

Interactive comment on “Coupled climate-carbon cycle simulation of the Last Glacial Maximum atmospheric CO₂ decrease using a large ensemble of modern plausible parameter sets” by Krista M. S. Kempainen et al.

Anonymous Referee #1

Received and published: 10 February 2018

General comments

I like it very much that the authors provided an comprehensive review of the mechanisms that governing the LGM atm. CO₂ drop. And I really appreciate that an extensive body of related work are mentioned during the discussion of model results. However, I do have several major concerns.

1) Research aim:

P1 I15-16 and P3 I19 suggest that the aim of this study is "to investigate the causes

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of the LGM atmospheric CO₂ drop". To me, this research aim is not appropriate. Previous studies have already proposed some hypotheses regarding the mechanisms that governing the decrease of atmospheric CO₂ concentration relative to preindustrial (as have been summarised in the Introduction). What is not yet clear is the interplay of these mechanisms and the relative quantitative contribution of each mechanism to the LGM atmospheric CO₂ drop. In this study, the authors did not explicitly propose any new hypothesis. Yet, the quantification of the contribution of different mechanisms using GENIE is not possible due to the simplification of the model and due to that many processes are not accounted for.

I think a more specific research aim/question is needed for this study. When setting up the aim/question, the authors might consider: What are the novel aspects of the model or the EFPC2 ensemble? In this manuscript, is it the first time that an interactive carbon cycle model is applied to LGM? Is it for the first time the sensitivity to process parameters is investigated for LGM climate (Holden et al 2013 pointed out that EMICs are important tools for exploring sensitivities and quantifying uncertainty)? Which mechanism(s) the authors would like to focus on? terrestrial carbon preservation? carbonate weathering?

2) Key findings/conclusions:

The current version of the Conclusion reads more like a summary of the model results. It's not clear what are the key findings of this study. I think the key message would become clear once the research aim/question is given.

3) Comparison of model results to field/proxy data or to results of other models:

A large body of text is devoted to the comparison between model results and data and to the explanation of differences between the two. To me this is a bit overdone. Holden et al. (2010), who used the same model and had the same principle for the design of the ensemble, clearly stated that the ensemble is developed to reproduce the main features, but not the precise observation.

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Specific comments (the following comments mainly concern the suggestions for improving the structure and presentation of model results.)

Abstract

P1 I15-16: It would be helpful to specifically state the research aim here.

Introduction

P3 I27: "unknown error" – does it mean the discrepancy between e.g. the modelled circulation and that obtained from proxy data? To me it sounds like a mistake one accidentally made in the model.

P3 I28: "unknown error" – Maybe "missing processes" is more suitable here?

Please add at the end of the Introduction an overview of the upcoming sections, viz., what will be presented in each section.

Method

P5, Table 1: what is OLR?

P6: I think a table or a flow chart summarising the conditions for the four stages would be helpful for readers to understand the set-up of experiments.

How long is the stage 2?

Is the total carbon inventory (that is, sum of atmospheric, terrestrial, ocean and lithospheric carbon inventory) unchanged over the four simulation stage?

LGM ensemble simulations

P9: Please explain here why the subset PGACF-16 is needed. This can be done by moving p37 I30-32 to section 4.1.

It would be very helpful if a brief overview of the following text of this section 4 is presented: which variables of which set will be presented.

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P25 I6-7: this is not true because in Table 6 none previous observation/model data study shows negative delta_OceanC.

P36 bottom: Fig. 18 should be Fig. 17.

P36 I3: "North Atlantic" and "North Pacific" should be switched.

Comparison of global-integrated numbers and spatial distribution between model results and data: I suggest to first compare the spatial distribution and then the global-integrated numbers because the latter is just the sum of the former.

Colour slots showing spatial distributions of variables, e.g. Fig 3, 6, 7, ...: please add contour line for land-ocean border.

Plots showing standard deviation in e.g. Fig 3, 6, ...: These plots are shown but never mentioned/used/discussed. So please consider move them to a supplementary information file – there are already many figures in the manuscript.

Conclusions

P39 I15: The positive delta_TerrC has been discussed and justified many times through out the manuscript. Thus, I have the impression that this is one key finding the authors would like to stress. I think this is a bit dangerous because this point is not well supported by data. The authors also seem de-stressing this point several times by stating there are other 4 ways of "achieving a plausible delta_CO2 interns of the sign of individual carbon reservoir changes (although Table 5 suggests those 4 ways are much likely to occur). I have to say I am confused by the above statements.

P39 I39 - P41 I15: I understand that it is a pity that carbon isotopes were not simulated. However, I don't think it is appropriate to extensively present inferred results in the Conclusion section.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2017-159>, 2018.

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