Response to Anonymous Referee #1

"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."

There seems to be a misunderstanding. We do not force the Southern Ocean to be iron-limited. Quite the contrary - the Southern Ocean in our model study is *not* iron limited because we do use the more recent Mahowald et al. dust forcing from 2006, which is the default in the model version used. The decision to return to the older dust deposition reconstruction temporarily in later HAMOCC versions to achieve a more realistic iron limitation in the Southern Ocean was taken within the HAMOCC development group at the MPI for Meteorology. We do not use these later model versions; we only wanted to clarify that, if one of the later model versions with an iron-limited Southern Ocean is used (in a hypothetical future study by ourselves or somebody else), the simulated ocean CO_2-uptake in response to an iron addition in the Southern Ocean will likely be larger. We will emphasize in the revised manuscript that we did not use the version with the Mahowald et al. (2005) dust deposition rates (see also response to major comments (1) and (3) below).

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

We think that the suggested sensitivity study is beyond the scope of this technical development paper, and that it is sufficient to warn the reader about the overestimated iron availability in the Southern Ocean in our reference simulation, and to discuss the potential underestimation of the effect of enhanced LGM iron deposition in this area.

Major Comments:

(1) 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 I guess that for a theoretical study on this effect, any dust flux dataset will do, even an outdated one.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 include a sensitivity analysis (e.g. 2x and 0.5x the Mahowald 2005 dust fluxes).

We agree with the referee that, in retrospect, it would have been better to use a more recent dust deposition estimate. In fact, we are currently working on the implementation of the recent estimate by Albani et al. (2016; see Figure below). We would also like to know how different our results would be if we had used the more recent dust deposition estimate. However, testing this sensitivity would basically mean that we have to re-do not only the LGM dust sensitivity simulations, but all the presented simulations, including the model spin-ups with and without ballasting. Changing the dust deposition fields will likely require re-tuning of the cyanobacteria production and will lead to a different model setup also for the control simulation without ballasting. As discussed in our general comment to all reviewers, we think that the repetition of our simulations with updated dust fields is therefore beyond the scope of this paper.

Simply scaling the LGM dust anomaly by a factor of 0.5 or 2 would be an easier-to-achieve sensitivity study, but the meaning of the results would be similarly questionable, since the problem of too high iron availability in the control simulation would persist.

(2) 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?

We extended all sensitivity simulations by another 2000 years, but even after 6500 years the ocean in the LGM iron run keeps taking up more CO_2 than in the reference run with modern dust/iron.

We attribute this long-term trend to a continuously reduced PIC/POC ratio of the export production relative to the reference simulation, and hence a continuously reduced export of alkalinity, while the PIC/POC ratio in the LGM ballast simulation increases again over time due reduced primary productivity in response to nitrate depletion (Fig. 2).

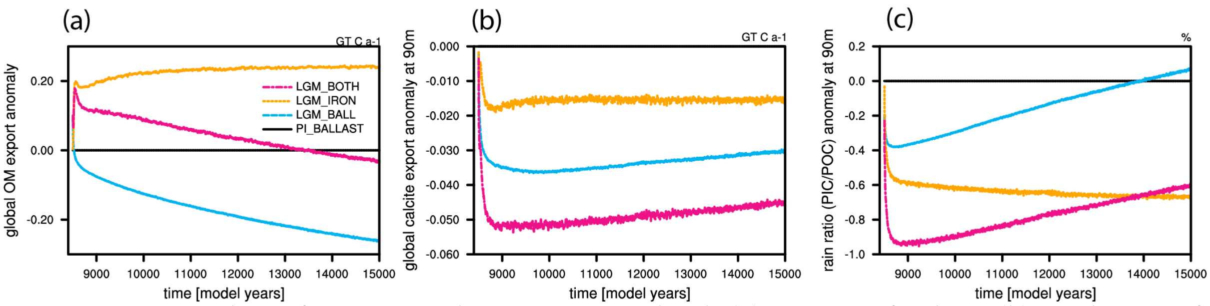

Figure 2: Anomalies of export production at 90m depth (a), export of calcite (b), and ratio of calcite versus organic matter (PIC/POC) for the simulation with LGM dust as ballast (LGM_BALL), with LGM dust for iron fertilization (LGM_IRON), and LGM dust for both (LGM_BOTH).

Note that long-term trends can also arise if the sediment burial fluxes of organic matter, calcite and opal are not balanced by the weathering fluxes – which we did not adjust in the sensitivity simulations. We will discuss these trends in more detail in the revised manuscript.

(3) 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.

Again, there seems to be a misunderstanding. We did not use the dataset from 2005. We only wanted to point out that, if the older dataset was used, the Southern Ocean would again be iron limited. The Southern Ocean in our model is not iron limited, because we used the relatively newer dust deposition product.

That said, the most recent dataset by Albani et al. (2016) looks more similar to the 2005 data than to the 2006 data (see Fig. 1 in AC1 / general comment to all reviewers).

As discussed in the general comment to all authors, we would rather not remove the iron results, because the cyanobacterial response that leads to the CO_2 drawdown is still at least consistent within the model; although the lack of iron fertilization in the Southern Ocean is not in line with observations.

But we will clarify in the revised version of the manuscript that the focus of this study is the introduction of the ballasting parameterization, and not the iron results.

Minor Comments:
page 11, line 5: There are many black lines in Figure 4.

We will change the text to 'black line in Fig. 4a'.

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

We agree that the effect of nitrate depletion is only important in nitrate-limited zones; however, in our model, the entire surface ocean is nitrate limited (page 14, line 13). Hence, the effect can play a role everywhere. We will point this out again in the revised manuscript. If any parts of the surface ocean were limited by phosphate, then the accelerated phosphate export due to higher sinking speeds would likely also lead to a reduced primary production in those areas.

Since the "main ballasting effect" is exactly the acceleration of particles, including particulate nitrate, it is unclear to us what is meant by separating the two effects. Did the comment aim at diagnosing the effects in different locations (i.e., nitrate depleted areas only versus other areas), or at performing new simulations, e.g., somehow keeping the NPP or nitrate export constant for the LGM ballasting sensitivity experiment? But then again, the constant NPP will also affect particle ballasting. Maybe the reviewer can elaborate?