MS-No.: ESD-2021-42

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

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Point-by-point reply to reviewer #1

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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.