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 of the revision

October 3, 2024

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

This paper studies the width of the range of existence of multiple stable equilibria of the AMOC, as a function of different coupling mechanisms between a box-model for ocean circulation and carbon-cycle model studying the relevant ODEs with the continuation software AUTO. Both models have been previously studied and validated in a range of publications. The methods and the results are innovative and relevant for understanding AMOC stability and tipping in a range of climate states, they allow to identify and discuss relevant mechanisms and they certainly demonstrate that, in principle, a feedback between AMOC and carbon cycle is possible. Still, additional work is required, in my opinion, to organize in a clearer and more logical way the presentation of both the methods and the results.

In particular:

1. *The AMOC box model is presented in section 2.1 with 5 boxes but these are then extended to 7 (with addition of two boxes for the Indo- Pacific) in section 2.3.*

   The coupled model is further modified when discussing the solution method in section 2.4, dropping the deep Atlantic box and substituting it with a global conservation constraint. This further change to the model structure occurs after having already discussed various additional coupling mechanisms in section 2.3 (which instead are fundamental for the further discussion of the results in section 3). An additional observation is that while the individual components (AMOC box model and

CC model) have been previously applied in the literature, with these modifications we are now talking about quite different models, to what extent are these now comparable to the 'full' version of the components ?

Author's reply:
The AMOC dynamics are still very comparable to the literature (i.e. Cimatoribus et al., 2014; Castellana et al., 2019). We had to retune the model to keep CO_2 concentrations similar to the ones of the SCP-M. The different ocean circulation and box structure does change the model quite a bit. However, the most important aspects of the model are the carbon cycle dynamics. In the uncoupled case, these are still exactly the same. When couplings are introduced, we obviously change the model, the effects of these changes are one of the aspects we investigate in this study.

Changes in manuscript:
We have added a remark of the connection between the models in revised Section 2.3.

2. *The different couplings (identified with BIO, E_s, FCA, CS_{LO}, CS_{HI} by the authors) are mostly introduced in section 2, but FCA is described later, in the results section 3.1 instead. In general it would be good to introduce these labels close to the equations or maybe add equation numbers in table 1.*

   Author's reply:
   Suggestion followed.

   Changes in manuscript:
   We have made sure that the labels are introduced near the equations, and we have included equation numbers in table 1.

3. *The couplings presented in the main text (additional ones are introduced in the appendix) appear a bit like mixed bag of random choices.*

Some additional discussion on why these should be considered important and relevant couplings (also with reference to the literature) would be recommended. Which feedbacks could be expected associated with these couplings?

Author's reply:
Suggestion followed.

Changes in manuscript:
We have added additional motivation on the choices of the couplings. We also added an extra explanation on how a coupling would change the dynamics of the system.

4. *When the SST dependence on atmospheric CO_2 concentrations is introduced, with a simple model of climate sensitivity, it would be good to remind the reader what role SST plays in the model equations, which processes it does control.*

   Author's reply:
   Suggestion followed.

   Changes in manuscript:
   We have included a short explanation in the revised text.

5. *Actually a similar observation is valid also for other couplings: the rain ratio coupling in eq. 4) could be accompanied by a short reminder on its role in the biogeochemical cycle (or at least just repeat the description from line 83)*

   Author's reply:
   Suggestion followed.

   Changes in manuscript:
   We have included a short explanation in the revised text.

6. *L217-210: It is said that when the BIO coupling is not used, then* PO_4 *concentrations become negative in the surface ocean under a collapsed AMOC regime. This is not shown in any plot (not even in the appendix) and in general this sounds quite ominous: negative concentrations? Is this a numerical issue? Which mechanism leads to the drop in concentrations if a fixed biological export production in the surface boxes is used. If this is so, why is the BIO coupling actually considered an option and not integrated permanently in the model?*

   Author's reply:
   In the original SCP-M, biological export production is constant. In our coupled model, as the AMOC collapses, advection of PO_4 into box n decreases a lot. At some point on the unstable branch, the source term of PO_4, i.e. mixing of PO_4 into box n through r_N and the AMOC, becomes smaller than the constant export production due to a weak AMOC and PO_4 concentrations will become negative. This shows that the original SCP-M, and our model without the BIO coupling, are not able to accurately simulate the carbon cycle of an AMOC off state because of missing processes. A main missing process is that biological production will decrease if nutrient concentrations decrease because of increased nutrient limitation. This process is captured in the BIO coupling which enables the model also to simulate a reasonable carbon cycle at the AMOC off state. It is a good suggestion to integrate the coupling permanently in the model, and if this model will be used in further research, that will also be the case. However, here we introduce the coupled model, i.e. the coupling between the carbon cycle processes of the SCP-M and with the ocean dynamics of the Cimatoribus box model, and we wanted to start off with staying as close to both original models as possible and then add new processes. This means we wanted to start of with a constant export production as modelled in the SCP-M and investigate how adding additional process affects the model.

   Changes in manuscript:
   We have clarified the text around L217 to better explain the reason for the negative PO_4 concentrations.

7. *Table 1 is introduced in line 237, after already on the previous page the impact of different couplings has been discussed.*

   Author's reply:
   It would indeed be more convenient if the table is placed earlier in the text.

   Changes in manuscript:
   The table is placed earlier in the revised text.

Other questions are:

1. *To what extent are these results sensitive to particular modelling choices (such as for example the depth of the boxes?)*

   Author's reply:
   We have tested the sensitivity to several variables, among which the rain ratio, depth of box n, and the strength of the global overturning circulation (ψ_1). Both the depth of box n and the global overturning circulation had little effect on the results. The rain ratio does have a strong effect on the CO_2 concentrations because it plays an important role in the burial of carbon in the sediments. The value of the rain ratio was chosen such that the atmospheric CO_2 concentrations on the AMOC on branch are around pre-industrial concentrations.

   Changes in manuscript:
   No changes necessary.

2. *The relationship between E_s and atmospheric CO_2 concentrations derived from CMIP6 models and described in eq. 8 could also be interpreted as a function of temperature, so should its use not be linked also to the activation of the climate sensitivity feedback?*

Author's reply:
It is indeed possible that the increased freshwater flux is also related to the temperature. It would therefore make sense to link it to the activation by the climate sensitivity feedback. However, by decoupling them as we did in this study, we can study the separate effects of both the change in temperature and the change in salinity. This is especially convenient because in this model set up the change in temperature mostly affects the carbon cycle dynamics while the salinity changes mostly affect the AMOC dynamics. However, we believe it is good to discuss this choice more explicitly in the text.

Changes in manuscript:
We have included a few sentences on this choice in the revision.

3. *L228 and onwards: the total carbon content in the ocean+atmosphere system is kept fixed. I believe that some additional explanation on this hypothesis is due. What about carbon in terrestrial vegetation and soil carbon ?*

   Author's reply:
   In this model we do not consider the terrestrial biosphere and therefore do not take changes in vegetation and soil carbon into account.

   Changes in manuscript:
   The model detail has been clarified.

4. *L237 onwards: there is no reference to the effect of introducing the E_s coupling compared to the BIO case alone.*

   Author's reply:
   Correct. We had not included one because there were hardly any changes in the model. However, for completeness, we have added it now.

Changes in manuscript:
A sentence discussing the E_s coupling compared to the BIO case has been added.

5. *Why were only these 'incremental' combinations of couplings explored? Is there a reason why only certain combinations should be used? I understand that considering all combinations might be confusing but maybe a short comment would be good.*

   Author's reply:
   In principle, we could have performed many other combinations of the feedbacks. However, this would make it quite complicated to describe everything clearly in the manuscript, and indeed would probably lead to confusion. This is why we try to keep the experiments presented in the main text relatively simple and use these incremental steps to keep all the different cases relatively similar in set up. The exact choices presented in the main text are based on what feedbacks had the most pronounced effect on the AMOC and the MEW.

   Changes in manuscript:
   We have added a comment one the motivation of the used cases.

6. *L239: I might have missed it, but is there an explanation why the FCA coupling increases atmospheric CO_2 ?*

   Author's reply:
   This is indeed not mention explicitly in the main text. The FCA coupling adjusts the rain ratio, i.e. the relative amount of $CaCO_3$ in the biological export production, which affects the amount of DIC and Alk burial in the sediments. In the setting used here, the FCA coupling reduces the rain ratio, lowering the DIC and Alk burial in the sediments. As the river influx needs to balance the burial in the sediments, CO_2 concentrations decrease to lower the river influx.

   Changes in manuscript:

An explanation has been added in the revised paper.

7. *The fact mentioned on line 255 that the on-branch becomes unstable before reaching the saddle-node bifurcation (due to a Hopf bifurcation): 1) is this an hypothesis or was it confirmed by AUTO? 2) Please clarify somewhere if the MEW is defined as the range between the saddle-node bifurcations or between the left saddle-node of the off-branch and the hopf bifurcation on the on-branch.*

   Author's reply:
   1) This is confirmed by AUTO, and 2) we have defined the MEW as the range between the two saddle-node bifurcations.

   Changes in manuscript:
   This has been clarified in the revision.

8. *The top axis of Figure 4 reports CO_2 values between about 50 and 750 with E_s varying between 0.25 and 0.50. Compared with fig 2c this does not look like the same fit (in that figure CO_2 between 400 and 1200 has E_s between 0.4 and 0.5).*

   Author's reply:
   Correct; thanks.

   Changes in manuscript:
   Figure 4 has been corrected.

9. *The fact that changes in SST (as modelled by eq. 3) do not affect ocean circulation in the model is discussed in the conclusions but indeed this might be a major drawback. Particularly through arctic amplification feedbacks, changes in the mean state can be associated with important changes in the meridional gradient of temperatures. Maybe the discussion on this point in the conclusions could be expanded.*

Author's reply:
We agree.

Changes in manuscript:
The discussion on this has been expanded in the revised paper.

10. *I realize that this is outside the scope of this study, but processes linked to sea-ice represent a major element affecting the strength of the AMOC, a comment might be in order. This could also be linked with the missing dependence of AMOC on model temperature which the authors recognize in the discussion.*

    Author's reply:
    We agree.

    Changes in manuscript:
    The discussion on this has been expanded in the revised paper.

11. *In the conclusions it could be beneficial to add a short comparison of these results (in particular the identification of the most important mechanisms) with other studies, maybe based on proxy data or on modelling with more complex climate models.*

    Author's reply:
    Suggestion followed.

    Changes in manuscript:
    We have included extra discussion, also based on comments from reviewer 2, where we compare our results to other studies.

Minor issues:

1. *Line 177: for reproducibility it would be better to list somewhere which 28 CMIP6 models were used, which ensemble members*

   Author's reply:
   We agree. A list of the models is included in the repository corresponding to the paper. However, we had already prepared a list to also include in the supplementary material, but for some reason it was not included in the submitted manuscript.

   Changes in manuscript:
   A list of models and what ensemble members are used have been included in the supplementary material.

2. *Line 301: "These clear and plausible mechanisms. . . ." → Which mechanisms? The previous sentence is about the CMIP6 fit . . . . . probably this paragraph (or the previous one) is not in the right place.*

   Author's reply:
   There was indeed a mistake in the order.

   Changes in manuscript:
   The order of the text has been revised.

References

- Castellana, D., Baars, S., Wubs, F. W., and Dijkstra, H. A.: Transition Probabilities of Noise-induced Transitions of the Atlantic Ocean Circulation, Scientific Reports, 9, 20 284, https://doi.org/10.1038/s41598-019-56435-6, 2019

- Cimatoribus, A. A., Drijfhout, S. S., and Dijkstra, H. A.: Meridional overturning circulation: stability and ocean feedbacks in a box model, Climate Dynamics, 42, 311–328, https://doi.org/10.1007/s00382-012-1576-9, 2014.

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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 #2 of the revision

October 3, 2024

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

This manuscript has gone through one review cycle. While I was not involved with the first review cycle, I have read the two review comments and the responses by the authors, and I believe the authors sincerely responded to the review comments. This is an ambitious work trying to evaluate the role of fully interactive carbon cycle to the AMOC multiple equilibrium window. There is high interest in the control of tipping points in the earth's climate system.

My main concern is about this manuscript is the mismatch in the timescale of global carbon cycle (including weathering and whole ocean inventory change) and AMOC (being a regional climate phenomenon). Re-organization of the global carbon cycle seems to involve much longer timescales than that of AMOC equilibria. In the discussion section, the authors stated that "Though not a limitation in the model, it is good to note that the range of timescales in the carbon cycle model is larger than in the circulation model, which does not affect our results but does affect the time dependent response of the system". I disagree with the above statement. The authors highlighted the importance of the balance between river input and sedimentation. For carbonate weathering system, its timescales are considered to be 10k-100k years, and the silicate weathering is on the order of 100k-1M years. These timescales are much longer than the timescale of AMOC variability O(1k year). On the timescales relevant to river input and sedimentation, there are other important changes in physical climate system that are not considered in this study, such as orbital parameters and the growth/decay of continental ice sheets.

For shorter timescale relevant to the AMOC variability, the internal re-distribution of carbon and alkalinity within the ocean can play more im-

portant roles. For example, the authors did not discuss the effect of changing ocean ventilation to the partitioning of carbon between ocean and atmosphere. There are studies making significant progress in understanding the important role played by AMOC (e.g. Goris et al., 2018; Katavouta et al, 2021; Zhang et al., 2024). The authors argue that the equilibrium solution is primarily controlled by the balance between river input and sedimentation for the set of processes represented in the model. In reality, there could be different processes dominating at different timescales. My suggestions to the authors are (1) to clarify what are the relevant timescales for this study and (2) to reference the work by others who examined the role of AMOC on the ocean carbon cycle over different timescales, and (3) discuss the main results in the context of the existing literature. In the previous works based on CMIP historical/scenario runs, the ocean carbon uptake in the subpolar North Atlantic decreases in the future climate with potential slowdown of AMOC. At the superficial level, this seems at odds against the lower atmospheric pCO2 in the off-state. Please explain how the transient and equilibrium solutions are different with respect to the AMOC's role.

Author's reply:

The change in timescales does not matter for our results since we are looking at steady states. Variability on shorter timescales, such as adjustment of the ocean to AMOC tipping, does not play a role a role here. Steady states are determined through parameter continuation, and transient behavior is not considered. We agree that this needs clarification.

We agree with the suggestion to include a more thorough discussion on how this work relates to existing literature.

Changes in manuscript:

We have added a discussion on the steady state approach for clarification. We have, also based on comments by reviewer 1, included a discussion where we compare our results to existing literature where we also highlight the timescales involved in different studies.