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Interactive comment on "CO_2 drawdown due to particle ballasting and iron addition by glacial aeolian dust: an estimate based on the ocean carbon cycle model MPIOM/HAMOCC version 1.6.2p3" *by* Malte Heinemann et al.

Anonymous Referee #3

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Heinemann et al. introduce a parameterization of the ballasting effect in the MPIOM/HAMOCC ocean model. This effect contributes to accelerate the export of POC (by reducing remineralization rates) and has the potential to strengthen the marine biological carbon pump, with consequence for atmospheric CO2 concentrations. Furthermore, the study investigates the consequences of enhanced Fe supply to the ocean on global export production during the last ice age (Martin hypothesis). The sensitivity experiments suggest that both effects only entail a rather limited (i.e. 12 ppmv) effect on atmospheric CO2, certainly leaning towards the lower end of available

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estimates from the literature.

This contribution is certainly both stimulating and timely and will certainly be of interest to the climate science community. I have to say, however, that the conclusions are somewhat weakened by the reduced sensitivity of the model to increased Fe availability. As mentioned below (last point), I would urge the authors to reconsider the modern Fe budget, which would allow the argumentation to be more relevant and certainly more convincing.

I'm not a climate modeler and as such have mostly concentrated on commenting the paleoclimatic/biogeochemical aspects of the manuscript. My comments are listed below -

As far as I understand the model set up does not account for the T-dependency of the remineralization length scale.

General comment -

As shown by Kwon et al., 2009 (NGeo), the most important parameter accounting for enhanced sequestration of CO2 into the ocean interior results from the redistribution of remineralized carbon from intermediate to bottom waters. In essence, the depth at which POC is being remineralized is not critical as long as POC respiration takes place at intermediate depths, from which nutrients and CO2 can rapidly be resupplied to the fertile surface ocean, with negligible consequences for atmospheric CO2 concentrations.

However, if the bulk of POC remineralization takes place in the deep ocean cell, then CO2 can be sequestered away from the atmosphere for centuries to millennia. So in essence, if the ballasting effect does not allow POC to be exported to the deep ocean, then one would expect the consequences for atmospheric pCO2 to be small.

I was wondering if you could come up with some sense on how generally colder temperatures characteristic of the LGM in combination with the ballasting effect would

affect atmospheric CO2 concentrations. I understand that adding T-dependent POC remineralization rates would be computationally expensive. But this aspect should at least be discussed in some more details. Maybe you could also consider adding a few sentences regarding the role of dissolved O2 concentration on remineralization rates, since intermediate waters were probably better ventilated/oxygenated during the LGM (e.g. Jaccard and Galbraith, 2012 (NGeo); Galbraith and Jaccard, 2015 (QSR)).

Detailed comment -

p. 1, l. 13 – Köhler et al., 2017 do not present any ice-core CO2 data. Please remove.

p. 2, l. 3 - . . . "enhanced aridity", is probably more adequate that "enhanced desert"

p. 2, l. 3-4 - please add appropriate references

p. 2, l. 16 – please consider citing Hain et al., 2010 (GBC)

p. 11, l. 24-25 – please note that this observation is consistent with paleoceanographic observations, which suggest enhanced export production in the South Atlantic during the LGM as a result of Fe-bearing dust fertilization (e.g. Kumar et al., 1995 (Nature), Martinez-Garcia et al., 2014 (Nature), Anderson et al., 2014 (Phil. Trans. R. Soc.)). Furthermore, using stable nitrogen isotopes as a proxy for the relative nitrate consumption by phytoplankton, Martinez-Garcia et al., 2014 (Nature) showed that the biological carbon pump was not only stronger but also more efficient, in line with the argument outlined here.

p. 14, l. 8-10 - As mentioned above, there is ample evidence suggesting enhanced export production in the Subarctic Zone of the Southern Ocean as a result of Fe-fertilization (see reference above), including outside of the direct influence of the Patagonian dust plume (e.g. Lamy et al., 2014 (Science). I am somewhat surprised that the model is not able to reproduce the paleoceanographic evidence.

p. 15 – I'm a bit puzzled by the final remarks. In essence you imply that Fe concentrations are too high in your control run, in part to the shortcomings associated with the

study published by Mahowald et al., 2006. As a consequence, adding Fe to simulate glacial conditions will not entail much of an effect on atmospheric CO2 concentrations. This certainly weakens the conclusions of the sensitivity study. Wouldn't it thus be possible to include model runs including the downscaled modern dust input?

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