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Version: Revision II

Title: A Saddle-Node Bifurcation may be Causing the AMOC Collapse in the Community Earth System Model

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We thank the Reviewers again for their careful reading and for the useful comments on the revised manuscript.

Point-by-point reply to Reviewer # 1

I appreciate the addition of Appendix B on the normal form of the saddle-node bifurcation and the discussion of the nonautonomous case. The explanation for the half-rate forcing case is much improved now, and the corrected reference seems appropriate. I do not have any further comments and recommend acceptance for publication.

Author's reply:

Thank you!

Point-by-point reply to Reviewer # 2

I would like to thank the authors for their detailed response to the previous round of comments, this is a much improved manuscript and I believe the clarity of the arguments is now much stronger. Overall, I believe that the main areas for improvement in the manuscript have now been address and only minor corrections now remain. I would support publication of this manuscript once these points have been addressed.

One significant conceptual point which remains for me is around Figure 9 and the cubic spline data. I believe that Fig 9a shows key information about the uncertainty of the minimum in the F_{ov}. However, it is very unclear to me why fitting cubic splines is the right thing to do here, which it should match the functional form, and why it gives rise to the very uneven and bimodal structure in the finer resolution pdfs which I believe may be spurious structure based on a very limited dataset. I am concerned additionally by the spikes in the pdfs outside of the fitted range which are shown in the figure. These seem spurious and confusing to the reader, as I suspect they are not realistic and are artefacts of the fitting system. I think that this approach should be considered and potentially compared to approaches such as Gaussian Processes to understand if they are adding information beyond that in Fig 9a or whether they are adding structure which cannot be reasonably inferred from the available data and could be misleading. If this approach remains in the paper then I would like it to be explained in more detail, particularly around the spatial structure and bimodality. I do not believe this significantly impinges on the conceptual importance of the paper and I think Fig 9a is strong enough to stand on its own and makes the point clearly enough, but I think that the cubic splines section should be reconsidered.

Author's reply:

The cubic splines were motivated by the procedure outlined in van Westen et al. (2024, https://www.science.org/doi/full/10.1126/sciadv.adk1189). However, as pointed out by the reviewer, the limited number of statistical equilibria indeed gives rise to spurious behaviour (see thin lines in Figure 9a).

To obtain an unbiased estimate of the F_{ovS} minimum, all F_{ovS} combinations of the four statistical equilibria (i.e., 6,250,000 combinations) are considered, from which the frequency of the F_{ovS} minimum per statistical

equilibrium is determined. The frequencies are: 1.1% ($\overline{F_H} = 0.45$ Sv), 21.7% ($\overline{F_H} = 0.465$ Sv), 43.2% ($\overline{F_H} = 0.48$ Sv) and 34.0% ($\overline{F_H} = 0.495$ Sv), with the weighted F_{ovS} minimum at $F_H = 0.482$ Sv. This indeed confirms that the F_{ovS} minimum is most likely found for $\overline{F_H} = 0.48$ Sv.

Changes in manuscript:

We kept the cubic spline procedure in the revised manuscript to demonstrate spurious behaviour for a low number of statistical equilibria. Revised Figure 9b now shows the four cumulative distribution functions of F_{ovS}, which are used to demonstrate that the F_{ovS} minimum is likely found for $\overline{F_H} = 0.48$ Sv. The main text is changed accordingly.

Minor Comments

1. *Line 24: Perhaps ref Wood (2019) box model here as well*

   Author's reply:

   This is a relevant reference here.

   Changes in manuscript:

   Reference incorporated.

2. *Line 63-65: With the section removed from the previous manuscript, this doesn't read as well and might now benefit from an "and" or some grammatical restructuring*

   Author's reply:

   Agreed.

   Changes in manuscript:

   Sentences were rewritten.

3. *Line 165: It is somewhat unclear from this text whether the greater overshoot in forcing is expected at the slower rate or not, and if so*

then why this would be expected. Additional explanation here would be appreciated.

Author's reply:

This is hard to assess, as the sensitivity in overshooting/undershooting the saddle-node bifurcation depends on initialisation conditions, forcing rate, and hosing location. This was explicitly demonstrated for the E-CCM in Section 3.2 and Figure 6.

Changes in manuscript:

We comment on this point when introducing the half-forcing rate simulation.

4. *Figs 2 and 5 are improved but the text is still rather small, if this could be improved at all that would greatly improve them, perhaps the subplot titles could be placed onto the empty space in the subplots to allow more room for expansion? I think the figures are manageable as is, but improvements would be welcome*

   Author's reply:

   Suggestion followed.

   Changes in manuscript:

   The figure captions, the x- and y-ticks and the x- and y-labels are displayed at a larger size.

5. *Line 209: Remind the reader that E_A is the freshwater forcing, or potentially harmonise such that F_H is freshwater forcing throughout the paper, although I understand that they are treated in different ways so this may be confusing.*

   Author's reply:

   We prefer to use E_A for the E-CCM as the freshwater forcing is different compared to the CESM. A reminder to E_A is useful here.

   Changes in manuscript:

   Suggestion followed.

6. *Line 209-210: Ref Fig 5 to support this claim.*

Author's reply:

Agreed.

Changes in manuscript:

Suggestion followed.

7. *Line 284: "Similar as in the Stommel Model" should be "Similar to the Stommel Model"*

   Author's reply:

   Agreed.

   Changes in manuscript:

   Corrected.

8. *Line 324: "precies" should be "precise"*

   Author's reply:

   Agreed.

   Changes in manuscript:

   Corrected.

9. *Line 355-356: Some comment on how reasonable it is that these terms are small would be helpful, is there a citation or some analysis from models which suggests the order of magnitude of these terms and whether they can be safely ignored?*

   Author's reply:

   Figure 7 shows the F_{gyre} and F_{ovN} contributions up to $F_H = 0.51$ Sv, which is beyond the critical value of $\overline{F_H} = 0.48$ Sv (Figure 2f). This supports our claim that these terms remain reasonably small prior to the onset of the AMOC collapse. Thereafter, they have a considerable contribution (e.g., Figure 4) and is attributed to large-scale AMOC changes.

   Changes in manuscript:

This is now discussed in the revised manuscript.

10. *Fig 8: I think that "observed values" is still slightly misleading, perhaps it could be "CESM Model" and "Obs Model" just to be completely clear that this is idealised model data and not observations*

    Author's reply:

    Agreed, observed model is more appropriate here.

    Changes in manuscript:

    We have revised Figure 8 and the text accordingly.

11. *Line 427: "Above-averaged" should be "above average"*

    Author's reply:

    Agreed.

    Changes in manuscript:

    Corrected.

12. *Line 487: "Second argument" should be "The second argument"*

    Author's reply:

    Agreed.

    Changes in manuscript:

    Corrected.

13. *Line 500: "loses resilience and making it more" should be "loses resilience, making it more"*

    Author's reply:

    Agreed.

    Changes in manuscript:

    Corrected.

14. *Line 574 appears to be simply completing the square on the previous step, if the procedure from Faure Ragani is more significant than this or is imparting additional meaning to this procedure it would be helpful to state this explicitly*

Author's reply:

Indeed, this is simply completing the square. This rewritten form motivates the scaling of variables.

Changes in manuscript:

We have rewritten these sentences.