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Climate of the Past Discussions

Interactive comment on "The simulated climate of the Last Glacial Maximum and the insights into the global carbon cycle" *by* R. J. Matear et al.

Anonymous Referee #1

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This paper describes simulation experiments with a global climate model for the Last Glacial Maximum and tries to investigate how and why atmospheric CO_2 was smaller during glacial times. Since this topic is still not entirely resolved it is worth the effort. The authors use a model, that was not used before for this exercise, and get indeed in some aspects different results than other studies.

Thus, the scope and aim of the study are worth publishing in this journal. However, the present form (and writing) of the manuscript is in various aspects poor and very difficult to digest, that I suggest some fundamental rewriting and focusing. Most of the citations given in the introduction are 5-15 years old, while there have been some substantial contributions to the field within the last 5 years.

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From my understanding the INCREASE in oxygen in the LGM simulation is what is special about these simulations. I believe this has not been shown before. So I suggest to focus on this issue and also to elaborate/discuss further why your model disagrees to other models here. Note, that data so far do not show anoxic deep ocean during LGM, but models tend to simulate anoxia, if overturing is reduced.

There are some well established processes, which the scientific community believes have been important for glacial/interglacial CO_2 change, although this list is not conclusive, and the level of understanding is also not always high, see for example Figure 6.5, page 482 in the WG1 part of the last IPCC (*Stocker et al.*, 2013). They are changes in (a) sea surface temperature, (b) sea-level and salinity, (c) ocean circulation, (d) iron fertilisation, (e) coral reefs and carbonate compensation, (f) land processes and (g) weathering. In the end in the study of Matear et al. three processes in the models are optimized in order to simulation carbon cycle changes which are at best in agreement with the reconstructions. This is a nice effort, however, within the study I have not seen any assumptions or results for (b) sea-level and salinity, and no mentioning at all of (e) carbonate compensation. From my understanding of the described carbon cycle model all carbon (organic or inorganic) biologically produced that then falls within the ocean to the abyss is remineralized / dissolved again and no sediment/ocean interaction ($\rightarrow$ carbonate compensation) is included in the model. Since carbonate compensation is to my understanding now a well established feedback process, which will modify any initial changes in the deep ocean carbonate ion concentration on multi-millennial years timescales this is an important limitation of the study, which needs to mentioned upfront (abstract) and discuss in its consequences. The contribution of carbonante compensation to changes based on other processes is in other model applications for LGM experiments always accounted for, see e.g. the CLIMBER (*Brovkin et al.*, 2007) or the BERN3D (*Menviel et al.*, 2012)

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model. Thus, since two rather well-known processes are missing in this study I do not think it is worth to tune the model to reconstruct observation.

The paper contains 17 figures, all of which consist of 4 subfigures, in which one variable is shown in a global map or in a latitude-depth view for (most of the figures) (1) reference case, (2) LGM, (3) anomaly of both and (4) present-day data constrains. It is helpful to see all these details, but one more level of condensation might help the paper to be better readable. If necessary, all these figures might be shown in the SI, but some more aggregated or conceptual understanding, which might emerge, would be better. For example Figs 15-17 all show oxygen concentration at different depth levels. I believe this can be condensed in one latitude-depth figure. Furthermore, most figures have the present-day data in the top right corner, but the reference case simulation results in the bottom left corner, so these two subfigures, which show essentially the same are never neighboring each other and are thus difficult to compare. I therefore suggest to change the ordering of the subfigures.

The reference simulation is called “Holocene climate” (HC). This is an unlucky formulation, since from my understanding it contains basically a pre-industrial climate. There is no averaging over the Holocene, it might eventually be called late Holocene climate. I suggest reformulation.

Within the text, very often it is written, that some variable changed “dramatically” or “significantly” or “changed” or “increased” etc, but no numbers are given by how much. Throughout the text this should be specified and be more explicit whenever possible.

These are the more general comments, I will follow with a chronological list of issues, (order does not imply importance here): «

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1. line 1: My understanding still is that “GCM” is a general circulation model, not a global climate model. Since “climate model” might only contain the atmosphere, I suggest changing this throughout the draft.
2. page 1094:
3. line 2: state which model you are using.
4. line 6: “cooling of the ocean” by how much?
5. line 7: “expansion of sea ice” by how much?
6. line 8: “significant redistribution of oxygen and carbon”, by how much?
7. line 25: high correlation of CO_2 and temperature: A better citation than Sigman et al 2010 here is: *Parrenin et al.* (2013) or *Fischer et al.* (2010).
8. page 1095:
9. lines 1-14: Newer references on glacial/interglacial CO_2 are *Bouttes et al.* (2010); *Schmitt et al.* (2012); *Skinner et al.* (2015), that ocean circulation changes / glacial ocean carbon storage is important; *Menviel et al.* (2012) about the importance of transient behaviour of the carbon cycle for the first time performed with and 3D ocean model and a possible role for a deepening of the mean remineralization depth and an increase in the oceanic nutrient inventory (both probably substituting the older Peacock citation); land carbon storage at LGM: *Ciais et al.* (2012).
10. lines 11: “some combination of the above”: *Brovkin et al.* (2007); *Bouttes et al.* (2010); *Menviel et al.* (2012).
11. lines 14: Interaction of physical and biological pump is complex and the net effect on atmospheric CO_2 difficult to project: *Völker and Köhler* (2013)

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12. page 1096:

13. Please describe your MK3L in more detail. Since you call GCM a global climate model, it is not clear to me if MK3L is a fully coupled atmosphere-ocean general circulation model or some reduced form (EMIC?). I know this can be derived from the given references, but it should also be stated here.

14. You do not describe in the text, what the boundary conditions for “Holocene climate” are. If this would have been called “pre-industrial” it would have been clear, but the Holocene was 10000 years long.

15. page 1097:

16. line 1-2: “dynamic land snow scheme”: Does this imply that at LGM your model has a year-round (permanent) snow cover in the areas where land ice sheets should exist? Please clarify, and maybe also show in a figure.

17. page 1098:

18. line 1: Here the unlucky choice of the wording “HC” for the reference climate is shown: “... between HC (=Holocene climate) and LGM climates”. So throughout the text this becomes a problem and should be changed (at best with a different name for the reference case).

19. line 1: “fixed ocean BGC parameterization”. Why “fixed”? Should be “improved” or “changed”.

20. line 6: This is a nice example how complicated/poor the paper is written sometimes: It says: “The LGM-simulated surface ocean is much colder than the HC (3.2âÛęC, see Table 2)”. Much easier to understand would be “The simulated surface ocean is by 3.2°C colder at LGM than in the reference case”.

21. page 1099:

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22. There is a variety of newer papers on present-day sea ice extent, e.g. *Cavalieri and Parkinson* (2012); *Parkinson and Cavalieri* (2012).

23. The whole section 3.1.2 on sea ice might need some streamlining. There are 8 values of sea ice extent (or cover / area, please be precise, both wording are used and I am note sure if they were used correctly (e.g. page 1100, line 14-16)) which might be condensed in a Table, maybe then also omitting some figures: maximum and minimum extent in the north and in the south, for both LGM and reference run. For modern times, we have data, for LGM some reconstruction exists, but uncertainties are high. I do not think, that from the paleo data given it can be said that sea ice extent is the area where sea ice concentration is higher that 15% (line 18). If the figure of sea ice distribution is in a revision still part of the paper (Fig 2 now) it should be plotted in polar projection, showing only polar regions of interest here.

24. page 1101:

25. line 2: “meridional overturing”: global or in Atlantic?

26. line 25: “This slight reduction and shoaling of the NADW in the LGM was consistent with our simulations.” Please change to “Such a reduction of the strength of the NADW and its shoaling during the LGM would be consistent with our simulation results.”

27. page 1102:

28. line 12: “much less” Strange wording, appears several times in the draft:, maybe use "a lot less”.

29. line 14ff: “The reduction in atmospheric CO2 between the HC and LGM meant the surface water equilibrated with a much lower atmospheric CO2 level in the LGM and subsequently reduced DIC concentrations.” This is not entirely correct:

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The marine carbonate system has 6 variables and 2 degrees of freedom. If you change alkalinity you would also change the concentration of dissolved CO_2 and therefore the gas exchange. Alkalinity is for example change by carbonate compensation, which is unfortunately not contained at all in the model.

30. line 23: “were much greater in the deep ocean than IN the HC simulation”. IN was missing here, this is one example, happens in that way ofter in the draft, so careful proof-reading by a native speaker is essential.

31. page 1103:

32. lines 18-28: Please also consider the proposed proxy-based changes in export production only north of the polar front published by *Kohfeld et al.* (2005).

33. page 1104:

34. line 19: “fixed atmospheric CO_2”. There is nothing to fix here, you revised your assumptions in order to meet the data, see at the beginning my opinion on that idea.

35. page 1105:

36. line 11: “scaling factor for POC”, please explain with more words. Increase by x10 is a lot.

37. line 12: There is no Eq 34.

38. line 23: There is no Eq 010

39. page 1106:

40. line 8: “With no PIC export the solubility of CO_2 ... increased”. To my understanding there is no reason for that, so something is mixed up here, which needs more words.

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41. page 1108:

42. Section “Calcium carbonate stability”. This section gives me a hard time. Maybe they want to test what others call “carbonate compensation”, but they never call it as such. They have no sediments in the model, so they focus on aragonite saturation state (why not calcite saturation state?) and lysocline changes. This needs some rewriting.

43. page 1109:

44. line 10: Please give a data-based reference that a super saturation of the entire water column outside the tropics was unrealistic.

45. line 21: There are no “good” evidence, maybe “supporting” evidence.

46. page 1112:

47. line 2: Reference Jaccard and Galbraith is from year 2012, not 2011.

48. Appendix: This is the same description of the GBC than in Matear and Lenton (2014), so might even be omitted or moved to the SI.

49. Appendix: In Eq A8 and A9 the parameter “0.08” denotes the “fixed rain ratio” of 8%, however in your earlier application (Matear and Lenton 2014) this has been 9%. This difference needs some description why this assumption has been changed and what it implications are.

50. Table 1: Orbital parameter: “0kBP” → “0kaBP” and “21kBP” → “21kaBP”

51. Table 2: You miss a lot of recent papers on LGM SST after the MARGO 2009 paper (here cited as Waelbroeck et al 2009), which showed that MARGO was biased (*Telford et al.*, 2013; *Schmidt et al.*, 2014), see also *Annan and Hargreaves* (2013).

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References

Annan, J. D., and J. C. Hargreaves (2013), A new global reconstruction of temperature changes at the Last Glacial Maximum, *Climate of the Past*, *9*(1), 367–376, doi:10.5194/cp-9-367-2013.

Bouttes, N., D. Paillard, and D. M. Roche (2010), Impact of brine-induced stratification on the glacial carbon cycle, *Climate of the Past*, *6*(5), 575–589, doi:10.5194/cp-6-575-2010.

Brovkin, V., A. Ganopolski, D. Archer, and S. Rahmstorf (2007), Lowering of glacial atmospheric CO_2 in response to changes in oceanic circulation and marine biogeochemistry, *Paleoceanography*, *22*, PA4202, doi: 10.1029/2006PA001380.

Cavalieri, D. J., and C. L. Parkinson (2012), Arctic sea ice variability and trends, 1979-2010, *The Cryosphere*, *6*(4), 881–889, doi:10.5194/tc-6-881-2012.

Ciais, P., A. Tagliabue, M. Cuntz, L. Bopp, M. Scholze, G. Hoffmann, A. Lourantou, S. P. Harrison, I. C. Prentice, D. I. Kelley, C. Koven, and S. L. Piao (2012), Large inert carbon pool in the terrestrial biosphere during the Last Glacial Maximum, *Nature Geoscience*, *5*(1), 74–79, doi:10.1038/ngeo1324.

Fischer, H., J. Schmitt, D. Lüthi, T. F. Stocker, T. Tschumi, P. Parekh, F. Joos, P. Köhler, C. Völker, R. Gersonde, C. Barbante, M. L. Floch, D. Raynaud, and E. Wolff (2010), The role of Southern Ocean processes on orbital and millennial CO_2 variations — a synthesis, *Quaternary Science Reviews*, *29*, 193–205, doi:10.1016/j.quascirev.2009.06.007.

Kohfeld, K. E., C. Le Quéré, S. Harrison, and R. F. Anderson (2005), Role of marine biology in glacial-interglacial CO_2 cycles, *Science*, *308*, 74–78.

Menviel, L., F. Joos, and S. Ritz (2012), Simulating atmospheric CO_2, ^{13}C and the marine carbon cycle during the Last Glacial-Interglacial cycle: possible role for a deepening of the mean remineralization depth and an increase in the oceanic nutrient inventory, *Quaternary Science Reviews*, *56*(0), 46 – 68, doi:10.1016/j.quascirev.2012.09.012.

Parkinson, C. L., and D. J. Cavalieri (2012), Antarctic sea ice variability and trends, 1979-2010, *The Cryosphere*, *6*(4), 871–880, doi:10.5194/tc-6-871-2012.

Parrenin, F., V. Masson-Delmotte, P. Köhler, D. Raynaud, D. Paillard, J. Schwander, C. Barbante, A. Landais, A. Wegner, and J. Jouzel (2013), Synchronous change in atmospheric CO_2 and Antarctic temperature during the last deglacial warming, *Science*, *339*, 1060 – 1063, doi:10.1126/science.1226368.

Schmidt, G. A., J. D. Annan, P. J. Bartlein, B. I. Cook, E. Guilyardi, J. C. Hargreaves, S. P. Harrison, M. Kageyama, A. N. LeGrande, B. Konecky, S. Lovejoy, M. E. Mann, V. Masson-

Delmotte, C. Risi, D. Thompson, A. Timmermann, L.-B. Tremblay, and P. Yiou (2014), Using palaeo-climate comparisons to constrain future projections in CMIP5, *Climate of the Past*, *10*(1), 221–250, doi:10.5194/cp-10-221-2014.

Schmitt, J., R. Schneider, J. Elsig, D. Leuenberger, A. Lourantou, J. Chappellaz, P. Köhler, F. Joos, T. F. Stocker, M. Leuenberger, and H. Fischer (2012), Carbon isotope constraints on the deglacial CO_2 rise from ice cores, *Science*, *336*, 711–714, doi:10.1126/science.1217161.

Skinner, L., I. McCave, L. Carter, S. Fallon, A. Scrivner, and F. Primeau (2015), Reduced ventilation and enhanced magnitude of the deep Pacific carbon pool during the last glacial period, *Earth and Planetary Science Letters*, *411*(0), 45 – 52, doi:http://dx.doi.org/10.1016/j.epsl.2014.11.024.

Stocker, T. F., D. Qin, G.-K. P. M. Tignor, S. Allen, J. Boschung, A. Nauels, Y. Xia, V. Bex, and P. Midgley (Eds.) (2013), *IPCC, 2013: Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change*, Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA.

Telford, R. J., C. Li, and M. Kucera (2013), Mismatch between the depth habitat of planktonic foraminifera and the calibration depth of SST transfer functions may bias reconstructions, *Climate of the Past*, *9*(2), 859–870, doi:10.5194/cp-9-859-2013.

Völker, C., and P. Köhler (2013), Responses of ocean circulation and carbon cycle to changes in the position of the Southern hemisphere westerlies at Last Glacial Maximum, *Paleoceanography*, *28*, 726–739, doi:10.1002/2013PA002556.

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