lines 13-14 - But atmospheric $\delta^{13}C$ at the LGM and preindustrial climates were similar at -6.46 ‰ and -6.36 ‰, respectively.
- Page 40, Line 28 - And what do these new records show?
- Page 40, Line 30 - The deglacial decrease in atmospheric $\delta^{14}C$ could also be caused by the exchange of highly negative ocean $\delta^{14}C$ with the atmosphere. The argument here is not clear.

3 Technical corrections

1. Page 7, Line 9 - "did not evidence numerical instability" –> "did not show evidence of numerical instability"
2. Page 7, Line 15 - "has" –> "had". Please use past tense in results sections.
3. Page 9, Line 7 - "impact of error" –> "error"
4. Page 9, Line 7 - "impact of certain" –> "certain"

5. Page 9, Line 7 - ", such as changing" –> ", such as changing" (two spaces)
6. Page 15, Line 4 - "thereis" –> "there is"
7. Page 15, Line 15 - "may have for instance" –> "may have"
8. Page 20, Line 21 - "for instance is that" –> "is that"
9. Page 23, Line 3 - "Most of the ensemble members" –> "Most members"
10. Page 26, first line of table - there is a tab separating -1160 from 530.
11. Page 36, Line 11 - double space –> single space
12. Page 38, Line 19 - "Terr" –> "TerrC"
13. Page 38, Line 27 - remove /