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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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In response to Katsumi Matsumoto:

1) The reviewer has made an important point here that we will rectify to the best of our ability. We agree completely with the reviewer in that it is important to pay homage to those scientists that made the initial connections, as well as referencing contemporary findings. We would hope to make the changes in the Introduction and there include some of the seminal studies that the review mentions, plus others that made important contributions to the field. However, it should be of note that we have payed homage already to the following seminal studies:

- The Broecker (1982) paper that the reviewer mentioned is indeed included in the text (p2, line 28), when we make a special effort to discuss the potential of ocean biology

for affecting the climate.

- Papers by Shackleton (1967) and Emiliani (1966) that were among the first to identify the sawtooth cycling between glacial-interglacial climates

- Jouzel (1987) and Petit (1999) discovered the remarkable correlation between atmospheric CO2 and climate cycles

- Duplessey (1988) found that a chemical divide between the upper and deep ocean occurred during glacial periods, indicating reorganisations in the ocean circulation.

- Archer & Maier-Reimer (1994) was the first to use a model with carbon compensation to show that changes in the dissolution of calcium carbonate in the deep ocean can explain large changes in glacial-interglacial CO2 changes.

2) The names of the experiments may be altered to reflect the changes that were made. The changes we might suggest are as follows:

- O-PI1 to O-PI

- O-PI2 to O-PI(pCO2-185)

- O-LGM1 to O-LGM

- O-LGM2 to O-LGM(pCO2-280)

- O-LGM3 to O-LGM(bgc-poc)

- O-LGM4 to O-LGM(bgc-rem)

- O-LGM5 to O-LGM(bgc-pic)

- O-LGM6 to O-LGM(bgc-all)

3) While justifications for the biogeochemical modifications have already been made in the Methods section, we can acknowledge that perhaps the prescription of changes (as opposed to their generation through modelling their mechanistic behaviour) was

not made clear enough in the text. To make this clear, we suggest adding a new paragraph in the Methods Section beneath line 10 on page 5: "These biogeochemical modifications are designed to capture fundamental changes in the oceanic biological pump that are likely to have occurred under glacial conditions (see above). They do not explicitly capture the modifications themselves in any mechanistic sense. Deepening the global remineralisation profile in experiment O-LGM$ˆ{bgc}_{rem}$, for instance, is a prescribed change that is informed by global cooling, rather than a change caused by cooling that is simulated in the model. However, the prescription of these changes allows us to undertake a theoretical investigation into their capacity to sequester carbon at the LGM."

4) De La Mare (1997) Nature

5) Easily changed and will do so.

6) For this comment, the reviewer asks for additional experiments to be run. These experiments are currently underway and their results can be supplemented into the paper within a matter of days. This would, however, require an extra paragraph of text and additions to the major tables.

7) The increase in salinity throughout the global ocean had a greater effect on the formation of deep waters in the Southern Ocean than in other regions. In fact, sea surface salinity decreased over much of the low to mid latitude oceans, including the North Atlantic, while it increased in the Southern Ocean. Please see the supplemental figure that is attached, showing pre-industrial salinity field (left) and the LGM-PI difference (right) in psu.

8) The Arctic in general was not necessarily warmer. The confusion may have arisen due to the pink colour, but if special attention is given to the colour bar at the bottom of Figure 1, it can be seen that this pink colour centres around a 0°C change. However, the reviewer is right to point out that there is a small region of slightly warmer temperature in the Arctic above eastern Europe. On further investigation of our simulations,

the warmer spot as seen in the annual average sea surface temperature difference (Fig 1b) is a symptom of warmer late winter, spring and early summer temperatures. The warmer temperatures also caused lower sea ice coverage in this region during spring/early-summer, despite increased sea ice coverage in general throughout the NH. The increase in temperature and loss of sea ice during the spring in the surface waters in this region was caused by significantly reduced wind stresses that reduced heat fluxes out of the ocean in late winter and early spring. Because this is a relatively small result in the context of this study, we have not included the above discussion within the paper, which we feel would de-rail the logical flow of the text.

9) The reviewer makes a good point. We referred to aragonite primarily because, as the more unstable species of carbonate, it shows the greatest sensitivity to changes in alkalinity. Also, changes in surface aragonite are more appropriate when assessing the model against reconstructions of coral reef changes. We agree with the reviewer and suggest changing the manuscript to accommodate changes in the calcite saturation depth, rather than the aragonite saturation depth. This would mean altering:

- Paragraph 3 of section 3.2.3 (beginning page 10)

- Section 3.3.3 (beginning page 14)

However, we have not altered our discussion of the aragonite saturation state in the surface ocean, because this provides the best model-data comparison for coral reef reconstructions.

10) We can add an extra sentence to introduce what the aragonite/calcite saturation state is.

11) By following the reviewer's suggestion (9), this comment would be addressed.

12) While low, the 326 PgC that was added to the ocean does fall within the bounds of error provided by Ciais et al (2011). This is an important point. However, the reviewer is correct in pointing out that this number is on the lower bound of the estimates. Com-

bined with the fact that the biogeochemical modifications could not deoxygenate the deep ocean, this indicates that additional processes (physical or biogeochemical) were not captured in the simulations, and therefore we would aim to make suggestions for future work to address these inconsistencies. This, we believe, is one of the critical conclusions of the study.

13) 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.

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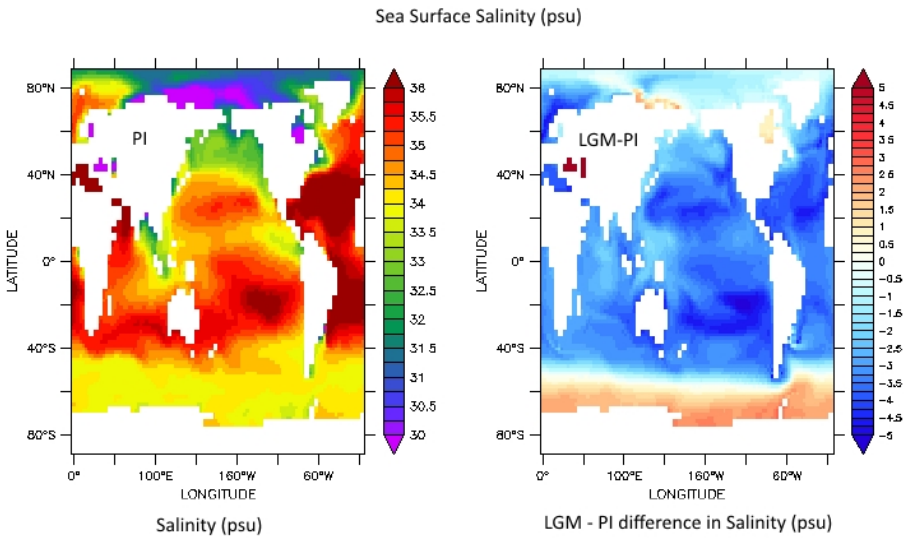

Fig. 1.