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Title: Potential effect of the marine carbon cycle on the multiple equilibria window of the Atlantic Meridional Overturning Circulation

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Point-by-point reply to reviewer #2

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We thank the reviewer for their careful reading and for the useful comments on the manuscript.

In this manuscript, Boot and co-authors couple a physical box model of the AMOC to a carbon cycle box model. This tackles an interesting and largely unanswered question of how the carbon cycle and the AMOC influence each other, with a particular focus on whether and how the carbon cycle may impact the stability of the AMOC. The paper is overall well written. It builds on previous work such that the two box models are well established in their own right. I have two (related) main concerns and in the current form of the manuscript I was not able to tell whether these concerns are indeed pointing to fundamental issues with the approach and results, or whether it is rather an issue of presentation.

1) Is the coupling of the 2 full models needed to answer what I interpret as the central question: how does the MEW depend on atmospheric CO2 concentrations? This is related to another fundamental aspect I am concerned with: The very purpose of idealized box models is to reduce the complexity of a system to a small number of leading-order processes which can then be probed in detail to gain intuitive understanding. The model developed here with 30 ODEs is so complex that I wonder whether much intuitive understanding can be gained? Furthermore, from the figures presented it appears that many of the processes included have no or barely any notable impact on the processes that are being studied (see the overlapping curves in Fig 3 and the many almost identical lines in Figure 5). From my reading of Figure 4 a key process driving changes in MEW is the increase of Es with increased CO2? In that case, why not, for example, take the physical AMOC model and force it with Es (as constrained by the CMIP6-derived CO2) and consider the resulting changes in the MEW? Although I wonder whether this would be *rather similar to the original work of Cimatoribus et al (2014)?*

Author's reply:
The main reason for coupling the two models is to study whether feedbacks in the carbon cycle have a major influence on AMOC dynamics in steady state. The main idea here is that the carbon cycle responds to changes in the AMOC, resulting in a response in atmospheric pCO_2. This influences the atmosphere and therefore can influence the AMOC. By just forcing the model with E_s we would not be able to capture the feedbacks in the carbon cycle, and the feedbacks between the AMOC and the carbon cycle and would indeed be similar to the sensitivity studies in Cimatoribus et al. (2014). We therefore believe that coupling the two models is essential for the overarching research questions of this work.

The total size of the system (i.e. 30 ODEs) is indeed large, but this is mainly because the carbon cycle is in itself very complex. To be able to capture carbon cycle dynamics we need 3 state variables per box (dissolved inorganic carbon, total alkalinity and a nutrient). This is the main reason for the relatively large problem size. Intuitively understanding the results is difficult, but this is inherent to studying carbon cycle dynamics, because it is such a complex system where biological, chemical and physical processes are intertwined. However, the most important carbon cycle variable for this study is atmospheric pCO_2. Changes in atmospheric pCO_2 can more easily be understood because it indirectly depends on the amount of carbon burial in the sediments. The burial rate is dependent on biological production and dissolution of calcium carbonate ($CaCO_3$) in the water column. To understand these processes, we do not need to have a full understanding of all the carbon cycle variables, which makes understanding the results already much simpler.

We have included the different feedbacks to see whether non-linear carbon cycle feedbacks can have a major influence of the multiple equilibria window (MEW) of the AMOC. What Fig. 3 shows is indeed that the effects of most feedbacks on AMOC dynamics are typically small when simulated under the same amount of carbon. We already made a selection to only highlight the feedbacks that are important for the conclusions in the main text. The BIO feedback is included because without it we cannot simulate an off branch. The E_s feedback is included to couple the physical climate to the carbon cycle. The FCA feedback is included because it changes the carbon cycle dynamics as seen in Fig. 3b, and especially when run under different carbon contents (Fig. 5). Since we are also using experiments with different amounts of carbon, and CO_2 concentrations, it was essential to also include the effects of temperature. We opted to use a low climate sensitivity (CS_{Lo}) and a high climate sensitivity (CS_{Hi}) to capture uncertainty in the climate sensitivity and to more clearly show the effect of the temperature feedback on the results.

Changes in manuscript:
No changes necessary.

2) Is the combined model suitable to probe the size of the MEW? In my reading of the results, the size of the MEW (the distance between the dot-dashed and dashed lines in Fig 3) is barely impacted at all by accounting for different processes - even when the CO2 concentrations (right column of Fig 3) change quite notably. Similarly, the MEW size in Fig 5 is either completely or mostly insensitive to changes in the processes that are accounted for and also to total carbon content. I find this quite remarkable, since this is a very complex non-linear model and the authors consider a wide range of feedbacks and forcings etc, yet the MEW is largely constant. Again, as far as I can tell the main sensitivity is to Es (or atmospheric CO2) in Fig 4. This makes me wonder whether the title of the study should rather be something along the lines of "Robustness of AMOC MEW to changes in marine carbon cycle"?

Author's reply:
The main response is indeed due to the sensitivity of the model to E_s as presented in Fig. 4 and the sensitivity of E_s to atmospheric pCO_2. We disagree with the reviewer that the MEW is mostly insensitive to total carbon content. In Fig. 5a for the bottom three cases (i.e. FCA, CS_{LO}, CS_{HI}) the MEW increases by approximately 20% as total carbon content increases by about 50%. We do show that the inclusion of certain non-linear carbon cycle feedbacks does not alter the response of the MEW. In that sense the suggested title would fit maybe better to the manuscript. However, since the MEW is, in our opinion, quite sensitive to the amount of carbon in the system, we do not think the suggested title adequately captures the conclusions of this paper.

Changes in manuscript:
No changes necessary.

These comments are intended to highlight the questions that arose for me when I read the manuscript, and as I said above much of my skepticism may be the result of a lack of clarity of presentation. The other reviewer had some constructive ideas of how the presentation could be improved and that may alleviate some of my concerns above as well. I will further add that I have little expertise in the carbon cycle aspect of this work, which certainly hindered my interpretation. Nevertheless, I believe that a substantial reduction in the complexity of the model and the range of feedbacks and other processes may be required to be able to meaningfully shed light on the governing processes. As it stands, I found it difficult to assess the value of both the approach and the results.

Author's reply:
Reviewer 1 indeed had some helpful suggestions on the presentation that we will follow. This will help to make the paper much clearer. As explained under comment 1, the carbon cycle is inherently complex, so we cannot reduce the complexity of the model much further. However, we will clarify the role of the additional feedbacks following suggestions of reviewer 1.

Changes in manuscript:
No additional changes necessary.

As a final comment, I was noting the absence of any model validation or comparison to previous formulations. At one point the authors state that they had to add two boxes to ensure realistic CO2 values. The original version apparently had very low CO2 under AMOC collapse, and the authors state that most previous modeling studies found increases in CO2 under AMOC collapse. However, the results in Fig. 3 still show substantial reductions in CO2 when going from the AMOC "on" to the "off" state. In my reading this prompts open questions as to how this work compares to previous studies. To instill confidence in this novel coupled model, I would argue that some form of validation is needed.

Author's reply:
With the used solution method, we can only solve for steady state solutions.

This makes it difficult to compare our results to other studies since these generally use time dependent simulations (see e.g. Gottschalk et al., 2019). Since we study the steady state response we do not expect that our model shows the same response as in these studies. However, our results have a similar order of magnitude as the studies which gives us confidence that the model is valid for our application.

Changes in manuscript:
We will mention how our results compare to the transient studies in Gottschalk et al. (2019).

References

- Gottschalk, J., Battaglia, G., Fischer, H., Frölicher, T. L., Jaccard, S. L., Jeltsch-Thömmes, A., Joos, F., Köhler, P., Meissner, K. J., Menviel, L., Nehrbass-Ahles, C., Schmitt, J., Schmittner, A., Skinner, L. C., and Stocker, T. F.: Mechanisms of millennial-scale atmospheric CO2 change in numerical model simulations, Quaternary Science Reviews, 220, 30–74, https://doi.org/https://doi.org/10.1016/j.quascirev.2019.05.013, 2019.