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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 colleagues present an interesting analysis of glacial change in atmospheric CO2 and marine oxygen. The authors investigate, using a range of factorial analyses, the impacts of glaciogenic iron input and an increased nutrient inventory in the glacial ocean. They apply an offline biogeochemical model for Last Glacial Maximum (LGM) and preindustrial (PI) conditions. They simulate an upper limit for the CO2 decrease due to iron fertilization of 20 ppm and a similar decrease due to an increase in whole ocean nutrient inventory. They present a novel model-proxy comparison for PI-LGM changes in O2. The results suggest a role of iron fertilization and changes in

nutrient inventory for low glacial CO2 and for the reconstructed oxygen changes.

The manuscript is concise and well written. Figures and tables are illustrative and support the conclusions.

I recommend publication of the manuscript after minor revision.

1) I find it interesting that the upper limit for iron fertilization is 20 ppm (p10, l215). I would appreciate if this finding is lifted to the abstract.

2) Figure 8 shows results from WOA2009 and simulated anomalies. Results for the model for the modern ocean should be displayed as well. This would permit the reader to assess the quality of the simulated O2 field.

3) There are some language problems, e.g. missing articles, and the manuscript would benefit from proof-reading by a native speaker.

4) There is no discussion on the role of the burial-nutrient feedback and how burial-nutrient feedback may affect the results of this study. On page 10, l221, it is mentioned that CaCO3 compensation is not included. However, this study does also not consider how changes in iron fertilization affect the balance between weathering and burial of organic matter. This also applies to some extent to the experiment with the increase in whole ocean nutrient inventory.

Several studies point to the potentially important role of the ocean/sediment/lithosphere fluxes of organic matter and how the associated burial-nutrient feedback modifies the magnitude and time scales of the response in CO2 and other tracers to changes in the marine biological cycles (Wallmann et al., 2016;Roth et al., 2014;Jeltsch-Thömmes et al., 2018). (Tschumi et al., 2011), for example, quantify the implication of ocean-sediment-lithosphere coupling for an experiment where the ocean P inventory is increased. (Menviel et al., 2012) present results from factorial experiments with altered iron fertilization/dust input and altered P inventory plus variation in other drivers from transient glacial-interglacial simulations. I suggest that this caveat is addressed on page 10 and perhaps also in the discussion section.

Minor and technical comments:

1) P1, line 11, p3, l46: ".. due to sea surface cooling" What matters is in my opinion the cooling of the whole ocean, including the ocean interior. Please modify the wording

2) P1, l16-18: This sentence is not so clear. The circulation changes itself likely induce a change in the efficiency of the biological pump (Volk and Hoffert, 1985) as may also be seen when looking at preformed/remineralized nutrients or AOU. I think it should rather read "whereas the other half is driven by iron fertilization and an increase in whole ocean P inventory" or similar.

3) P5, l90: Is convection included in the offline model and how is this done?

4) P9, l192, You may also refer to (Menviel et al., 2012)

5) P8, l182: missing word: "shortwave radiation"

6) P10, l207: you may include here EMICs results (e.g. (Muglia et al., 2017;Parekh et al., 2008;Menviel et al., 2012;Heinze et al., 2016).

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