MS-No.: ESD-2021-42

Title: Effect of the Atlantic Meridional Overturning Circulation on Atmospheric pCO_2 Variations

Author(s): Daan Boot, Anna S von der Heydt and Henk A. Dijkstra

Point-by-point reply to reviewer #1

October 27, 2021

We thank the reviewer for his/her careful reading and for the useful comments on the manuscript.

1. *The major issue relates to the parametrization added that gives rise to the internal oscillation. A part of this oscillation involves changes in the riverine flux of alkalinity as a function of pCO2 and the other is linked to an increase in temperature due to an increase in ocean alkalinity within 1000 years. What are the reasons behind these parametrizations? I understand that weathering is modulated by pCO2. However, I thought that this was a slow process, and I don't think that a change in atm. CO2 should directly lead to a proportional change in alkalinity river influx (within 1000 years). Maybe the oscillations you highight are relevant for longer timescales, i.e. glacial/interglacial changes in pCO2. I suggest to carefully read the litterature on changes in weathering during G-IG cycles. I can't find a reason for an increase in ocean alkalinity leading to an increase in temperature though (green box at t=0 to blue box at t=T/4 in fig. 6).*

   Author's reply:
   There are two different processes here: (1) the coupling between alkalinity and temperature, and (2) the riverine influx of alkalinity.

   (1) There is no direct coupling between alkalinity and atmospheric temperature. However, alkalinity indirectly influences temperature. It does this via its influence on the pH. The pH of the surface ocean determines the oceanic pCO_2. The gas exchange is proportional to the pCO_2 difference between the ocean and atmosphere. From this we see that the gas

exchange is influenced by the pH, and thus alkalinity. Via the gas exchange, atmospheric pCO_2 changes, and therefore also the atmospheric temperature.

Concerning figure 6: we understand the confusion here. In this figure, blocks of the same color have the same 'first event'. The first event is recognizable by the thick outlining. For the blue blocks this is 'Atmospheric pCO2 starts to increase'. This process continues for half a period, and is thus still present as alkalinity starts to increase.

(2) The parameterization used in this study is the same as used in the original SCP-M and is based on work by Toggweiler (2008).

It is true that riverine fluxes generally work on longer time scales (order 10 kyr). However, in the oscillation, our system does not reach equilibrium. The riverine influx is determined by atmospheric pCO_2 which again is influenced by processes on shorter timescales than the river fluxes. We would also like to point out that the amplitude of the river flux is small compared to that of the sinks of alkalinity in the oscillation (fig. 7b).

Changes in manuscript:
We will make Figure 6 clearer. Furthermore, we will clarify the role of the river flux in the oscillation mechanism.

2. *The paper is hard to follow. A combination of 13*7 experiments are performed. They are labelled with 1 or 2 letters per feedback and numbers for experiments, making it difficult to recall what we are looking at. If more explicit labels were used in Figures 3 and 4, it would help. In addition, there is very little justification/discussion of the different experiments, leading to confusion. The parametrization of the rain ratio feedback is not common. I thought that the largest impact on rain ratio would come from changes in silicifiers, and thus silicate and/or iron concentration in the ocean. L. 278, the authors state that "for low rain ratios, we only have a constant dissolution", which confuses me, as I don't see a link between dissolution and rain ratio in the methods.*

   Author's reply:
   We understand that the labelling of the experiments may be confusing. We will choose clearer, more explicit labels in the revision.

   About the justification of the experiments: we will make it clearer in the text. We generally choose experiments to test the effect of a feedback that is used in more complicated models. Feedbacks that were more certain (such as the temperature feedback ($\lambda_T > 0$)) or had a large effect on the solution (such as the efficiency feedback ($\lambda_\epsilon > 0$)) were used in experiments with more than one feedback.

The parameterization of the rain ratio feedback used in this study is taken from Ridgwell et al. (2007). This parameterization is also optional in the EMIC CSIRO Mk3L-COAL model (Buchanan et al., 2019). Our model does not include silicifiers and/or iron. Therefore, we do take these effects into account.

In our model, dissolution of $CaCO_3$ is dependent on two components: (1) a component proportional to the rain ratio and related to the saturation state; and (2) a constant component. When the saturation state is larger than 1, the first component is equal to 0. For this specific experiment the saturation state is always above 1 when the rain ratio is low. So what is meant with L. 278 is that when the rain ratio is low, the saturation state is always larger than 1, thus we have no saturation driven dissolution, but only a constant dissolution (the second component).

Changes in manuscript:
We will use more explicit labels for the experiments. Furthermore, we will include a better justification for the performed experiments and we will make the text around L.278 clearer.

3. *Discussion and implication of the results:*
The study scans a large range of parameters yielding pCO2 values of 70-300 ppm, but without really trying to assess physical plausability. For example, in Figure 4, multipliers 0.1-10 are included in the parametrizations, but without much justification. What can the authors deduce from their results? What are the probable ranges?

The discussion needs to put the results back in context and discuss them in light of previous experiments. In the Introduction, the authors cite previous studies that simulated the impact of AMOC changes on the carbon cycle with Earth system models (in which most of the feedbacks explored were included). Can your results help understand better these previous simulations?

Author's reply:
We agree. Reviewer 2 also commented on the justification of these experiments.

Changes in manuscript:
We will include more justification of these experiments and discussion of the results.

Minor points:

1. *L. 41: I am not sure that "not well understood" is appropriate, since a lot of studies have highlighted the impact of AMOC on pCO2 and the reverse as highlighted in the 2 following paragraphs. It is however a complex interaction.*

   Author's reply:
   We agree that it is not the most appropriate wording.

   Changes in manuscript:
   We will change the text to reflect the complex interactions.

2. *L 272: Please amend: "Fig. 4a, b is yellow.."*

   Author's reply:
   Suggestion followed

   Changes in manuscript:
   The text will be changed accordingly.

3. *L. 295: What is the meaning of "we continue in the piston velocity"?*

   Author's reply:
   This means that we use the piston velocity parameter as our continuation parameter. **Changes in manuscript:**
   We will clarify this in the revised text.

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Point-by-point reply to reviewer #2

October 27, 2021

We thank the reviewer for his/her careful reading and for the useful comments on the manuscript.

1. *In the Inroduction section, are there any papers using 3D OGCM to simulate the atmospheric pCO2-AMOC strength relationship under PI and LGM? If so, these papers need to be properly cited.*

   Author's reply:
   In the introduction we already cite multiple papers that simulate the atmospheric pCO_2-AMOC relationship using EMICs, ESMs and (A)OGCMs.

   Examples of the cited papers are:
   Menviel et al. (2014) (EMIC in the LGM); Menviel et al. (2008) (EMIC in the LGM and PI); Mariotti et al. (2012) (AOGCM in the LGM); Nielsen et al. (2019) (ESM with a PI control simulation); Huiskamp and Meissner (2012) (ESM in the LGM); Gregory et al. (2005) (AOGCMs and EMICs with a PI control simulation); and Gottschalk et al. (2019) (ESMs and EMICS in the LGM).

   However, we may have missed some interesting papers.

   Changes in manuscript:
   We will look into the literature again, and add new citations to the introduction.

2. *I didn't see any experiments to test the plausibility of the box model to address the AMOC-pCO2 relationship problem. I would suggest that you set up two more experiments fully including all the feedbacks you mentioned in Table 2 and check if the atmospheric pCO2 is reasonable under two scenarios.*

Author's reply:
That is a good suggestion.

Changes in manuscript:
Suggestion will be followed. We will include results of these two extra experiments.

3. *In general, I think all the experiments should be set up with other feedbacks properly included to make the case more realistic. For example, when studying the role of biological feedback (x-0 and x-1 in Table 2), the x-0 could be set up with all λ = 1, x-1 then should be only with λ_{BI}=0, etc.*

   Author's reply:
   We chose to set up the experiments as in the original paper, since we base our model on the SCP-M and this model contains no feedbacks. The SCP-M is tuned to accurately represent both the PI and LGM conditions. We therefore consider that we start with a "realistic model" if all feedbacks are switched off (i.e. experiment x-0). Switching on all the feedbacks would not necessarily lead to a more realistic case, since the SCP-M is not tuned to include these parameters.

   Changes in manuscript:
   We will better justify our approach.

4. *In lines 266-270, the three parameters are selected as control parameters: the rain ratio, the biological production and the piston velocity. Please explain the reasons for picking these parameters. Also, the multiplier changes from 0.1 to 10 without reasonable explanations. I would suggest using more realistic ranges.*

   Author's reply:
   We use these three parameters since they more or less represent the three carbon pumps often used in the traditional view of the oceanic carbon cycle. The rain ratio affects the strength of the carbonate pump, the biological production the soft tissue pump and the piston velocity the solubility pump. We chose these three parameters to see whether a (large) change in one of the traditional pumps can invoke large non-linear changes or bifurcations in this model.

We agree that the multiplier range does not necessarily reflect realistic values. One of the goals of this study was to get a better understanding of the sensitivity of the carbon cycle to these parameter changes and whether bifurcations can arise.

Changes in manuscript:
We will better motivate the reasons for varying these three parameters, and why we choose a large range in parameter values.

Comments/concerns about specific feedbacks/parameters are below.

1. *In equation (2), the authors chose 0.54 °C/(Wm-2) to compute the temperature change. As this parameter is important in equation (12) to control the AMOC strength, what is the sensitivity of this parameter to coupling AMOC-carbon cycle?*

   Author's reply:
   The precise value of this parameter (0.54) is not very important in this study as the sensitivity to this parameter is generally low. This can also be seen in section 3.2., where we check the sensitivity of atmospheric pCO_2 to the value of λ_A. This can also be interpreted as the sensitivity of the relationship to this 0.54 (since 0.54 is multiplied with λ_A), which is low.

   We do see that the system is prone to show Hopf bifurcations when λ_T is increased. However, this is when we increase λ_T to relatively large values (order 20).

   Changes in manuscript:
   No changes necessary.

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List of changes

December 7, 2021

Following is a list of notable changes made in the revision as a response to the comments of the reviewers. There have also been some small textual changes (spelling, rephrasing, etc.). These small changes are not included in this list.

1. We have included a study using a 3D OGCM in the introduction (l. 48).
2. We have included a justification of the chosen approach (around l. 230).
3. We have changed the labelling of the experiments (Table 2 and everywhere in the main text).
4. We have performed more experiments where we use all the feedbacks. These experiments are included in updated versions of Fig. 2, 3 and 4. These experiments are also discussed in the main text.
5. We have included a justification of the chosen approach in section 3.2 (l. 277-281).
6. We have updated Fig. 6 to make it clearer.
7. We have included a discussion on the role of the river flux in the oscillation (l. 356-360).
8. We have made the connection between our work and other studies in the discussion (l. 373).
9. We have included an extra paragraph in the discussion to comment on the realism of our experiments and the usefulness of our results (l. 406-412).