

Interactive comment on “CO₂ 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 #1

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This manuscript by Malte Heinemann et al. introduces a new parameterization of the ballasting effect in the MPI-OM/HAMOCC ocean model. This effect, in which sinking dust particles accelerate the soft tissue pump carbon export, has until now not been included in iron fertilization estimates of LGM dust. It is therefore a very welcome development. However, the convoluted (and ethically questionable) way the authors force an iron limited Southern Ocean makes the iron fertilization results very unbelievable. In addition, there is no way to estimate the robustness of the ballasting results presented here as there is no sensitivity analysis or uncertainty estimation. For these reasons I

C1

cannot support the publication of this manuscript in its current form.

Major Comments:

The estimation of the ballasting effect was performed using only the Mahowald et al., 2005 dataset. I guess that for a theoretical study on this effect, any dust flux dataset will do, even an outdated one. But what would have happened if the authors used a different dust flux dataset, would the results have been 20 ppm pCO₂ drawdown due to ballasting, or 1 ppm? To get a feel for the uncertainty of the results, the authors should either use several different (and recent!) dust flux datasets, or included a sensitivity analysis (e.g. 2x and 0.5x the Mahowald 2005 dust fluxes).

Figure 4(a): Even after 4,500 years the iron fertilization has not yet reached an equilibrium state for the atmospheric pCO₂. Could you discuss that in chapter 4.4? Is there some long-term ocean feedback?

Page 15, lines 3-10: let me get this straight: Your model doesn't reproduce Southern Ocean iron fertilization using the Mahowald 2006 dust fluxes and you therefore conclude that the Mahowald 2006 dust fluxes are overestimated? And instead of including the updated version of that dataset (Albani et al., 2014), you decide to include data that you like better from an older paper from 2005, which itself is based on old model studies from 2003 and 2004? That is very sketchy. Maybe the model you are using is just bad at reproducing nutrient limitation and shouldn't be used at all for iron fertilization studies? I suggest that the authors either perform the simulations again with up-to-date estimates of dust fluxes using an updated version or a different model, or that they remove any mention of iron fertilization from the text and only discuss the ballasting effect.

Minor Comments: page 11, line 5: There are many black lines in Figure 4. Page 11, lines 30-31: The authors argue that primary production is reduced over many ocean regions because of nitrate depletion due to increased particle sinking speeds. I would add here that this is important in nitrate-limited zones. In fact, it would be interesting to

C2

compare the relative strengths of this effect to the main ballasting effect.

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