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Climate of the Past Discussions

Interactive comment on "Effects of eustatic sea-level change, ocean dynamics, and iron fertilization on atmospheric $p\mathrm{CO}_2$ and seawater composition over the last 130 000 years" *by* K. Wallmann et al.

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Wallmann et al. present box model simulations of the last glacial CO2 cycle under prescribed forcings of changes in sea level, oceanic circulation, and iron fertilization. This is a follow-up of the study by Wallman (2014). In the previous study, he explored a hypothesis that response of POC burial to sea level changes forms a strong positive feedback between sea level and CO2. Under certain conditions, a box model in that study revealed self-sustained oscillations without any need for external (e.g. orbital) forcings to explain glacial cycles. The current study is less ambitious and doesn't put

significance of the orbital forcing in question. The ocean model is extended from 3 to 24 boxes with more realistic resolution of major ocean basins. Careful tuning of oceanic circulation as well as a thoughtful selection of external forcings lead to much better matching of ice-core CO2 than in the previous paper.

My main critical remark is about motivation of this study. Why to invest huge intellectual effort into making box model more realistic? Since circulation between model boxes is prescribed by hand, it cannot be more realistic that in a model with explicit ocean dynamics. In my view, the usefulness of box models is limited to conceptual studies. The paper by Wallmann (2014) has already illustrated the power of the POC hypothesis. It was also shown that abrupt CO2 growth during deglaciation could be reproduced if vertical mixing is allowed to relax instantaneously to its Holocene value at glacial terminations: abrupt forcing leads to rapid CO2 changes. Similar link of fast changes in ocean dynamics to abrupt deglacial CO2 rise is found and highlighted in the paper under review. What really new can we learn from the improved model study? The novelty should be more clearly presented in conclusions and in the abstract.

Another problem with discussion of the box model results is that it is difficult to avoid circular logic. Some results are straightforward consequences of model assumptions. For example, the abstract says "the other half of the glacial drop of CO2 was linked to reduced deep ocean dynamics". The slowdown of glacial deep ocean is an assumption of the model, and there is no surprise in higher DIC content of the deep ocean and lower atmospheric CO2 in that case. The authors should clearly indicate which results are direct implications of their assumptions and which ones are novel, non-trivial consequences of interactions between biogeochemical components.

Regarding the POC hypothesis, there are still few conceptual questions, which require more detailed discussion in introduction. Firstly, why terrestrial plants cannot utilize sediment nutrients after the shelf exposure? Tree roots could be many meters deep. Secondly, increased nutrient inventory and utilization reduces the oxygen content. How good is the box model in reproducing oxygen minimum zones at present? Since ocean

boxes are huge, do they represent the oxygen limitation in a plausible way?

Minor comments.

- p. 2406, l.15-17. Who are “they”? Sea-level changes? Does weathering add depleted 13C or 14C to the ocean? Changes in 14C are rather a signal of overturning and water masses changes than a result of sea-level changes.

l.19: What does “reduced deep ocean dynamics” mean: slower overturning? Do we have a proxy for it? The Atlantic overturning was shoaled, but was it slower? How do we know that “transit time” was longer – in the whole ocean? Is it the model outcome or is it the model assumptions? It is written like it is a fact – but is it a model truth or a data truth? If it is an artifact of the model setup, should it be highlighted in the abstract?

l. 28: terrestrial biosphere is not accounted for in the study. Can we consider the the Holocene dynamics without accounting for the biosphere regrowth and peat accumulation on land?

P. 2407, l.1-110: How could a decline in iron deposition lead to an increase in atmospheric CO2 by 12-13 ppm in few decades? This corresponds to a source of about 30-40 GtC from the ocean. What can cause a sustainable flux of 1-2 GtC/yr from the ocean?

P2409, l.24-29 constraining glacial water fluxes based on d13C: this process should be explained in more details. Was it optimization of parameters?

P2413: C:P ratio in POM is roughly 100:1, while it is about 50:1 for soils and 1000:1 for wood biomass. Trees on exposed tropical shelves could store much more C than marine sediments, and the net effect on atmospheric CO2 would be rather neutral.

Figure 2: numbers are not readable.

Fig. 3h, POC weathering: is river POC flux included? If so, is this POC originated from shelves or from internal continental area? Exposed shelf area goes to 0, but POC flux

from exposed area is not 0: please explain.

Figure 7: What mechanisms are responsible for the pCO2 drawdown from 10 to 8 ka? A usual interpretation is that this CO2 drop is an effect of terrestrial carbon uptake.

Figure 8: Please add a plot of difference LGM to PRE, otherwise it is difficult to see how exactly the P and O2 pools are re-distributed at LGM. Also, add a plot of observations for PRE as averaged on the model resolution from the data.

Figure 9: Please add a plot of difference LGM to PRE.

Figure 10a, black line: the model shows a decrease in carbonate ion concentration during deglaciation. This would lead to a CaCO3 dissolution spike during deglaciation, opposite to what was observed.

Figure 11: plot a difference LGM to PRE.

Figure 13: please add arrows of shifts from STD to STD LGM. Is there added value of showing CC and CC-CN LGM versions?

Figure 14: a nice conceptual plot, but arrows are very selective. Why there is no direct effect of insolation on climate (temperature) and circulation (via SST/precip pattern)? Also, sea level changes have a direct effect on circulation eg through the Bering straight.

Interactive comment on Clim. Past Discuss., 11, 2405, 2015.