

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 #2

Received and published: 12 September 2018

The manuscript by Heinemann et al., describes the addition of a ballasting parameterisation within the MPI-OM/HAMOCC model and is used to quantify the contribution of ballasting to glacial-interglacial changes in CO₂ associated with changing dust fluxes. The authors find that ballasting by dust particles has a smaller drawdown of atmospheric CO₂ compared with the effect of iron fertilisation when forced with glacial dust fluxes. I think this is a really interesting question to explore as there has been comparatively less focus on processes affecting organic carbon fluxes in the ocean interior than on the effects of iron fertilisation. However, I think it's difficult to reach a satisfy-

[Printer-friendly version](#)

[Discussion paper](#)



ing answer because the iron fertilisation effect in these experiments does not occur in the Southern Ocean as generally understood by the iron hypothesis. The authors are open about this in the manuscript but ultimately I think this limits the findings. I have detailed a number of comments on this as well as the ballasting parameterisation and sediment model below. If the authors are able to address this key issue then I think the manuscript would be suitable for publication.

General Comments:

The modelled iron fertilisation effect in the model does not occur in the Southern Ocean as understood by the iron hypothesis. This has a number of issues for interpreting the results. Firstly, CO₂ drawdown associated with export production varies by location (DeVries et al., 2012) and therefore the CO₂ sensitivity for the iron fertilisation experiments may not be comparable. The sensitivity falls below the cited range in the introduction (8 ppm vs. 15–40 ppm). Secondly, changes in ballasting and sinking rates will lead to changes in nutrient distributions which could potentially enhance or reduce any export production changes associated with iron fertilisation. For example, an increase in export production with iron fertilisation may be reduced if ballasting increases sinking speeds locally relocating nutrients within the water column. For these reasons, I think the comparison of CO₂ changes is hard to interpret fully.

The description of the ballasting scheme, its appropriateness and impacts needs better description overall. The scheme from Gehlen et al., (2006) assigns a single sinking rate to all particle types according to the average excess density particles. While this scheme has been used previously, I think a few things need discussion: this scheme assumes a key role for particle aggregation (this is really a ballasting and aggregation parameterisation) and that this scheme differs considerably from other ballasting schemes used previously, (Howard et al., 2006; Hoffman and Schellnhuber 2009). Given the significant impact on opal sinking rates, I think this needs some thought. Additional figures, such as Taylor diagrams showing statistical fits for the new and old scheme versus observations would help assure me this scheme is working well. Please

[Printer-friendly version](#)[Discussion paper](#)

also state all the units when describing the ballasting parameterisation.

The inclusion of sediments here is not well described or justified. The experiments don't seem to have reached a steady-state (e.g., Figure 4a), is this because the sediments are still responding? Depending on the processes in the sediment model, there could be different responses to iron fertilisation and ballasting as ballasting will affect the ratios of particulate matter reaching the seafloor (e.g., Ridgwell 2003). Would it be possible to isolate and quantify the effect of sediments on the CO₂ drawdown?

Specific Comments:

Pg 2, lines 20 - 30: The citations for dust/lithogenic ballasting seem limited to only a few papers (Klaas and Archer 2002; Dunne et al., 2007) with a lack of more recent papers focussing on observed effects.

Pg 3, line 14: I am not sure the experiments here can be called equilibrium experiments as atmospheric CO₂ still seems to be changing in Figure 4a, and as also mentioned at the bottom of page 5.

Pg 3, line 33: The description of the box model of atmospheric CO₂ referred to here is quite limited. The description later on might be better located here.

Pg 4, lines 3-5: This is quite a lot of description of the grid-setup, does it have implications or relevance for the interpretation of the results?

Figure 2: It might be helpful to also see the global flux profile, e.g., a Martin Curve equivalent, to get a handle on how the sinking speeds contribute to changes in particulate fluxes.

Pg 7, lines 9-10: a change in the sinking rate for opal from 30 m day⁻¹ to 5 m day⁻¹ is quite dramatic. I would like some discussion about this change, e.g., how does it compare to values in literature and other models? Is this scheme better because of the explicit use of density or are there other things missing? Adding some summary plots about different tracers (see general comments) would also help clarify the impact

of this change.

Pg 8, lines 15-20: no quantification of opal export here

Pg 8, lines 26-29: As I understand, the sediment trap data presented in Honjo et al., (2008) is normalised to 2000 m using the Martin curve on the basis that gravitational settling is the dominant process at this depth. The data here is reported at 1000 m. Did you apply the same normalisation and if so can the same assumptions apply at this depth?

Figure 3: What causes the transfer efficiency pattern in the standard model (panel k)? From the previous description, it seems like this should be globally uniform.

Pg 10, line 6: I think the comparison between the ballast scheme here and Weber et al., (2016) is unwarranted as this is not the focus of the manuscript. The Weber analysis derives from an inversion of nutrient distributions and so represents the net effect of any number of potential processes. Any differences might therefore reflect the importance of other processes other than ballasting in some regions.

Pg 11, line 7: I am unfamiliar with this approach to modelling atmospheric CO₂, where does 2.1 Gt C / 1 ppm relationship derive from?

Figure 4: The CO₂ drawdown for the iron fertilisation (8 ppm) is lower than the published range mentioned in the Introduction (15 - 40 ppm). This needs some discussion, see also general comments.

Pg 14, lines 1-3: Does the weakening of the calcite export reflect a shift towards silicifying organisms? If so, does this also have an effect on ballasting sinking rates? i.e., is there a dual effect of ballasting from dust and from opal? I think these effects are quite interesting!

References

DeVries et al., (2012) The sequestration efficiency of the biological pump. Geophysical

Research Letters. 39 (13)

Hofmann and Schellnhuber (2009) Oceanic acidification affects marine carbon pump and triggers extended marine oxygen holes. PNAS. 106 (9)

Howard et al., (2006) Sensitivity of ocean carbon tracer distributions to particulate organic flux parameterizations. Global Biogeochemical Cycles. 20 (3)

Ridgwell (2003) An end to the "rain ratio" reign?. Geochemistry Geophysics Geosystems. 4 (6)

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2018-137>, 2018.

GMDD

Interactive
comment

Printer-friendly version

Discussion paper

