Response to Anonymous Referee #3

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 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.

As shown in Segschneider and Bendtsen (2013) for a HAMOCC global warming experiment, the inclusion of T-dependent remineralization has a more complex impact on the carbon sequestration than one would expect from a simple remineralization depth scale change (reduction for warming, increase for cooling). Compensating effects due to changes in remineralization and hence euphotic layer nutrient supply -- driving changes in primary production -- and further complication due to shifts in the ecosystem (opal vs. calcite producers) and resulting changes in surface alkalinity and hence CO_2-fluxes make it it non-trivial to make any statements on the potential magnitude of including T-dependent remineralization on atmospheric CO_2. Segschneider and Bendtsen were planning to perform corresponding experiments for a glacial ocean setup, but due to some unforeseen developments this has not materialized.

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)).

We would prefer to address this point later, when we actually have the glacial ocean set-up, rather than to speculate here. But we can add a brief statement to the discussion that one should keep this in mind.

Detailed comment

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

The 80ppm pCO_2 difference between the early Holocene and the LGM was estimated from the CO_2 data spline presented in Fig. 1a of Köhler et al. (2017). For that time period, the spline is based on data from the WAIS Divide Ice Core; we will add the reference pointing directly to this data in the revised manuscript (Marcott et al. 2014).

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

We will clarify: "… enhanced desert dust production and enhanced glacigenic dust production."

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

We will clarify that these are also results of the modelling studies referred to in the previous sentence (in particular, Mahowald et al., 2006).

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

Thanks, the reference will be added to the list.

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.

We will add those results to the discussion. Thank you for pointing them out to us.

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.

Yes, we were also surprised and somewhat disappointed by that result (see response to your next comment). The disappointment turned into our motivation to fix this issue by using a more recent dust deposition field.

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?

Understanding this may require a bit of a historical background: When starting our model development, we were not really aiming at an investigation of the iron fertilisation effect on glacial pCO_2. Due to the standard model setup, however, in which dust is a source of iron, any change in the dust input intended to estimate the ballast effect on dust driven glacial pCO_2 decrease, will likewise have an effect on the amount of iron from the same dust input field. Therefore, we had to single out the effects of glacial dust on iron fertilization and enhanced settling velocities. And only then it turned out that the biological production was nowhere iron limited in the standard HAMOCC version. Likewise, we (both the authors of this study, and the model developers at MPI) were limited to the Mahowald et al. 2006 dust fields, as they were the only ones available with LGM/modern (and future) fields.

As discussed in our general response to all reviewers, we are currently working on the implementation of a more recent dust deposition reconstruction by Albani et al. (2016), which is expected to lead to iron limitation of phytoplankton growth rates in the simulated Southern Ocean, in line with modern observations. However, this development will take several months at least. And, because the lack of iron limitation occurs in both control simulations with and without ballasting and not only within the sensitivity runs, including the new dust field or using a dust field that is scaled down would require the repetition of the control simulations and of the sensitivity runs, and the release of a new standard version of HAMOCC, which we think is beyond the scope of this paper. We will clarify that the main scope of this manuscript is the description of the ballasting parameterization and the estimate of the LGM dust ballasting effect on atmospheric CO_2.

References

Marcott, S. A., et al..: Centennial Scale Changes in the Global Carbon Cycle During the Last Deglaciation, Nature, 514, 616–619, https://doi.org/10.1038/nature13799, 2014.

Segschneider, J., and J. Bendtsen (2013), Temperature-dependent remineralization in a warming ocean increases surface pCO_2 through changes in marine ecosystem composition, Global Biogeochem. Cycles, 27, 1214–1225, doi:10.1002/2013GB004684.