Re-review on

Sequential changes in ocean circulation and biological export productivity during the last glacial cycle: a model-data study

by CM O'Neill et al

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This is a re-review on a major revision of the paper. I have already been involved as reviewer in evaluating the original version.

In principle, I can agree with the rebuttal and the effort undertaken to revise the paper. However, I have to say that the rebuttal was focusing too much on lengthly arguments why things have (or have not) been done ending in 77 pages of responses. In this sense less is more. One can not expect to read that much arguments in detail. Over the length of arguments, however, the changes in the paper have been forgotten here and there. For example. Table S13 in the SI gives numbers for various parameters and processes, but the Table is to my knowledge not cited in the main text, and also never refered to its content (I did not check on all Tables and figure in the SI).
Before acceptance I ask the authors to correct my issues below, mainly minor, but 1–2 potentialla major (#2, #6).

1. Model description: The paragraph on C3, C4 and terrestrial biosphere (mainly on page 6) gives mainly arguments why something is NOT done. This is a discussion and should be put in section 5.3.

2. Model description: Nothing is said on weathering, volcanic outgassing and the corresponding 13C, the only details to that are found in the unreferred Table S13. Please include a paragraph here (and not only in the rebuttal), and check if all material in the SI is addressed at least once. Here also, some explanation is necessary for the weathering fluxes, since they come in units "mol/m^3/yr. Does this imply the weathering is put in the entire water mass, or only surface boxes (which?)? Also

extend the Table S12 on the references, on which the chosen parameter values are based on, or extend the table with footnotes, in which you explain your choice, if one reference is not possible (I believe those details have been in the rebuttal already). Air-sea fractionation factors: Are they fixed? Do you fractionate both fluxes air2sea and sea2air, as typically done (e.g. Mook, 1986)? The line "silicate weathering CO_2 flux δ^{13}C" is not necessary, since this is obvious. Maybe add another column for the units and make the table wide enough, that no line-breaks in individual entries are necessary.

3. Page 10: Schneider et al (2013) gives 3 potential processes for the 0.4‰ change from PGM to LGM, not only the "likely cause by land C" mentioned here. These three causes are given later-on in section 3, page 15, but I believe they are better suited here.

4. page 11, line 6. You need to start a sentence with a word, not with "$\sim$".

5. page 15, lines 9ff: The 0.4‰ rise in $\delta^{13}CO_2$ is between PGM and LGM, not between last interglacial and Holocene.

6. Terrestrial biosphere: There is still something wrong here. In Fig 12 you show a decline in land C from 2200 Pgc (MIS 5e) to 1700 PgC (MIS 2). This release of 500 PgC leads to a RISE in CO_2 on a hundred-thousands years time-scale of about 25 ppm (airborne fraction for 100 kyr should be about 10%), see also Köhler et al. (2010) cited here. However, in Figure 14 it is suggestes that the contribution of terrestrial carbon is always more or less the same. I believe this is obtained by switching land C on/off for equilibrium runs, but never doing transient runs. I therfore believe, this is wrong, land C should be extracted from Fig 14. (also, what does "(RHS)" (added to the label of terrestrial biosphere in Fig 14) mean? Right-hand-side? of what? y-axes are the same left and right???). So, I am not sure what the correct answer from that model to the contribution of land C on CO_2 is, but Figure 12 should guide the solution. Maybe the runs were too long, leading more or less to similar oceanic C uptake of the C released from land? Anyhow, if a decent

answer can be found here it should be included in the list of processes changing CO_2 given on page 30, which discuss fig 14, even if land C is NOT contributing to the deglacial CO_2 rise, but make the CO_2 changes, that need to be explained, larger. If no decent answer comes up for the land C contribution (e.g. due to the setup with equilibrium runs), this should also be stated here.

7. section 5.2. You might add Köhler et al. (2010), which is already cited, to the list of references that claim that a number of processes are necessary to explain the LGM-Holocene CO_2 change (line 20), and also to those which claim a contribution of wind-borne iron-induced Southern Ocean productivity (line 30). If interested in more detail on both, they are found in previous papers (Köhler et al., 2005; Köhler and Fischer, 2006).

8. Data and code (section 7). What is found at https://doi.org/10.5281/zenodo.3559339 is a V2 from December 2019, suggesting that the final changes necessary for this revision here, have not yet been uploaded.

9. Throughout the draft: "Kohler", should be "Köhler"

10. Throughout the draft: "Francois et al, 1999", should be "François et al, 1999"

11. page 39, line 5: Second author of Kohfeld et al. 2005, is "Le Quéré, C.", not "Quéré, C.L."

12. page 36: Authors missing in "Arneth et al 2017".

13. Check reference list for details, e.g "CO_2", not CO2; not DOI and http, etc.

14. I believe it should be "C_3" and "C_4", not "C3" and "C4".

References

Köhler, P. and Fischer, H.: Simulating low frequency changes in atmospheric CO_2 during the last 740 000 years, Climate of the Past, 2, 57–78, doi:10.5194/cp-2-57-2006, 2006.

Köhler, P., Fischer, H., Munhoven, G., and Zeebe, R. E.: Quantitative interpretation of atmospheric carbon records over the last glacial termination, Global Biogeochemical Cycles, 19, GB4020, doi:10.1029/2004GB002345, 2005.

Mook, W. G.: ^{13}C in atmospheric CO_2, Netherlands Journal of Sea Research, 20, 211–223, 1986.