

***Interactive comment on* “The [simple carbon project] model v1.0” by Cameron O’Neill et al.**

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Thank you for your constructive and thorough comments, suggestions and input into the manuscript. We feel it makes a very strong contribution to the quality of the work. Please see below our responses to the individual comments. We have made reference to changes to the manuscript, which is included as a supplement to the author comments, in track changes.

Page and line references below refer to locations in the revised document with track changes. Please note the attached, marked-up document contains amendments from both sets of reviewer comments.

RC: This paper is mostly well-written. The model is nicely described and is a genuinely useful framework for investigating physical and biogeochemical controls on the marine carbon cycle. I have no serious concerns with the work and my comments are mostly

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suggestions for rewording and clarification. A few slightly more important issues are below, followed by line-by-line notes.

RC: I did not see a description of the numerical method employed or confirmation of model stability and potential for numerical error. Fig 7 shows some potential error propagation / numerical oscillation? Have the authors investigated this?

AC: the model equations are a set of partial differential equations, one for each element in the model. These are solved with a straightforward 1st order Euler forward time-stepping method with a standard timestep of one year. We find the model to be stable and approaching steady state for most of the simulations we have undertaken. However, as noted by the reviewer, this stability is challenged by scenarios with strong forcing.

Figure 7 of our original submission shows this instability for the extreme emissions scenario RCP8.5. We have re-run this scenario with a reduced timestep (0.5 years) and find that the weak instability in the model results for air-sea carbon exchange, is eliminated. We have also run the other RCP scenarios at reduced timestep, which shows a smoother trajectory for air-sea gas exchange of carbon, shown in the revised Figure 7.

We have added a description of the numerical method (P8, L6).

RC: Figure placement needs significant improvement. For example, Figure 9 appears 5 pages after it is mentioned and is in a different section.

AC: We have revised the figure and table placements. Note some of these are slightly out of place in the marked-up version of the manuscript, but this is resolved in the clean document.

RC. A few times it is noted that the model supports a physical overturning mechanism for driving LGM-Holocene changes, it should be made clearer that this idea has been proposed before and the current work supports it, rather than introducing the concept.

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AC: In the discussion of the LGM-Holocene modelling results, we have made mention of previous findings. This point is addressed specifically in Section 5.3 (P42, L11).

Line-by-line:

Page2: line 15: summary of box models is too vague

AC: We have expanded this summary in the introduction (P2, L16)

Page 2, Line 32: “simple carbon project model model”

AC: offending duplicate removed

RC: Page 3: Line 20: does ‘zonally averaged’ make sense? There are no spatial dimensions here? E.g. later zonally-averaged refers to a 2D model.

AC: We agree that the use of “zonally averaged” when referring to box models and the Talley 2-D conceptual model is confusing. We have removed “zonally averaged” from both instances (P5, L4; P6, L7).

RC: Figure 1. It is not always clear which arrows exchange with which boxes. E.g. some arrows are entirely within one box, some cross box borders but do not terminate. The diagram should show what is actually happening in the model.

AC: we assume this comment refers to the red and orange arrows. We have amended Figure 1 by trimming the arrows to show only where there is a flux between boxes, via shortened arrows that cross the relevant box border, and removing arrows wholly contained within box borders.

RC: Page 5 Line 14 to end: A little confusion over model dimensionality. Be precise here. Explain how the model has no spatial dimensions but does have a representation of sizes and locations of boxes (if that is indeed correct).

AC: We have clarified the relationship between model boxes and spatial dimensions in the real ocean (P6, L16).

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RC: Figure 2. Caption – “implemented”.

AC: Amended

RC: Figure 2. Also explain the direction of arrows here.

AC: We have added the following statement to the caption for Figure 2: “The arrows refer to the direction of file linkages and the order of their activation during the routine of setting up and running the model.”

RC: Page 9: Line 1: Biological flux “parameters” or “parameterization”.

AC: now “Biological flux parameterisation”

RC: Line 2: “action of biological activity” – reword

AC: On page 10 last paragraph (L12), reworded as: “The biological pump (e.g. Broecker, 1982) is a descriptor of marine biological activity, whereby organisms consume nutrients in shallow waters, die, sink and then release those nutrients at depth.”

RC: Line 21: sub-surface or subsurface

AC: sub surface replaced with “subsurface” throughout (e.g. P11, L17).

RC: Page 11: Line17: ‘lending it some interest’, consider rewording

AC: On page 13 Section 2.3.1 reworded as: “According to Farrell and Prell (1989) it is a dynamic process, and the dissolution and burial in sediments of CaCO₃ is observed to vary across (and within) glacial/interglacial cycles), suggesting an influence on carbon cycling”.

RC: Line 34: should be “non-saturation-dependent” ?, “Earth”

AC: Amended as such (P14, L21-22)

RC: Page 13: line 5: replace “extra ocean” with something more descriptive

AC: Section 2.5, page 16 we have replaced with “Atmosphere and terrestrial carbon

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cycle”

RC: line 22: non- zero

AC: Amended (P16, L25)

RC: line 25: short-term, long-term

AC: Amended (P16 and P17)

RC: Page 14: Line 3: state meaning of beta parameter

AC: added to page 17, line 6: “beta is the parameterisation of carbon fertilisation, causing NPP to increase (decrease) logarithmically with rising (falling) atmospheric CO₂ levels, with a typical value of 0.4-0.8 (Harman et al, 2011).”

RC: Figure 4: This is hard to read, consider better ways to display (e.g. title and unit on x axis)

AC: We have revised Figure 4 by reducing the number of subplots shown, as the points can be made with one chart for each of sea surface temperature, salinity and piston velocity. We have also consolidated the y-axes labels to reduce clutter. The subplots now include the modern-day values/assumptions used in the model (P24).

RC: Page 19: Line 7: “the carbon isotopes” reword

AC: Page 25, line 6. Reworded as “fractionates the ratios of the isotopes of carbon leading to higher values for d¹³C and to a lesser extent, D¹⁴C, in the atmosphere”.

RC: Line 12 “the values for the isotopes” too vague

AC: Page 25, line 12 reworded as: “Increasing the fraction of deep water upwelled into the sub polar surface ocean box (Fig. 4(l)) intuitively raises CO₂, but lowers d¹³C and D¹⁴C, by upwelling carbon rich and isotopically-depleted water to the ocean surface boxes.”

RC: Line 17: “response to the shocks” does not give the right impression

AC: We agree, and have altered this sentence to read (P25, L18): “In response, the Earth’s carbon cycle continually partitions carbon between its component reservoirs, with positive and negative feedbacks. The net effect is a build-up of carbon in most reservoirs”.

RC: Figure 5. Explain data in panel b, why are there two data lines for the atmosphere?

AC: this was an issue with the Atmospheric D14C data we have gathered, which has now been rectified in Figure 5 (P26).

RC: Figure 5. Remove “selection of boxes shown to reduce clutter”, ironically this statement is itself clutter.

AC: amended as suggested (P26)

RC: Figure 6. Remove “fed into” from caption. Use inputted or similar.

AC: reworded as “.....which are inputted to SCP-M for the modern carbon cycle simulation.” (P28)

RC: Page 20: Line 17: “carbon cycle destination for human emissions” – not clear what this means

AC: P27, 33: “Figure 8 shows the partitioning of anthropogenic CO₂ emissions into the carbon cycle reservoirs by 2100, as simulated with SCP-M.”

RC: Line 19-20: explain this in more detail, a little confusing

AC: we think it is better to delete this sentence altogether. The point is a bit nuanced and perhaps extraneous. We have changed the chart to show a slightly different metric so the sentence is redundant.

RC: Line 22: the figures are becoming a long way from the relevant text by this point in the paper.

AC: Modified in the manuscript throughout. Note that some of the figures and tables

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are slightly out of place in the marked-up response attached as a supplement (due to the presence of deleted text), however it is resolved in the clean document (without track changes).

RC: Figure 7: use of multiple transparencies and colors here makes it very hard to see the ranges, especially in the bottom panel. Also, it appears there is some oscillation developing in the model? Have you investigated this?

AC: Yes, for the extreme emissions scenario RCP 8.5 there is some numerical oscillation at the end of the simulation. Please see second AC above for our response. The simulations have been run at reduced step size and the model result trajectories are smoother. The Figure is revised on page 29.

With regards to the transparencies for the CMIP-5 model ranges in Figure 7, we have employed a mixture of hatching and infill, to better distinguish the ranges (P29).

RC: Figure 8: This is quite simple, can a comparison be incorporated?

AC: We have incorporated the corresponding model results from the IPCC WG1 5th assessment report (Chapter 6) in pie chart for comparison, and referenced in Figure caption (P30).

RC: Line 22: “release of emissions” should be reworded

AC: P28 L3: Reworded as: “By 2100 in RCP 6.0, the carbon cycle is substantially changed from the preindustrial/late Holocene state as a result of the accumulation of hundreds of years of human industrial CO₂ emissions (Fig. 9).”

RC: Figure 9: I would consider if there is a better way to show this. It takes a very long time to decode this information. Bar or pie charts would be more easily understood.

AC: we acknowledge the comment and agree this is a busy figure. However, we feel that Figure 9, and the information it shows on carbon fluxes between boxes in the model, is intrinsic to the model documentation. This is because it displays flux values

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which we believe are plausible, and that this demonstrates the validity of the model for this purpose (modelling carbon fluxes between the different Earth reservoirs). To help simplify, we have replaced the absolute values for the scenario results (the RCP6.0) with the + or – variation from the baseline scenario, in PgC, to highlight what is changing.

RC: Page 22: Line 8: first line here needs clarification.

AC: This section (Section 4, P30) has been substantially revised.

RC: Page 25: Note that table 4 is in the appendix

AC: amended to “as per Table 6 in the Appendix” (P33, L18).

RC: Page 29 Line 8: net respiration versus net uptake should be made clear

AC: Page 37, line 5: amended as “.....effect of the terrestrial biosphere, which causes net uptake CO₂ in the Holocene period (increases atmospheric $\delta^{13}\text{C}$), and net respiration of CO₂ in the LGM period (decreases atmospheric $\delta^{13}\text{C}$)”.

RC: Page 30 Line 10: don't need ‘however’ here

AC: Amended (P37, L18): “SCP-M results shown for comparison with sparse carbonate proxy data”

RC: Line 30: “showed deltas in the range of” – be more precise here

AC: We have expanded to (P40, L10): “According to Francois et al. (1999), palynological and sedimentological data infer that the terrestrial biosphere carbon stock was 700-1350 PgC smaller in the LGM, than the present.”

RC: Page 31 Line 6: “but critically *are* accompanied”

AC: amended accordingly (P42, L9)

RC: Line 7: *the* carbonate ion proxy?

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AC: amended accordingly (P42, L11)

RC: Line 23: superscript 14C

AC: amended (P42, L12; P43, L17)

RC: Line 25: it cannot be wholly explained by overturning changes, as these must be combined with temp/salinity etc. changes listed afterwards. Also this work confirms that reduced overturning can drive the changes, rather than proposing this.

AC: “wholly” has been removed (P43, L20). Page 42, line 11 specifically addresses the second part of this comment. “This is not a new finding.” etc.

RC: Figure 12: as with Figure 9, it’s very difficult to get any meaning from this figure. Perhaps colours could be used to denote increases/decreases at the very least?

AC: We have modified the figure to show the positive or negative variation for the LGM, in PgC, to highlight what is changing, as with Figure 9.

Please also note the supplement to this comment:

<https://www.geosci-model-dev-discuss.net/gmd-2018-176/gmd-2018-176-AC2-supplement.pdf>

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2018-176>, 2018.

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