Geoscientific
Model Development
Discussions

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.

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First, we would like to thank all three referees for the work they put into reviewing this manuscript, and for their helpful comments and suggestions.

Before responding to the individual reviewer comments, we would like to discuss an issue that all three reviewers criticized, namely, the fact that in the particular version of MPIOM including HAMOCC that we used for this study, which we were restricted to because it was the default version of MPIOM available to the MPI-ESM community

at the time we started our experiments, the growth of non-diazotroph phytoplankton is limited by nitrate everywhere, and nowhere by iron. Only cyanobacterial growth is limited by iron availability. As pointed out in the manuscript and by the reviewers, there is ample evidence for the existence of iron-limited areas in the modern ocean. Our model results with respect to iron fertilization, and in particular the presented quantitative comparison of the iron fertilization effects to the effects of particle ballasting by LGM dust on atmospheric pCO2 are therefore biased (the LGM iron fertilization effect is likely underestimated).

Despite this bias, the iron fertilization results are still consistent within the model world. And we think that the presented effect of iron fertilization on cyanobacteria, which otherwise may not have been so dominant, is still relevant. We therefore would rather not follow the suggestion of the first reviewer to remove the iron fertilization results from the manuscript. However, we do agree that the focus of this manuscript should be the description of the ballasting parameterization and the ballasting effect due to enhanced LGM dust deposition, and we will clarify this in the revised manuscript.

In response to the presented lack of iron limitation, and following the reviewers' suggestion to test the iron fertilization results using a more up-to-date dust deposition estimate, we are currently working together with the HAMOCC developers at the MPI for Meteorology on the implementation of the recent dust deposition fields by Albani et al. (2016, GRL, http://dx.doi.org/10.1002/2016GL067911). Given the lower dust deposition rates of this recent estimate especially compared to Mahowald et al. 2006 (which is the estimate we used in this study; Figure 1), we anticipate that phytoplankton growth will again be iron limited in the Southern Ocean. However, this implementation and model re-tuning takes time, and ideally will happen synchronized with the developments at the MPI for Meteorology (to allow for a comparison of our results within the BMBF PalMod project, which provides the funding for this work). Even when the model adjustments to the new dust forcing are done, we would still need to re-do not only the LGM dust sensitivity simulations, but all the presented simulations, including the model spin-ups

with and without ballasting, because the problem (lack of iron limitation) occurs in both control simulations. This will take several months, with uncertain outcome.

Hence we think that, for this technical development manuscript, it is more appropriate to stick with the simulations as they are, to focus on a better description of the ballasting parameterization and effects, including an improved comparison to observations, and to clearly discuss the bias of the iron fertilization results, but to leave the complete repetition of all simulations with a future model version to a future study.
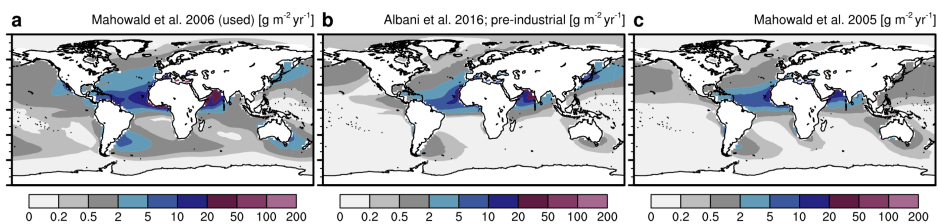
Fig. 1. Comparison of annual mean dust deposition estimates a) by Mahowald et al. 2006 (used here), b) by Albani et al. 2016 (future MPIOM versions?), and c) by Mahowald et al. 2005.