Response to Reviewer 2

We thank the reviewer for their report and their useful comments. We appreciate in particular their very thorough assessment of our manuscript. We reply to their points in the following.

Summary:

> *Soons and co-authors present a conceptual study on the role of the Bering Strait for AMOC stability. They extend the five-box model by (Wood et al., 2019) with a simple parametrization of the Bering Strait throughflow and explore its impact on AMOC stability on idealized hosing and so-called Arctic amplification experiments. The authors find that the box model is able to reproduce some features also present in the CLIMBER-X intermediate complexity model and may thus serve as a toy model to better understand certain dynamics involving the Bering Strait. The manuscript is generally well written, though rather technical, which makes it partly hard to follow as it also uses a lot of terminology specific to this box model and abbreviations. While I do see the value of such simple conceptual models for fast hypothesis testing and understanding dynamics, I am not fully convinced by the motivation. The authors make repeated comparisons to results with the CLIMBER-X EMIC (mostly the author's earlier study on the same topic), and how the box model can reproduce a limited number of findings from the more complex model. However, I wonder what the benefit of this box model truly is. CLIMBER-X is a very fast ESM with simplified but complete physics, that is fast enough to do all the experiments done here with the box model with very moderate computational resources. One could therefore argue to directly use the EMIC and not the box model. I want to reiterate that I'm not saying that such box models are useless, I'm only questioning the framing in this study. Generally, I would advise the authors to drop the comparison to CLIMBER-X, and to treat this really just as a useful but conceptual model. I'm also sceptical of the entire framing of a Bering Strait closure as a geoengineering option to mitigate an AMOC collapse. Instead, I would change the framing to one of an interesting academic problem, but not more. There also seems to be a more fundamental issue with the numerics of the timescale, which needs to be resolved (see my comment below).*

Major comments:

1. **Referee's comment:**

   > *You only seem to describe simulations from Soons and Dijkstra 2025 in section 2.1. I think a reference or a brief summary would have been enough. Though, generally I think that the extended comparisons to CLIMBER-X go too far here. Tuning the model to the EMIC is fine, but a comparisons to emission driven experiments in CLIMBER-X are not really commensurable and should be removed. While the box model extension is described in detail, I'm missing a description of the hosing and arctic amplification experiments in the main text. Especially, the latter is somewhat obscure at least for earth system modellers, and needs to be better explained and motivated.*

   Reply:

   > We will now remove model details from section 2.1. We do think, however, that providing the specifics of the CLIMBER-X experiments improves readability, and will retained those. We will also add a subsection to the Models section, where the forcing protocols for the hosing and Arctic amplification

experiments are described and motivated. We respectfully disagree regarding the comparison with the emission-driven experiments in CLIMBER-X. A key point of our study is qualitatively explaining the results seen in the EMIC. We will justify now in the Models section that our representation of the temperature-induced destabilization of the AMOC captures the main mechanism affecting the AMOC that is present in more-detailed models.

2. **Referee's comment:**

   I tried to reproduce the results of the extended Wood model myself, and found a mismatch in the timescale of Fig. 8. I cannot reproduce the timescale of that figure by a factor of 100, such that the transient response is wrong. With Table C1 as printed, the response is slow. Box flushing times are V_N/q_n *about* 60 *yr,* V_T/q_n *about* 160 *yr and* V_B/q_n *about* 1800 *yr. The AMOC in Fig. 8a (*$F_H = -0.2$ *Sv,* $r = 1/yr$*,* $\Delta T = 8.6\,^\circ C$*) needs more than* 100 *yr to reach* 0 *Sv and about* 250 *yr to approach* -7 *Sv (Fig. E). The paper shows* $q_n = 17.5 \rightarrow 0$ *Sv in about* 5 *yr and the final state of* -10 *Sv by about* 35 *yr. Multiplying all time derivatives by* 100 *(equivalent to dividing all volumes by* 100*) reproduces the paper's transient results. It seems* t_{dim} *is off by a factor of* 100*. This would mean that all time axes are* 100 *times too short and all forcing* 100 *times too fast. I urge the authors to double check and correct the results, as this may compromise many results of the study especially the comparison to CLIMBER-X, because the forcings rates are* 100 *times faster in the box model. Also, there seem to be some inconsistencies between the the equations in the appendix and the Zenodo code, such as terms missing in eqs. A1b and A3b, or a wrong sign in the hosing equation.*

   Reply:

   Well spotted! Indeed in our non-dimensionalization of the equations the time scaling factor was a factor 10^2 too large. This only affects our results quantitatively: the forcing rates are reported a factor 10^2 too large, while the time axes a factor 10^2 too small. We will adjust this in Figures 7 to 9, and in the text. Moreover, this now reduces the large time scale discrepancy between our conceptual results in Figures 8a&b and the EMIC results in Figures 8d&e. There were indeed terms missing in eqs. A1b and A3b, namely those representing the subtropical gyre exchange for the tropical box. These will be added to the equations in the Appendix.

Minor/technical comments:

1. **Referee's comment:**

   L1: EMIC has not been defined in the abstract.

   Reply:

   Indeed, this will be adjusted.

2. **Referee's comment:**

   L65: What is the Bering Strait Throughflow in CLIMBER-X under PI conditions? Please mention this here and how this compares to the observations.

Reply:

The Bering Strait Throughflow is parametrized in the EMIC, where it is only represented as a baroclinic tracer exchange, and does not contain a barotropic component. Therefore, we will only add a comparison between the modelled transport of freshwater and the observed transport. This transport is an order of magnitude too small in CLIMBER-X, and so –as shown in the manuscript– the response of the AMOC to a closure of the Strait in CLIMBER-X is small compared to the response in other models.

3. **Referee's comment:**

L219 and following: The parameterization of the Arctic amplification needs to be better explained and why this is equivalent to a global warming scenario.

Reply:

We will add a justification of the Arctic amplification forcing to the Model section. We think that this conceptual framework is qualitatively the same as a global warming scenario in regards to an AMOC weakening, since the main mechanism through which the AMOC is affected, is captured in the Extended Wood Model. Under global warming scenario it is namely the heating of the North Atlantic surface waters that precede the AMOC weakening and lead to the onset of its collapse via the salt-advection feedback.

4. **Referee's comment:**

L 225: Give a brief explanation why arctic amplification on its own causes the AMOC tipping in general (I presume due to its effect on the buoyancy forcing) and how realistic the mechanism really is.

Reply:

It is indeed due to its effect on the buoyancy forcing. An AMOC weakening is preceded by the heating of the North Atlantic surface waters, which inhibits deep convection at these sites. This in turn causes the weakening, which is then amplified via the salt-advection feedback (Drijfhout et al., 2025). Of course other effects at lower latitudes also play an effect, but under rapid CO_2 increases the dominant cause is Arctic Amplification (Dai, 2022).

5. **Referee's comment:**

L225-226: This sentence is confusing (at least to me). Do you mean to say that a closed Bering Strait allows for higher Arctic amplification temperatures in the negative freshwater hosing regime? Please clarify.

Reply:

That was indeed the intention of the original sentence. We will now rephrase it.

6. **Referee's comment:**

L226: To me it seems that the effect of a closed versus open Bering Strait is vanishingly small for the high hosing regime (Fig. 5e, 6a) and not the reverse of the negative hosing regime. Am I missing something.

Reply:

The salinity of the deep box (S_B) remains relatively unchanged under hosing compared to the salinity of the North Atlantic box (S_N), since the latter has a smaller volume. In the high hosing regime the salinity difference $S_B - S_N$ is smaller, and so the salinifying effect of a reversed BST is smaller too, as can be deduced when comparing Figures 5b, 5d and 5f. This will be mentioned in the text.

7. **Referee's comment:**

L273: How can you distinguish between hosing induced versus arctic amplification induced when both may contribute to the tipping point for $F_H > 0$, especially in the regime just below 0.3 Sv, close to the hosing induced tipping point?

Reply:

We use "hosing-induced tipping" to refer to an AMOC tipping induced by a change in the hosing amount F_H. As in this case of an AMOC collapse the amount of hosing does not change, while the northern Atlantic's atmospheric temperature T_N^a does, we refer to this as a temperature-induced tipping. Of course the background hosing is a destabilizing factor, but it did not cause the tipping.

8. **Referee's comment:**

L275: I don't really understand the link to the CLIMBER-X remaining carbon budget. I think the connection to CLIMBER-X has not been established here, at least with regard to this specific box model setup of combined hosing and arctic amplification. Because the latter ha no clear equivalent in the EMIC. I think showing the CLIMBER-X result here is rather suggestive that the simple box modelling can be connected to a remaining carbon budget, which it cannot. I therefore strongly suggest to remove this panel from the figure.

Reply:

The justification of using Arctic amplification in the conceptual model as a qualitatively similar forcing as global warming in an emission-driven scenario in the EMIC with regard to an AMOC weakening has now been added to the Model section, and has been discussed in previous answers. We do think the conceptual model results can be linked in a qualitative way to the carbon budget in the EMIC, as a larger amount of emitted carbon will result in a larger warming of the higher latitudes compared to the warming of the lower latitudes. This will be made more explicit by comparing these warming differences directly with the emitted total carbon in the EMIC with a supplementary figure. This relationship is reasonably linear. Hence, we think we can justify the comparison presented in Figure 7, since a larger amount of carbon released at 1%/yr implies a larger peak in Arctic amplification at the end of the forcing period, i.e. our idealized ΔT_N^a. Of course, this relationship is not that straightforward for lower carbon release rates (Hankel and Bonan, 2026), which will also be mentioned in the text.

9. **Referee's comment:**

L310: I guess you mean the forcing profile of equation 4? Clarify.

Reply:

Indeed, this will be made more explicit in the text.

10. **Referee's comment:**

L357: While I see this effect in the salinity, I don't see the delay in the AMOC panels of Fig, 8a,d.

Reply:

Agreed, we will add a supplementary figure that highlights the difference between the OBS and CBS trajectories.

11. **Referee's comment:**

L361: If you want to mention CLIMBER-X, give some examples of such destabilizing effects in the model (including some supplementary figure).

Reply:

Agreed, we will now include a supplementary figure depicting the increase in surface freshwater flux into the North Atlantic, and the slight but continued increase in northern hemispheric temperatures during the EMIC simulations. Both inhibit the AMOC's recovery in the EMIC but are not captured by the conceptual framework.

12. **Referee's comment:**

Figures: Color of the experiments with different hydraulic constants are hard to distinguish in all figures and should be changed.

Reply:

We will adjust the coloring to make the experiments easier to distinguish.

13. **Referee's comment:**

Figure 6: This is the Hopf point not the saddle-node. The $\Delta T_N^{a,1}$ in matches the first loss of stability along the ON branch, which is a Hopf bifurcation. See also L.240-249. Also, Fig. 6c does include ν_{20} at $q_0 = 12$ Sv, although the text says this case is omitted.

Reply:

The $\Delta T_N^{a,1}$ indeed matches the point of first loss of stability along the ON branch, regardless of whether it is a saddle-node or Hopf bifurcation. This was not clear from the original text, and will be clarified. Also, we will remove the mentioned data point from Figure 6c.

14. **Referee's comment:**

Figure 7: The y-axis should have units of °C not Sv.

Reply:

Agreed, this will be adjusted.

References

Wood, R.; Alkhayuon, H.; Ashwin, P.; Hawkins, E.; Jackson, L.; Quinn, C.; Rodriguez, J.; Smith, R. Observable, low-order dynamical controls on tipping behaviour of the Atlantic Meridional Overturning Circulation. Geophysical Research Abstracts. 2019.

Drijfhout, S.; Angevaare, J. R.; Mecking, J.; Van Westen, R. M.; Rahmstorf, S. Shutdown of northern Atlantic overturning after 2100 following deep mixing collapse in CMIP6 projections. Environmental Research Letters **2025**, 20, 094062.

Dai, A. Arctic amplification is the main cause of the Atlantic meridional overturning circulation weakening under large CO2 increases. Climate Dynamics **2022**, 58, 3243–3259.

Hankel, C.; Bonan, D. B. Transient evolution of polar amplification under different CO2 ramping rates. Geophysical Research Letters **2026**, 53, e2025GL120079.