MS-No.: ESD-2023-30

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

Authors: Amber A. Boot, Anna S. von der Heydt and Henk A. Dijkstra

Point-by-point reply to reviewer #1

July 15, 2024

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

Summary:

Boot et al. study the "multiple equilibria window" (MEW) of the Atlantic Meridional Overturning Circulation (AMOC) in a coupled ocean-carbon cycle box model. Specifically, they study the interactions between the AMOC and the carbon cycle, and show, among other things, that:

1. *AMOC off states have lower atmospheric* pCO_2.
2. *the MEW widens when more carbon is added to the system.*

I think that this is an exciting paper. The model seems reasonable (and indeed is based on multiple previous studies) and the methodological approach is sound. The AMOC and its nonlinear behaviour are subjects of great relevance, both with respect to present or future tipping risk as well as for understanding paleoclimate dynamics, and the paper derives some interesting new insights. I always appreciate seeing dynamical systems approaches (and indeed, AUTO) used for these purposes. However, I do think that the paper's key insights are still a little obscured behind the modelling details, and I recommend a few revisions to bring them out more clearly.

Specific comments:

1. *First, it's challenging on an initial read to keep track of all of the cases and what they mean. This is challenging to get around without major rewrites (which I do not think are needed), but I think at minimum*

Table 1 could be made more helpful by describing clearly what all the lambda values represent. This could be done either within the table itself, or perhaps more productively in the caption.

Author's reply:
We agree that it can be difficult to keep track of all the different cases.

Changes in manuscript:
We will increase the clarity of the caption for Table 1 where we will more elaborately explain what the different lambda values represent.

2. *Next, Figure 1: I think it's worth mentioning explicitly in the caption that the strength of the AMOC downwelling is set by the meridional density gradient between ts and n. Understanding exactly how AMOC strength is set in the model will help readers later on when mechanisms are explained.*

   Author's reply:
   We agree.

   Changes in manuscript:
   Suggestion followed.

3. *Figure 3: I found this quite confusing at first read, not least because of the overlap between many of the curves. If the key point of this figure is to show the general shape of the AMOC bifurcation diagram as well as to illustrate that off states have lower pCO2, perhaps it might be worth showing only this: i.e. AMOC vs Ea and pCO2 vs Ea for one single case (and moving the other cases to the Appendix). This is not essential, but I offer it as a suggestion.*

   Author's reply:
   We agree that the figure can be confusing.

Changes in manuscript:
Suggestion followed.

4. *Figure 4: my first comment is that this is really big compared to other figures that strike me as equally important, e.g. 5a. Second, it seems like what really matters are not the blue and orange lines themselves but the spaces they demarcate – why not label them accordingly? e.g. the region between the lines is precisely the MEW, the region above the blue line is one where only the off state is stable, and the region below the orange line is that where only the on-state is stable. Third, why not include CO2 levels as a second x-axis at the top of the graph which maps nonlinearly onto Es? I think these changes would make the figure vastly easier to understand at first glance.*

   Author's reply:
   Thank you for the useful suggestions.

   Changes in manuscript:
   Suggestions followed: we will label the monostable regimes and multiple equilibria window explicitly in the figure and use a second x-axis for the CO_2 concentrations instead of the green line and decrease the size of the figure.

5. *Figure 5: My main comment here is that this could be much larger. For example, it seems like 5a shows a major result of the paper, but it's small and hard to read. Maybe a and b could be on the top row and c in the middle on the bottom row? Also, it's worth mentioning in the caption the result from Caves et al. (2016) that total carbon content has varied between 24,000 and 96,000 Pg C, to make the reader understand immediately that the changes explored in the figure are reasonable.*

   Author's reply:
   Thank you for the suggestions, and indeed Fig. 5a shows the main result of this paper.

Changes in manuscript:
All suggestions followed.

6. *Figure 6. I guess this is probably a Latex quirk, but it's strange to me that it's placed after the Appendix and all of the references – this makes it easy to miss at first glance. It would be good to place it much more prominently near the end of the text. Finally, I suggest replacing dTC/dt with d[DIC]/dt (if indeed that's what's meant).*

   Author's reply:
   It is indeed a Latex quirk. We have chosen not to use DIC since atmospheric pCO_2 is also part of the total carbon (TC) content of the system.

   Changes in manuscript:
   We will make sure that Fig. 6 will placed correctly in the main text. We will clarify in the caption of the figure that TC represents DIC and atmospheric pCO_2.

More minor comments:

1. *Line 20: It may be worth mentioning studies reporting a present-day AMOC weakening, e.g. Caesar et al. (2018), Boers (2021), Ditlevsen and Ditlevsen (2023).*

   Author's reply:
   We agree.

   Changes in manuscript:
   We will mention papers studying AMOC collapses in present-day climate as suggested by the reviewer.

2. *Lines 37-38: I'm not directly familiar with the studies by Barker et al., but at a glance it seems like these are primarily observational (i.e.*

not model-based). It may be worth mentioning this, as it highlights the novelty of the authors' work.

Author's reply:
The work of Barker et al. is indeed observation based.

Changes in manuscript:
We will mention this in the revision.

3. *Line 38: "of how"?*

   Author's reply:
   -

   Changes in manuscript:
   Suggestion followed.

4. *Line 54: "eddy-induced" (consistent with wind-induced)*

   Author's reply:
   -

   Changes in manuscript:
   Suggestion followed.

5. *Line 87: "to form the model used..."*

   Author's reply:
   -

   Changes in manuscript:
   Suggestion followed.

6. *Line 97: I suggest always using "riverine flux" instead of "river flux" for clarity; "river flux" is repeated a number of times throughout the paper.*

   Author's reply:
   We agree.

   Changes in manuscript:
   Suggestion followed.

7. *Line 106/Eq. (1): It seems like there is a sum over all j missing here?*

   Author's reply:
   You are right. Formally there should indeed be a sum over j there.

   Changes in manuscript:
   Suggestion followed.

8. *Line 128/Eq. (4): do you have some more justification for this? e.g. the 0.81 power law?*

   Author's reply:
   This value is taken from Ridgwell et al. (2007) which represents thermodynamic calcification rates. They use this 0.81 value as a calibration parameter in the GENIE-1 Earth System Model.

   Changes in manuscript:
   We will cite the source of the function (i.e. Ridgwell et al. (2007)), and provide more background on the power law. We will also fix a typo in this equation, i.e. the expression between the brackets should be $(\frac{[Ca^{2+}][CO_3^{2-}]}{K_{sp,i}} - 1)$

9. *Line 151: Eq. (6): The linear dependence on atmospheric CO2 here (e.g. as opposed to other powers) is a fairly strong assumption that*

should probably be discussed.

Author's reply:
The linear dependence used here comes from the original SCP-M (O'Neill et al., 2019) which is based on the works of Toggweiler (2008). In models, such as LOSCAR (Zeebe, 2012), a model of similar complexity designed to simulate the long term carbon cycle, where a power law is used. Specifically in LOSCAR the power law causes atmospheric pCO_2 to converge in time to a predefined pCO_2 value. Since we apply a steady state approach this method can not be used. There are also models such as COPSE (Bergman et al., 2004) and GEOCARB-SULF (Royer, 2014) that use a much more complex weathering term including effects of temperature (which is linked to atmospheric pCO_2) and vegetation. This type of parameterization is too complex for our model.

Other powers could obviously be used in the model. Powers larger than one will decrease the sensitivity of the model to changes in the burial of $CaCO_3$ in the ocean, and powers smaller than one will increase the sensitivity of the model. Given that the model does not seem to be very sensitive to non-linear feedbacks in the carbon cycle, we would not expect additional non-linear behavior.

Changes in manuscript:
We will add a few lines in the discussion where we highlight that the parameterization we use is linear and based on previous work. We will also what it would mean for the results, as described above, when a nonlinear dependence is used.

10. *Line 189: (Andersson et al. 2017)*

Author's reply:
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Changes in manuscript:
Suggestion followed.

11. *Line 233: I think the usage of "saddle nodes" is confusing, and recommend that every instance of this be replaced with "saddle-node bifurcations".*

    Author's reply:
    We agree.

    Changes in manuscript:
    Suggestion followed.

12. *Figure 4: which case are these results from?*

    Author's reply:
    They are from the uncoupled case, i.e. without active carbon cycle model in there.

    Changes in manuscript:
    We will clarify this in the caption.

13. *Line 325: and rate-induced tipping, see e.g. Alkhayuon et al. (2019), Lohmann and Ditlevsen (2021)*

    Author's reply:
    -

    Changes in manuscript:
    Suggestion followed.

14. *Line 345: space after (Eq. A2)*

    Author's reply:
    -

Changes in manuscript:
Suggestion followed.

15. *Table B1 caption: "based on Cimatoribus et al. (2014)". similar in B2-B4.*

    Author's reply:
    -

    Changes in manuscript:
    Suggestion followed.

References

- Ridgwell, A., Zondervan, I., Hargreaves, J. C., Bijma, J., and Lenton, T. M.: Assessing the potential long-term increase of oceanic fossil fuel CO2 uptake due to CO2-calcification feedback, Biogeosciences, 4, 481–492, https://doi.org/10.5194/bg-4-481-2007, 2007.
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- Toggweiler, J. R.: Origin of the 100,000-yr time scale in Antarctic temperatures and atmospheric CO2, Paleoceanography, 23, PA2211, https://doi.org/10.1029/2006PA001405, 2008
- Bergman, N.M., Lenton, T.M., Watson, A.J., 2004. COPSE: A new model of biogeochemical cycling over Phanerozoic time. American Journal of Science 304, 397–437
- Royer, D.L., 2014. Atmospheric CO2 and O2 during the Phanerozoic: tools. Patterns, and Impacts 251–267
- Zeebe, R. E.: LOSCAR: Long-term Ocean-atmosphere-Sediment CArbon cycle Reservoir Model v2.0.4, Geosci. Model Dev., 5, 149–166, https://doi.org/10.5194/gmd-5-149-2012, 2012.

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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

July 15, 2024

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.