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Climate of the Past
Discussions

Interactive comment on "Glacial CO_2 decrease and deep-water deoxygenation by iron fertilization from glaciogenic dust" *by* Akitomo Yamamoto et al.

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Yamamoto and co-workers present a nice modeling study of glacial ocean oxygen and carbon changes. The manuscript is well written (except for a few typos) and nicely illustrated. I think the main new finding is that glaciological iron sources from Patagonia are particularly important for lowering atmospheric CO2. Although similar suggestions have been made previously with simpler models (e.g. Brovkin et al., 2007) this study is the first to my knowledge that cleanly separates glaciological from other (desert) dust sources.

However, I have a few concerns that require revisions. Some of those concerns result

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from a study by Khatiwala et al. that is currently in review with Science Advances. We hope that it will be published soon so that the authors can access it and consider it in their revision. Khatiwala et al. use a data-constrained model of the LGM to decompose the carbon cycle. They show that using the AOU approximation to calculate respired carbon leads to large errors (even the wrong sign) in LGM – PI simulations. This conclusion is supported by previous studies who have demonstrated the errors in the AOU approximation (Russell et al., 2003; Ito et al., 2004; Duteil et al., 2013). For this reason, I would advise not to use it and remove the corresponding parts of the manuscript (e.g. in section 3.2). It is OK to refer to the iron fertilization effect as increasing the efficiency of the biological pump, but not that the LGM biological pump was enhanced. Khatiwala et al. show that the biological pump was not enhanced, but that air-sea disequilibrium was increased, which caused the glacial ocean carbon inventory to be larger. Air-sea disequilibrium was enhanced in the LGM not only for carbon but also for oxygen and radiocarbon. Khatiwala et al. show that in their best fitting model the ideal age of the whole ocean is younger, while the whole ocean c14-age is older due to the increased disequilibrium (or increased preformed c14-age). This is relevant for the discussion at the end of section 3 (lines 280-287) and the corresponding parts of the abstract (lines 22-24). Thus, ideal age and c14-age cannot be compared and there may not be a discrepancy here between modeled younger ideal age and older (observed) c14-age. I also think that one quantitative oxygen reconstruction from the Southern Ocean alone (Gottschalk et al. 2016) is not enough to indicate that the model is wrong. Reconstructions have errors and therefore I would not overemphasize this apparent discrepancy.

Another concern is the discussion of nutrient inventory changes. Somes et al. (2017) have considered this and shown that existing nitrogen isotope data provide no constraints on this effect. I'm also not aware of other observations supporting it (including evidence provided in this manuscript). For this reason, I think this effect remains unconstrained by observations and thus highly uncertain. I'd encourage the authors to reflect this uncertainty more in their discussion of this effect and to cite the above paper, which has also examined its effects on oxygen.

The authors claim that their model fits reconstructions of export production by Kohfeld et al. (2005), which show not much change in the Pacific sector of the Southern Ocean. However, there are some newer data from that region by Studer et al. (2015) and Wang et al. (2017) that indicate increased nutrient utilization there as well. This suggests that the model underestimates iron fertilization in the Pacific sector of the Southern Ocean. In any case, given the uncertainties in existing paleo data and iron models and solubility of iron, it is not fair to say that the upper limit of iron fertilization is 20 ppm as claimed here in lines 214-215. Khatiwala et al. suggest an iron effect of 35 ppm. Here I also disagree with Fortunat's suggestion to mention the CO2 limit in the abstract. I don't think it is a robust result. However, the idea that the effect of iron fertilization is limited and that increasing fluxes will have a smaller effect at high fluxes than at low fluxes is robust and agrees with previous results (Muglia et al., 2018). The latter paper suggests this limitation is due to increased scavenging rather than reduced regions of iron limitation. Both seem plausible explanations.

Minor comments: Line 16-17: I suggest to remove "(e.g. more sluggish ocean circulation)" because no such attribution was done in the paper. Khatiwala et al. suggest no CO2 effect from ocean circulation changes.

Line 17-18: I suggest to remove "enhanced efficiency of the biological pump" here for the above mentioned reasons.

Line 21: this sentence is awkward. I suggest to rephrase to "glacial deep water was a more severe environment for . . . than the modern ocean."

Lines 24, 26: again, I'd suggest to rephrase to avoid using the term "biological pump" because it has not been quantified how much CO2 change was due to biological pump changes. Perhaps better to use "iron fertilization and/or global nutrient increase".

Line 31: the biological pump also includes the CaCO3 pump

Lines 50-51: consider including Schmittner and Somes (2015) and Somes et al. (2017)

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who have also looked at oxygen

Lines 51-52: Khatiwala et al. have explored oxygen changes in more detail

Line 83: see above comments on “biological pump”

109-110: iron solubility is modified by transport in the atmosphere. This leads to increasing solubility at lower concentrations. This effect has been considered in Muglia et al. (2017; their Fig. 2). This suggests using a constant solubility is not correct. This should be discussed.

116-119: This is about a factor of 10 increase in the 3% experiments. Compare with Muglia et al. (2018) who only have a factor of 4 increase in their best fitting model, which is constrained by d15N and d13C data.

129-130: Muglia et al. (2017) shows the sea level effect to be important.

General comment on section 2: how was the effect of sea level lowering on benthic denitrification treated? Somes et al. (2017) show that this effect reduces N loss in the LGM ocean and leads to a larger N inventory.

165: delete: “because dust deposition flux of the Southern Ocean is underestimated in LGM_dust”

166: delete “in the” and “with iron limitation”

167: delete “in the”

182-184: see above comment on new data from the S. Pacific

199-201: see above comments on biological pump. I doubt that this conclusion is true because of the use of the AOU approximation here, which compromises the results.

239: replace “is the one” with “may be one of the”. Or even better remove this whole part due to my above comments.

243: typo: “whehre”

265-266: Schmittner and Somes (2015) and Somes et al. (2017) also get a deep ocean O2 decrease

318: I don't think that's an issue. See comments above.

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