

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 authors tackle a grand old problem in geochemistry, the low glacial atmospheric pCO₂. The topic is of interest to and appropriate for the CP readership.

Since Broecker's work in the 1980s, it's been well recognized that the ocean chemistry must have played a significant role. However, the community has struggled to explain the problem with any single mechanism or a combination/sequence of multiple mechanisms. This new modeling work by Buchanan does not offer a new mechanism per se but very nicely puts some of the major mechanisms within a theoretical framework offered by a single model architecture.

There is a lot to like in this submission. The coupled model simulations of physics (ice, circulation) are quite reasonable for both PI and LGM. I was impressed with their LGM

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simulations that realized a deep chemical divide that separated the upper ocean (well ventilated, low values of C, P, ALK) from the deeper ocean. The rationale for requiring biogeochemical mechanisms is well articulated. There is good discussion of the new results with available paleoproxies. Data-model disagreement (e.g., deep O₂) and model shortcomings (e.g., inability to simulate open system carbonate compensation) are plainly presented. The paper is organized logically and well written. I believe this submission would make a nice contribution to the field and I support its publication.

I do have some comments and suggestions in no particular order that the authors may consider in their revision.

1) Overall I see newer papers being cited. While this shows that the authors are up to date and is obviously good, I also feel that some of the original papers should be cited. For example, David Archer has written many important papers on G-I CO₂. There is a lot in his 2000 paper in *Rev. of Geophysics*. The lead author should read it, if he has not already, and cite it. Archer has explicitly modeled open system carbonate compensation in an OGCM, something that this work does not do and speculates on. Another example is that Broecker (1982) is probably one of the first to say that the ocean is key to the low glacial CO₂, way earlier than the 2014-2015 papers cited (line 9, p. 2). There are other examples. My preference is that original papers are cited instead of newer papers that regurgitate original ideas.

2) The names of LGM experiments are just numeric (esp. 3-6) which I found a bit difficult to commit to my memory. Perhaps rename them to something more obviously descriptive.

3) The biogeochemical runs (LGM3-5) all simulate the effects of the desired modifications without explicitly modeling the modifications themselves mechanistically. The authors should discuss the actual mechanism (e.g., how do you envision the ocean actually increasing production? Fe? Excess nutrients from shelf weathering? How do you envision remineralization depth scale actually increasing? Temperature dependence?

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But you actually only change the power law exponent everywhere without regard to how the temperature distribution changed. How would the ocean turn off PIC export? Diatom dominance due to Si leakage? What about corals?). The authors should then discuss possible impacts of the simplifications made.

4) Need reference for why whaling records give true estimates of ice.

5) Change “whom” to “who” on page 7, line 34.

6) Section 3.2.1 Carbon: I might suggest splitting the attribution of changes in carbon content to ocean physics into those driven by solubility, ice, and circulation. For example, how can we make better sense of the 517 PgC change (page 9, line 12)? You could create a mask for SST as it relates to solubility and control for that. Likewise, you could create a sea ice mask as it relates to gas exchange and control for that. Then you can get the circulation component by subtraction: total change – change due to solubility – change due to ice. An example of how you can do this splitting can be seen, for example, in Matsumoto et al. (2010) in *Tellus*. This splitting would require making masks from the PI1 run and doing additional experiments. . .extra work for sure.

7) What specifically about the glacial BCs that causes the NADW to go down by 25% and the AABW-NADW boundary to shoal by 1500 m? I think this is rather important to note.

8) Figure 1b: why is the Arctic warmer in LGM than PI?

9) The authors exclusively discuss aragonite when considering carbonate ion saturation. My sense is that it is far more common to discuss calcite over aragonite in the paleo literature as it related to the glacial CO₂ problem. Lysocline is typically estimated based on forams in sediments. I would suggest that the authors switch to calcite in their revision.

10) Define omega on page 10, line 27.

11) It is incorrect to equate aragonite saturation horizon and the lysocline on page 11,

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line 5-6. The former is a water column chemistry feature; the latter is a sedimentary feature. Also, the phrase “aragonite saturation horizon” sounds incorrect to me. It’s more accurate to say, “carbonate ion saturation horizon with respective mineral aragonite.” May be shorten the phrase after introducing it in full and correctly the first time it appears in the text. Again, it’s far more common to talk about calcite than aragonite. It should be noted also that the lysocline and the saturation horizon (for whichever form of CaCO₃ mineral) could theoretically be decoupled.

12) Despite the qualifier on page 13, line 6, my sense is that 326 Pg is still low. Glacial atmospheric CO₂ was lower by ~100 ppm (or ~200 PgC). That only leaves 126 Pg (326-200) for change in terrestrial biosphere change. That seems too small. The deep ocean carbon isotope constraint on change in terrestrial biosphere (e.g., Shackleton, 1977) is still pretty strong in my view.

13) There must be a mistake on page 8, line 9. Can’t be tens of thousands of Sv.

Enjoyed reading this.

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