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Climate of the Past Discussions

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

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The major suggestion by the reviewer is for a general re-wording of our conclusions. When we suggest that "physical changes cannot in isolation explain the necessary drawdown of CO2 at the Last Glacial Maximum", the reviewer asks for "the simulated physical changes cannot in isolation explain the necessary drawdown of CO2 at the Last Glacial Maximum". While this necessitates only a small number of additions to the paper, the effect on our conclusions is a significant one. However, we agree with the reviewer in their suggestion because we acknowledge that probably the biggest assumption of this work is that the physics of the LGM as simulated by the CSIRO Mk3L were an accurate representation of reality. Thus, we would undertake the re-wording as is suggested by Andreas Schmittner and feel that this change will significantly improve the manuscript.

The remaining comments by the reviewer are very specific in nature. These are suggestions for additional references, clarifications of model architecture/omissions, experimental caveats, and grammatical issues. We will to the best of our ability accommodate these changes into the manuscript.

We detail our responses to each specific suggestion made by the review below. These responses are made in the order to which the reviewer made their comments.

1) The work of Annan and Hargreaves (2013; doi:10.5194/cp-9-367-2013), has already been referenced in the manuscript in the section on Sea Surface Temperature (section 3.1.1).

2) The effects of wind stresses and tidal mixing can be added to the discussion of physical factors affecting the glacial sequestration of carbon and circulation changes.

3) The Bering Strait is open in the model, and we can make this point clear by adding an extra sentence.

4) The addition of 0.5 psu of salinity is indeed about half that estimated for a 120 m drop in sea level at the LGM. The accumulation of water in snow across the land caused the development of a drier atmosphere in the simulated LGM, and this increased the salinity of the ocean by increasing evaporation. Because we did not artificially add salinity to the ocean in the ocean-only experiments, the addition of salinity was maintained at 0.5 psu. It is possible for us to re-run our experiments by adding an additional 0.5 psu to the salinity field. This would alter our results, potentially causing an increased sequestration of carbon in the deep ocean by further increasing the salinity-driven density gradients.

5) Monthly averages. This can be added to the sentence.

6) We have neglected to include certain parts of the equation because they are available in Appendix A of Matear and Lention (2014), which we point the reader to for further information. However, we could easily make these additions to the methods

section. These additions would include:

- An explanation of the Michaelis-Menten relationship between nutrient availability and phytoplankton uptake.

- An explanation of the maximum growth rate of phytoplankton as dependent on temperature.

- An explanation of the light limitation term F(I).

The multiplication by 12 is to convert from moles carbon to grams of carbon, but this may not be necessary to include in the equation.

7) A disclaimer can be added to ensure that these processes are acknowledged and that the ready is clear that we do not consider them in this study.

8) It should be noted that we use the term "broadly" in this sentence. This refers to the overall pattern of expansion, including the greater increase in sea ice cover in the western North Atlantic relative to the east. Also, the sea ice cover is represented in the paper as a fractional cover, that is on a scale from 0 (no cover) to 1 (complete cover). Thus, seasonally-free ice cover in the eastern Nordic seas as suggested by proxy data (de Vernal et al, 2005) cannot distinguish if small amounts of sea ice was still present during annual minima. However, the reviewer makes a good point, as the conditions at the LGM in the North Atlantic most likely consisted of greater ice cover in the western Nordic seas and sea ice free summers in the eastern Nordic Seas.

This inconsistency can be addressed in the manuscript. We suggest that the sentence "this study is broadly consistent with the palaeo evidence in the North Atlantic" is kept, but that we acknowledge that the seasonal opening of the eastern Nordic Seas was not captured in the sentences prior.

However, it should be remembered that this is a course resolution climate system model and it cannot be expected to capture fine detail changes in sea ice within a region like the Nordic Seas. In fact, on closer inspection, large summertime reductions

in sea ice were simulated in the region north of Eastern Europe. Although this is not the eastern Nordic Seas, we again suggest that the model "broadly" captures the pattern of sea ice changes at the LGM in the North Atlantic.

9) This was indeed a mistake. The total volume transport out of the Southern Ocean was calculated by taking the depth and longitudinal integrated transport (Sv) across 45°S to obtain in m yr-1. Thus, the reviewer is correct in that it is not Sverdrups and this is a mistake. However, for the sake of reader comprehension, we will alter these measurements to be in Sv and to reflect the average export out of the Southern Ocean across 45°S and in depth.

10) We can update our model comparison values using the study that the reviewer proposes. This will necessitate an addition to Table 2 and will necessitate changes to our discussion of the changes in circulation. Namely, that the weakening of the AMOC is not consistent with PMIP3 simulations of the LGM conditions. This weakening in the AMOC is due to a reduction in North Atlantic salinity in the LGM simulation. SEE ATTACHED FIGURES Once again, this inconsistency may be rectified by re-completing the experiments with an artificial addition of 0.5 psu salt to the salinity field to ensure that the ocean increased in total salinity by 1 psu, consistent with a sea level drop of 120 m.

11) Follow this link –> http://www.ldeo.columbia.edu/~broecker/Home_files/WhatDrvsGlacCy

12) We agree that the increase in diapycnal mixing due to enhanced tidal mixing proposed by Schmittner et al (2015) should be acknowledged in the text. This can be added to this discussion, acknowledging that we do not consider tidal mixing in the coarse resolution climate model.

13) A comparison of our results with Schmittner and Somes (2016) will be a constructive addition to the manuscript and we thank the reviewer for bringing it to our attention. We aim to include all necessary comparisons, which will include direct comparisons between their export production fields and the carbon sequestration they achieve.

14) We have addressed the changes in the lysocline seen in the model experiments in light of our neglect of sediment interactions later in the manuscript. See section 3.3.3. Changes will also be made to this section based on the suggestions of the other reviewer.

15) We agree with the reviewer and expect to make the comparison with Schmittner and Somes (2016) regarding biological pump efficiency, relative to biological pump strength.

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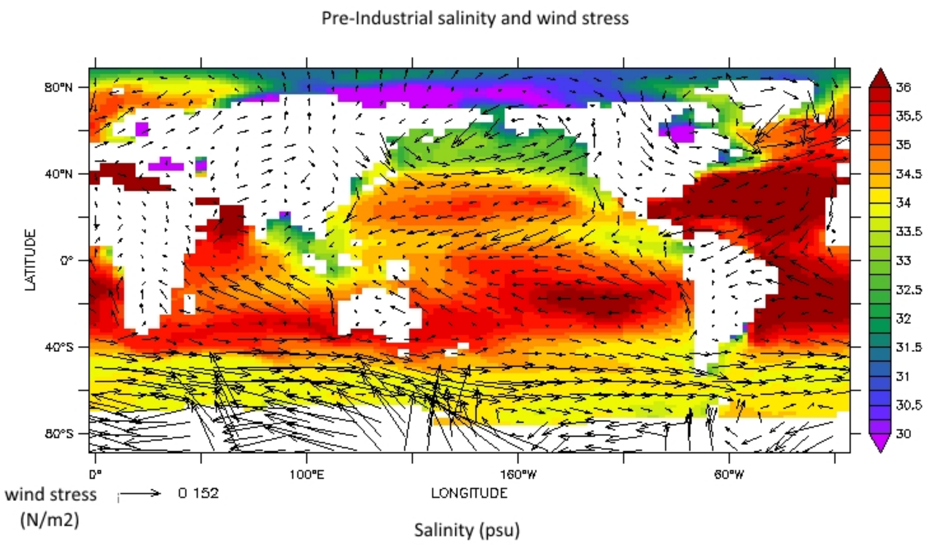

Fig. 1.

Last Glacial Maximum salinity and wind stress

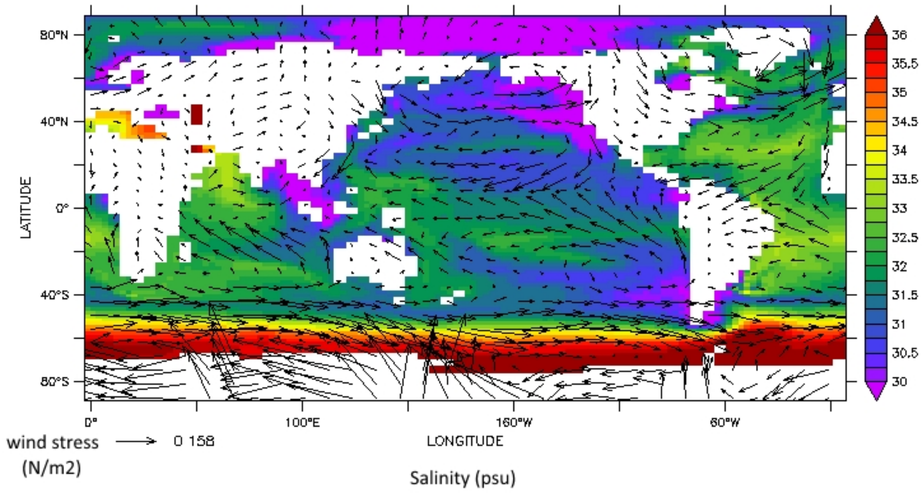

Fig. 2.