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Geoscientific Model Development Discussions

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 General Comment: There is a lot of different topics/issues presented in this paper (e.g. model description and concept, LGM pCO2 change, partition of carbon under anthropogenic forcing), however I would have liked to see additional information on the

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model experiments as well as more background information. The model description is incomplete without information on temperature, salinity and the carbon isotopes section should be moved to the main text. Sensitivity studies are performed but the initial set of parameters are unclear and the reasoning behind the changes to these parameters is not substantiated, leaving the reader guessing as to why such experiment was performed and figuring out whether the range of parameters studied made physical sense or not.

AC: We have addressed these comments in more detail in response to the specific comments below. As a general comment, we have not tried to exhaustively review or document the starting values for all parameters. However, in response to the comment we have added additional text in Section 2.2.2 (Ocean and circulation and mixing) to explain our choice of parameters for the modern/late Holocene model spin-up. In response to the comments, we have also added more detail to Section 2.2.3 (Biological flux parameterisation) to explain our input values for marine biological production/export parameters. Throughout the document we have added more references to Table 6 in the Appendix that shows the model's parameters and dimensions, and their sources. At the start of Section 3.2 (Sensitivity tests), we have added a paragraph to explain the rationale for undertaking the sensitivity tests, and what range of values we have chosen. In addition, as suggested in the comments below, we have added to the Figure 4 subplots the modern parameter values/assumptions for visual reference with the sensitivity tests.

1) Introduction

RC: The introduction focuses on glacial/interglacial variations in atmospheric CO2. This is indeed one part of the study, but not only. I would have thought that (at least) the first part of the introduction should be devoted to the reasoning behind setting up such a box model.

AC: We have re-arranged the introduction by moving the discussion of box models and

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rationale for SCP-M, to the front (Page 2, line 16). We have moved the discussion of the LGM-Holocene modelling to a later section in the paper. For this reason, many of the following items can now be found in section 4 (Page 30, line 3).

RC: P1, L.18: Despite years of research, and significant progress, the sequence of events leading to glacial/interglacial changes in atmospheric CO2 is still poorly constrained. However, I don't think this can be called the "LGM Holocene dilemma". And I think the authors mean "glacial/interglacial" variations and not "interglacial" (here and throughout the text, e.g. p2, L.4).

AC: We have replaced the phrase "LGM Holocene dilemma" with "LGM-Holocene transition" and changed "interglacial" to "glacial/interglacial" throughout the manuscript (e.g. Page 30, line 3).

RC: P1, L.22: I am not sure these two references are the best to define the "LGM".

AC: Included (Yokoyama, 2000), ice sheet and glacier proxies (Clark, 2009) and stratigraphic records (Hughes et al, 2013; Hughes and Gibbard 2015) (see P32 L3 of the amended manuscript).

RC: P1, L. 26: and to the fact that the terrestrial carbon content was most likely reduced (e.g. Ciais et al., 2012, Peterson et al., 2014).

AC: we have added the following (P32, L7): "... alongside changes in the terrestrial biosphere stock of carbon (e.g. Francois et al, 1999; Ciais et al, 2012; Peterson et al, 2014; Hoogakker et al, 2016)"

RC: P2, L.2: only the reference to one review (Sigman et al. 2010) is given, while additional references could be given for all the hypotheses cited (at least one per mechanism). Another review could be mentioned: Kohfeld and Ridgwell, 2009.

AC: We have added Kohfeld and Ridgwell (2009), Broecker (1982), Sarmiento and Toggweiler (1984) for the ocean carbon reservoir reference (now on P32 L7).

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For the hypotheses cited, we have added (on P32 and P33):

Ocean biology: Martin (1990), Watson et al (2000), Martinez-Garcia (2014) Ocean circulation and mixing/stratification: Toggweiler (1985, 1999), Curry and Oppo (2005, Kohfeld and Ridgewell (2009), Anderson et al (2009), de Boer and Hogg (2014)), Menviel et al (2016), Muglia et al (2018). Sea ice cover: Stephens and Keeling (2000) Synthesis of mechanisms: Kohfeld and Chase (2017), Ferrari et al (2014). Other features are implicated including temperature, terrestrial biosphere, ocean volume, shelf carbonates. (Trent-Staid and Prell (2002), Annan and Hargreaves (2013), Ciais et al (2012), Opdyke and Walker (1992), Ridgewell et al (2003)).

RC: P2, L.4-11: I would strongly suggest to significantly revise this paragraph, which really does not do justice to the last 15 years of work on the topic of glacial/interglacial changes in atmospheric CO2. Many sensitivity experiments and transient simulations have been performed with box models, models of intermediate complexity and OGCMs to understand glacial/interglacial changes in pCO2. A few references (non-exhaustive list) include Stephens & Keeling (2000), Toggweiler et al., (2006), references within Kohfeld and Ridgwell (2009), Hain et al., (2010), Tagliabue et al., (2010), Hesse et al., (2011), Bouttes et al., (2012), Tschumi et al., (2011), Chikamoto et al., (2012), Menviel et al., (2012), Ganopolski & Brovkin (2017), Menviel et al., (2017). . .. Many of which (if not all of them) also included a thorough model-data comparison.

AC: Paragraph revised, and moved to the modelling section (P32, L18)

RC: On the contrary, I would have liked to see in the introduction more details with respect to the rationale of constructing a new carbon cycle box model.

AC: We have expanded this discussion and added it to the front of the introduction (as per response above; see P2 L16 of the revised manuscript), as well as the discussion section (Section 5).

RC: P2, L. 25: Please reformulate “extra-ocean”

AC: replaced with “carbon cycle” (P2, L34).

RC: (Please also reformulate header of section 2.4)

AC: Replaced with “Atmosphere and terrestrial carbon cycle” (now Section 2.5, P16).

2) Model description

RC: The model description is incomplete. In section 3, it is stated that the model is forced by SST and SSS, however there is no mention of the treatment of temperature and salinity in the model.

AC: We have added a description of the model’s treatment of temperature and salinity in Section 2.4 (P15). The temperature and salinity in each of the model’s surface ocean boxes is prescribed. The model does not solve for these values, rather takes them as inputs for the calculation of pCO2 in the ocean. We argue that this is a plausible approach for paleo-reconstructions given the emergence of paleo- estimates for SST across glacial-interglacial cycles (e.g. Kohfeld and Chase, 2017), as a useful forcing for model-data exercises.

The starting data are sourced from modern (GLODAPv2) ocean data, mapped into box model space, with adjustments made to the values for the model experiments, e.g. glacial period temperature (decrease) and salinity (increase) are forced. Temperature feeds into the pCO2 / CO2-3 calculation and air-sea fractionation factors for d13C. Salinity feeds into the pCO2 / CO2-3 calculation.

RC: There is no description of the parametrization of the carbon isotopes in the main part of the manuscript. Since the manuscript focuses on carbon isotopes, the main formulations have to be clearly laid out.

AC: We have moved the description of carbon isotopes to the main body of the document (Section 2.7, P17).

RC: In addition, marine export production is prescribed (p9), but there is little informa-

tion on the values used, how they were chosen and how they vary in the experiment.

AC: We have added more information on the marine export production, as follows (Section 2.2.3 P12, L8):

“We started with a global base productivity/export value at 100m of 5 mol C m-2 yr-1, which falls in the range of Martin et al (1987), of 1.2-7.1 mol C m-2 yr-1 , and Sarmiento and Gruber (2006), 0-5 mol C m-2 yr-1. Additionally, then we have manually tuned the individual surface box values, via a scalar for each box, to match the GLODAPv2 data for DIC, phosphorous, alkalinity, CO2-3 and the carbon isotopes. We chose a value for the “b-scalar” for the Martin et al (1987) export curve, of 0.75, which falls in the range of Berelson et al (2001) of 0.82 +/- 0.16, and also within that of Gloege et al (2017) of 0.68-1.13, and close to the original Martin estimate of 0.858. We found a global value of 0.75 to produce a better fit to the GLODAPv2 data when calibrating the model. The b parameter controls the depth decay of the biological export flux. “

We have added a table (P13, top of page) which shows the initial values for marine export production, and the part of the manuscript dealing with the LGM-Holocene experiments now has a table setting out how the parameters vary in the experiments.

RC: Figure 4 could be helpful in that sense: the late Holocene and/or modern day values of all parameters should be clearly indicated in that figure.

AC: Figure 4 (P 24) amended to include modern day values/assumptions for the parameters shown.

RC: P 3, L. 5-6: “simulates sources and sinks”. Some of these sources and sinks are really simplified, for example anthropogenic and volcanic emissions are a simple prescribed flux into the atmosphere. Weathering and river fluxes are also close to a simple pre- scribed flux. So, for some it might be more precise to state “includes forcing” than “simulate sources”.

AC: OK, done (Section 2; P2, L30)

RC: P3, L. 13-15; I am confused by this sentence.

AC: Removed offending sentence

RC: Ocean circulation and mixing: Box 4: why is there no exchange with boxes 3, 5 and 7 in equation 1? From the matrix, it looks like there are exchanges with boxes 5 and 7 but not 3, why?

AC: There are a few aspects to this comment. With regard to exchange between Box 4 and Box 3, we have assumed that this flux is small compared with the lateral transport and mixing fluxes between Boxes 4/6 and boxes 1/3. We assume this is the divide between northward flowing water sourced from Antarctic Intermediate Water (AAIW) and Subantarctic Mode Water (SAMW), overlying southward return flow from Atlantic Meridional Overturning Circulation (AMOC) and Pacific/Indian Deep Water (PDW/IDW).

With regard to exchange between Box 4 and boxes 5 and 7, this flux is shown in Equation 4 by the flux (–C4): it is simply a flux out of Box 4. The matrix (Equation 6) shows that this flux is split into Box 7 and Box 5 via the alpha parameter, described in the text.

As general comments on the matrices and the logic of the fluxes. The concentration of an element in each box is a function of a) the magnitude of the physical flux (in Sv) into a box and the element concentration of the originating box and b) the magnitude of the flux (in Sv) out and element concentration of the box itself. The concentration of the 'downstream' box does not enter the equation.

As shown in Figure 1, box 4 receives flux of DIC (C) from box 2 via Psi2. Psi2 also directly transmits to box 7 from box 4, but this is a flux out of box 4 and box 7 does not enter Equation 1. Likewise, Psi1 (red arrow in Figure 1) transmits C from box 6 into box 4 (as per equation 1), but the outward flux of carbon from box 4 into boxes 5 and 7 is function of box 4 element concentration, and boxes 5 and 7 do not need to enter this

equation.

We have added text in the manuscript to specifically address this (P9).

RC: Box 1: why no exchange with boxes 2 & 7 in equation 2?.

AC: Equation 2 refers to the parameter gamma2, which governs mixing between the low latitude surface box (1) and intermediate box (3). We assume that northward lateral transport takes place between the sub polar, intermediate and northern boxes. This water is colder and denser than the overlying mixed layer, given its deep-upwelled sources from AAIW and SAMW from upwelled NADW/PDW/IDW (e.g. Talley, 2013). We assume that Box 1, the low latitude surface box, represents the mixed layer (e.g. Kara et al, 2013), which is mainly under the influence of ocean surface processes. We prescribe vertical mixing between this box and the underlying intermediate box via the gamma2 parameter, conceptually the thermocline mixing described by Liu et al (2016).

As such, the parameter only operates on boxes 1 and 3 as per equation 2 (and as shown in Figure 1).

We have added text in the manuscript to specifically address this (P10).

RC: P11, L. 15-17: “around glacial cycles” is not precise enough. In addition, I don’t think this sentence is correct, as changes were opposite in the Atlantic and Pacific Oceans.

AC: (P13, L20) reworded as: “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”.

The aim of this sentence is to briefly introduce carbonate sediment burial and dissolution as an influence on the carbon cycle.

3) Modelling results

RC: P16, L. 17: Is [CO3] approximated by ALK-DIC or fully calculated using ALk, DIC,

T, S, P?

AC: The latter. We use the method of Follows et al (2006) which calculates pCO2 and CO2-3 as a function of Alk, DIC, T, S and P. The purpose of this sentence was to highlight that the approximation for CO2-3 of Alk-DIC is useful for interpreting model results charts. We have amended this sentence accordingly (P22 L23), and expanded the description of the pCO2 and carbonate ion calculations to identify DIC, Alk, T, S and P as inputs (P13 L10).

RC: P16, L.20: please reformulate as “remineralization of organic matter”

AC: Amended (P22, L25).

RC: P17, L. 2-7: Please explain your reasoning behind varying the rain ratio.

AC: This paragraph has been re-worded, with the first reference to the rain ratio removed – as it is confusing (P23, L12).

RC: I don’t understand why changing the rain ratio impacts atmospheric D14C and I suppose that the surface ocean pCO2 change could eventually impact atm d13C, but not “heavily” (L. 6-7).

AC: Re atmospheric D14C. Increasing the rain ratio leads to higher pCO2 in the ocean surface boxes (removes alkalinity in ratio 2:1 to DIC), and subsequent de-gassing of CO2 to the atmosphere, which increases atmospheric CO2. The air-sea fractionation factors for D14C, that we have used, exhibit greater fractionation of the isotopic ratio in out-gassing to the atmosphere versus in-gassing to the ocean, so there is a modest decrease in atmospheric D14C (the atmosphere is preferentially receiving 12C). We have removed the word “heavily” as that wording indeed exaggerates the effects (P23, L26).

RC: P19 L6. Please add “and there is a reduced outgassing of old low D14C waters”

AC: Amended accordingly (P25, L3).

RC: P19 L8. Please remove “around the interglacial cycles” and please note that the year of the reference is actually 2008.

AC: Done, reference updated throughout document (e.g. P30, L12).

P19, L. 9-14: I suppose the authors expect a change in pCO2 due to the change in ocean area resulting from varying sea-level (and thus ocean volume) on G/IG timescales. Please spell it out. Please take out “volume” on L.9. The impact on D14C is surprising though.

AC: Amended accordingly (P25, L10).

RC: P22 L7. This sentence is not correct, re-formulate.

AC: We recompiled this section as part of the discussion of LGM-Holocene work (Section 4, P30)

RC: P22, L. 10-14: I don’t really agree with this paragraph. It is probably true for simple carbon cycle box model for which all parameters have to be tested and therefore the G/IG CO2 problem is explored by assessing the impact of each parameters. But, over the last years the G/IG CO2 problem has also been studied with coupled models providing a representation (granted this representation is associated with large uncertainties) of physical and bio- logical changes occurring during glacial times.

AC: Easier to remove this paragraph.

RC: P23 L4-6 “I am not sure what is meant here or what has been done”

AC: Sentenced removed as is extraneous.

4) Discussion

RC: A discussion of the capacity of the model and the results is missing. I would have liked to see a paragraph on why this model should be used. What are its benefits and limitations? Its fast processing time should be discussed here, instead of the intro-

duction. I would have liked to see the results of the future experiments discussed in the context of the CMIP5 results. Only Jones et al and Wang are referenced in this part. I would have liked to see a discussion of the results of the LGM experiments in comparison with other studies. Recently Muglia et al (2018) and AMOC, iron fertilisation. Menviel was consistent. These two studies among others could help discuss the effects of Z, psi1, psi2 as shown in Figure 10.

AC: We have added new discussion Sections 5.1 (Model advantages and limitations), 5.2 (Modern carbon cycle simulations) and 5.3 (LGM-late Holocene modelling) to address this comment.

RC: Abstract The second part of the abstract focusses on the LGM simulations. I would suggest to tone down that part and instead add some information about the use and limitations of the model.

AC: Noted and amended accordingly, incorporating summary of limitations described above.

RC: Minor and typos P3 L10-12 please reformulate AC: Easier to remove, as the point is made in the preceding sentences P16 L29-35. Please reformulate this paragraph. AC: Amended (P23, L11-24) P17 L10 “decreases” AC: Fixed (P23, L30) P19 L19 please reformulate sentence. AC: Amended (P25, L24) P19 L26. Maybe “appropriate” instead of “accurate”. AC: Fixed (P25, L32)

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2018-176/gmd-2018-176-AC1-supplement.pdf

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