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Interactive comment on "An investigation of carbon cycle dynamics since the Last Glacial Maximum: Complex interactions between the terrestrial biosphere, weathering, ocean alkalinity, and CO_2 radiative warming in an Earth system model of intermediate complexity" *by* C. T. Simmons et al.

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The manuscript by Simmons et al. is devoted to an important issue of climate-carbon cycle interactions during the last 23 thousand years. Mechanisms responsible for the 100-ppm CO2 increase during deglaciation have been identified during the last two decades. Still, while we know the main processes behind the CO2 increase, their

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particular role and the strength differ from model to model. In particular, differences among the box, 2-D and 3-D models of the ocean biogeochemistry are particularly striking.

The experiments done by Simmons et al. are of especial importance as the UVIC model has 3-dimensional ocean model. The experimental design of their study is quite complex, and it is not that easy to follow the logic of experiments in the discussion section. My main suggestion is to restructure the results part, especially the marine section. Another major remark is to include more 2-D vertical plots of marine biogeochemistry, which would justify the results described in the text. In the current version, most of plots are timeseries or surface plots, which do not fully exploit an advantage of 3-d models over the box models.

Specific comments

The title is unusually long. In fact, it is more an abstract than a title: “An investigation of carbon cycle dynamics since the Last Glacial Maximum: Complex interactions between the terrestrial biosphere, weathering, ocean alkalinity, and CO2 radiative warming in an Earth system model of intermediate complexity”. I strongly recommend reducing it, for example, to “An investigation of carbon cycle dynamics since the Last Glacial Maximum using an Earth system model of intermediate complexity”. Besides, the interactive terms in the current title are subjects of different categories: terrestrial biosphere (model component), weathering (process), alkalinity (variable), CO2 warming (process/forcing). It is awkward to mix them into one list/group.

The structure suffers from the same mixture of different categories:

3.1 Atmospheric CO2 - component

3.2 Terrestrial carbon - component

3.3 Physical and Dynamics ocean changes - processes

3.4 Alkalinity response to ocean ventilation - variable

3.5 Sensitivity to Weathering and Carbonate Compensation - process

3.6 The Alkalinity Response to Holocene Terrestrial Uptake - variable

Sections 3.1 and 3.2 are relatively easy to read and perceive, while sections 3.3 – 3.5 are very difficult to read. The main reason, in my view, is that the authors try to focus on particular components/processes based on sensitivity studies, which are too different in terms of processes and their effect on the carbon cycle and climate. For example, all FC experiments have rather low CO2 and, consequently, smaller radiative warming. All PC experiments have much warmer climate, which affects all biogeochemical fluxes. CO2rad CA is the third group of experiments, which differ from the previous two categories of runs.

My suggestion is to restructure the results in accordance the experimental setup in the Table 1. The line of result presentation could be as follows:

Section 3.1. FC experiments: no radiative warming.

- 1st: Changes in the CO2/carbon dynamics in the CA experiment

- 2nd: Sensitivity experiments: effects of higher/lower weathering on CO2, changes in the ocean DIC, terrestrial carbon (if any)

Section 3.2 CO2 rad experiments: prescribed radiative warming, but interactive atmospheric CO2.

- 1st: Effect of radiative change on the ocean/atmospheric circulation, surface climate

- 2nd: Effects of weathering changes on CO2, ocean carbon, terrestrial carbon

Section 3.3. PC experiments: prescribed radiative warming and atmospheric CO2.

- 1st: Changes in the carbon dynamics in the CA experiment

- 2nd: Effects of high weathering changes on the ocean carbon

Changes in the climate and ocean circulation - very important outcomes of the study -

should be discussed in more details and supported by better figures. The meridional overturning in Atlantic should be also shown as a 2D plot (depth/lat) as it is the main advantage of the 3D ocean model over box models. What is a vertical distribution of water masses at LGM vs 15.5 kyr BP? The D14C data could be also shown at the 2D plot (depth/lat). It is an added value to the time series of changes in these quantities.

Sections 2.2-2.3: The ocean volume change at LGM due to sea level drop (roughly 3%) – was it accounted in the LGM and transient simulations? If not, what is the possible effect of sea level change on the atmospheric CO2?

p. 9, l. 7-8: if the ice sheet decay does not produce a freshwater flux into the ocean, what is a reason for the saw-tooth output of the model (eg Fig. 1a,c,d)? It is not discussed in the paper.

p.16, l. 17: could you comment on the effect of an absence of peat/permafrost module in the terrestrial biosphere model?

Section 3.3: This section is very important part of the result discussion, but it needs to be reorganized, otherwise it is extremely difficult to read it. See major comments above.

p. 18, l. 6: “vegetation” should be “vegetation biomass”

p. 19, l. 9: “these simulations lack of freshwater fluxes. . .” – perhaps, without “of”?

Section 3.4 (p.22): l.18-21: Discussion of the effect of circulation changes on DIC and carbonate ion concentration (FC HW and CO2 rad HW) could move into the new section 3.2 (see comment above). 2-D plots of changes in DIC and carbonate ion (Atlantic vs Pacific) are needed to justify discussion of spatial differences in distribution of these species (eg “reduced DIC storage in the deep Atlantic ocean” – how could readers see it without explicit map of 2-D vertical profiles of DIC storage in the Atlantic?). The most interesting is to see at what depth the DIC concentration changed.

Why the authors picked up the HW and not LW experiments (eg p. 24, 1st para)? Any

rationale for this decision? The LW experiments lead to higher CO2 at the end because the ocean alkalinity is reduced.

P.25, l. 9: “sensitivity to weathering”: What does it mean “another important factor”? Weathering experiment was already discussed in the previous section.

l.20. “increase in calcifiers” – could you add a 2-d plot of calcification rate to justify this statement?

p. 26, l. 7: what are “the other runs”? Be explicit.

p. 27, l. 10: What is “this simulation” – FC CA (the last mentioned above) or FC HW?

p.28, l.16-23: “Greater ocean ventilation” etc. – again, this statement should be justified by 2-D plot (depth/lat) of circulation changes

Section 3.6:

p.29, l.20-23, p.30, l.1-13: I do not understand why prescribed concentration simulation is the best illustration of the carbonate compensation effect proposed by Broecker et al. In my view, a discussion of effect of carbonate compensation on atmospheric CO2 requires interactive (FC-type) CO2 simulation. Prescribed atmospheric CO2 changes (first down and then up) are mirrored in the carbonate ion concentration, so I have a trouble with understanding causality in this experiment. The FC experiments in the next para are more insightful, and they basically show no desired effect of CO2 growth during the late Holocene (l. 18-20). Experiments with more extensive ice shelves might be useful, but they also do not show a sustained growth of CO2 from 6 to 0 ka. I miss this point in the conclusions section.

The last para on the sensitivity of alkalinity to the terrestrial carbon uptake (p.31, l. 14-23) seems to be very different from the scope of the section. It is interesting to know an effect of ice shelves on ocean alkalinity, but this should be a separate subsection with a clear title and message.

Conclusions: p.34, l. 13-15: I do not understand the point 1. If the sedimentation rate is higher than the weathering rate, the alkalinity is decreasing and CO2 is increasing, independently on the scale of the weathering rate.

Figures: General – plots of time series are sub-optimal. The label font should be increased, and a grid added to quantify time series values in the middle of the plot. Figure captions are very long. Some of them contain rationale of experiments or details of experimental description, which belongs to the main text.

Figs. 2-3. What is shown on these maps – terrestrial carbon densities? I guess, they should be in units of kgC/m2.

Figs. 6-7. The same unit question as above.

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