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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.

Anonymous Referee #2

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Explaining glacial-interglacial changes in atm. CO2 and climate remains a difficult problem. The consideration of the space-time evolution of various proxies in transient, prognostic simulation provides one way forward to improve our understanding. Transient simulations including ocean-sediment-weathering interactions were until recently only feasible with box models as applied here. More recently, 2-d and 3-d ocean circulation models coupled to simplified atmospheric representations were used to explore glacial-interglacial CO2 dynamics.

A problem for any such study is that our mechanistic understanding for example of

the processes that control the transfer of nutrients, carbon, alkalinity, silica etc from land to ocean, or the processes that control burial, or Southern Ocean circulation are highly limited. The authors appear overly confident that their box model yields the right answer as evidenced by their statement at the beginning of the discussion: "we are confident that the major conclusions drawn from our study are robust, since the structure of our model is based on sound geochemical principals". It is then perhaps no surprise that the authors do not discuss potential shortcomings of their setup nor compare their results with the findings presented by other studies relying on transient simulations with dynamic ocean circulation. I also question somewhat the usefulness of a 24-box model to address the impact of ocean circulation changes on atm. CO2. In this sense, I miss a discussion with a critical assessment of the model and its results.

A few more comments, in addition to those already raised by Victor Brovkin, here below

1) a) The authors are asked to perform a simulation where atmospheric d13C is not prescribed but evolves freely for the standard setup and for the setup with constant circulation (i.e. without tuning to ocean d13C, see point b). By prescribing atm. d13C, the ocean d13C signature is also forced through air-sea gas exchange; the comparison of measured and "simulated" d13C-DIC in table A5 appears therefore not very relevant. Simulations where d13C is not prescribed but simulated would allow the reader and the authors to gauge whether the proposed mechanisms are consistent with the atm. and oceanic d13C proxy records within the context of the box model. Ocean d13C-data of DIC are available for the last 130 ka (e.g., Olivier et al.). Atmospheric d13C data provide one constraint for models of glacial-interglacial CO2. D13C data are available over the past 20,000 years (Schmitt et al, Elsig et al,) for the penultimate deglaciation and the Eem (Schneider et al.) and emerging for the last glacial period.

b) 2409, line 27: LGM water fluxes were tuned to match LGM d13C distribution. It is not clear to me how this tuning was performed. Was the tuning done for modern model conditions, i.e. under the assumption that long-term ocean-sediment interaction, iron fertilization, temperature changes etc do not affect d13C. Or was the tuning done using full transient runs with all forcings applied to take into account the long time scales in the system? This tuning may be problematic as it makes it impossible to evaluate model performance and the realism of the proposed mechanisms by using atm. and oceanic d13C as a constraint.

2) a) A key mechanism of this manuscript is the change in POM burial in response to sea level changes. Here, burial of C is modelled by scaling export production with the area covered by different depth intervals and assuming a time-invariant burial efficiency. In contrast, burial efficiency of P is in addition modulated by oxygen in the water column (not in the sediment layer). This raises the questions (i) to which extent this 24-box model can meaningfully simulate oxygen concentrations (ii) to which extent oxygen in the water column is a proxy for oxygen in the sediment layer. Finally it is controversially discussed whether and to which extent such a fractionation between the burial of C and P indeed occurs (Anderson et al., 2001) and it would be worthwhile to mention this.

b) There are alternative mechanistic formulations to describe the diagenetic processes and sediment models that describe the transport, dissolution and burial of biogenic particles as well as the transport of solutes within the active sediment layers (Heinze et al., 1999;Gehlen et al., 2006) and such models are applied to study ocean-sediment interactions on the glacial interglacial time scales, including the possible role of changes in burial, a whole ocean nutrient increase, iron fertilization, or ocean circulation (e.g., (Brovkin et al., 2012;Brovkin et al., 2007;Matsumoto et al., 2014;Menviel et al., 2012;Roth et al., 2014;Tschumi et al., 2011;Lambert et al., 2015) and other). The findings of this study should be compared to the findings of these and similar earlier studies that discussed burial-nutrient-pCO2 feedbacks and glacial-interglacial CO2 variations.

c) I am confused about the role of POM weathering. On page 2424, line 20 it is stated that POC weathering increases due to expansion of shelves and the increase appears to be displayed in Figure 3h that shows global rate of POC weathering calculated from

exposed shelf area . From Figure 3h, I estimate that this additional weathering leads to an input of about 2000 – 3000 GtC and presumably similar amounts of P and N. Is this realistic? This amount corresponds roughly to the carbon stored in today's soils. I note that the model does not account for the growth of plants on exposed shelves that would supply such a flux by photosynthesis. How does an increase in the amount of POC weathering during glacial times compare with land area covered by ice and the generally smaller productivity and carbon pools during glacial times compared to interglacials?

d) page 2413, line 2 it is stated: "Neglecting the glacial increase in the weathering of P-bearing solids is raising the LGM pCO2 value by 50 ppmv." In other words, about 50 ppm of the glacial-interglacial CO2 difference are attributable to an increase in P weathering. What is the evidence for this increase?

3) The burial mechanism applied leads to a large in increase in atm. CO2 over the Holocene. How realistic is this given that the model does not include changes in terrestrial carbon storage and implied carbonate compensation and transient changes in the lysocline (Broecker et al., 2001)?

4) It would be useful to clearly outline which data are used for model tuning and which data are used for the a posteriori evaluation of results.

5) Given the cost-efficiency of a 24-box model I miss a comprehensive variation of model parameters to assess how uncertainties in model parameters affect results

6) Figure 8, 9, and 11 should be changed. The contouring suggests higher model resolution than provided by a 24 box model and is misleading. It would be more appropriate to show the colors on the model grid given in Figure 2 without any interpolation.

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