Replies to Reviewer #1:

Thank you very much for all of your constructive comments. We have carefully revised our manuscript based on the advice by you and other reviewers. The following are our point-by-point replies.

This manuscript is a follow up of two previous studies published in Journal of Climate. As its predecessors, this study uses an idealized box-model to understand the centennial variability of the AMOC. The difference here is the addition of a Southern Hemisphere and subtropical wind.

This is the second round of review of this manuscript. I am sorry to say that my major concerns remain. There is basically no significant new results or novelty from previous analyses. Also there is still physical issues and misunderstanding.

I did not see any improvement of the manuscript. In particular, the manuscript still does not provide any new fundamental insight compare to LY22 and YYL23. It is also now misrepresenting Sévellec et al (2006), a 20-year-old paper, to try to convey the novelty of their study… This is quite unacceptable!

Please see below my more detailed comments.

Hence, I still recommend this work to be rejected.

Major Comments:

1. *In the manuscript the authors argues that they want to describe the simplest explanation for the centennial oscillation. This new study demonstrate that it was already done in LY22 and YYL23. No significant change can be seen by the addition of the southern hemisphere or wind. The authors agree with my comment in their response. This shows that there is no novelty in this paper.*

2. *The author are now suggesting that their mode is different from Sévellec et al (2006). This is not acceptable. Also they imply that the mode is more complex or less well explains in Sévellec et al (2006). This is not true (See their section 4). There is a full derivation of the stability, including stability and mechanism explanation in a setting with 4 variables (where 2 are almost slave because of atmospheric relaxation). It is hard to have a simpler model than that and to still allow oscillation… This paper, together with LY22 and YYL23 (who find several results equivalent to Sévellec et al, 2006, as they acknowledged), seals the deal. There is absolutely no novelty in the submitted manuscript.*

3. *Inclusion of the subtropical wind - this part is still simply wrong. By trying to fix the issue the authors have made several more mistakes.*

a. Based on their schematic Fig. 1b, the wind would not transport anything… The return flow being between the same boxes that the surface flow… This mean that the net baroclinic heat and freshwater transport would be zero, since no baroclinic structure of temperature and salinity exist within a single cell... Also this flow schematic is inconsistent with the set of equations (7). After a round of review, where I asked mass to be conserved, this inconsistency/mistake is worrying. My only conclusion is that the authors have not fully think through the inclusion of wind, despite my comment…

b. The derivation of the wind scaling in the response to my comment is wrong. There is absolutely no reason to set only "u" and not "|u|" as proportional to "dyT" and "|dyT|", respectively. This was my point… It would be better to set the wind stress to something proportional to "dyT.|dyT|" (since it is a function of "u.|u|").

Based on that I do not feel that the authors have properly implemented the wind in this second attempt. Given that they have added new mistakes, I am not convinced that they will be able to do it in the future.

In general there is plenty of false statement both in the text and the response. But, in my opinion, the three points above are salient enough to prevent publications, especially after a round of review which does not show any improvement of the manuscript regarding these points.

Reference:

Sévellec, F., T. Huck, and M. Ben Jelloul, 2006: On the mechanism of centennial thermohaline oscillations, J. Mar. Res., 64, 355-392.

Replies to Reviewer #2:

The authors attempt to explain the multicentennial oscillations as an inherent property of the Atlantic meridional overturning circulation (AMOC) using an extended two-hemisphere box models. They have addressed each question raised by the reviewers in detail and made improvements to the manuscript.

However, there is one point that requires further clarification from the authors: Previous studies have indicated that thermohaline circulation can exhibit three regimes: thermally dominated, salinity dominated, and oscillatory. There has been considerable research on the oscillatory regime, as such, Colin de Verdière et la. 2006, Colin de Verdière 2007, Sévellec et al. 2006,2010, Prange et al. 2023 . The authors propose that multicentennial oscillations occur between the centennial and millennial oscillations of the thermohaline circulation. It should be emphasized that a simple box model can effectively illustrate the mechanisms underlying the multicentennial oscillations of the Atlantic meridional overturning circulation, rather than from one-hemisphere box models extended to two-hemisphere box models.

References:

1. Colin de Verdière, A., M. Ben Jelloul, and F. Sévellec, 2006: Bifurcation structure of thermohaline millennial oscillations. J. Climate, 19, 5777-5795.

2. Colin de Verdière, A. 2007. A simple model of millenial oscillations of the thermohaline circulation. J. Phys. Oceanogr., 37, 1142-1155.

3. Prange et al., Sci. Adv. 9, eadh1106 (2023).

4. Sévellec, Florian, Thierry Huck, and Mahdi B. Jelloul. 2006. "On the mechanism of centennial thermohaline oscillations." Journal of Marine Research , 64 (3),355-392.

5. Sévellec, Florian, Thierry Huck, and Alain C. de Verdière. 2010. "From centennial to millennial oscillation of the thermohaline circulation." Journal of Marine Research, 68 (5), 723-742.

Replies to Reviewer #3:

The authors obviously made great efforts replying to all my comments. However, I found that the most concerning points are not addressed sufficiently, if addressed at all, and remain in this new version. The authors seem very insistent on some certain points, even if commented on by more than one reviewer. While I still think this work is an interesting and meaningful continuation of earlier papers, and makes the study on AMOC multicentennial oscillation more complete, I still have many major and minor comments on the manuscript. I would recommend a major revision. Please see comments below.

Major Comments

1. *Section 2a: As mentioned in previous round of review and by other reviewers as well, the Southern Ocean wind dynamics are wrongly represented in this model, especially when it extends all the way to 70S. It is wrong that q is not related to the zonal wind stress in Southern Ocean at all. It is not enough to argue that you are only interested in this specific behavior of AMOC so you can safely ignore Southern Ocean dynamics. You cannot do that because people know that AMOC will not behave correctly without a correct representation of Southern Ocean wind vs eddy dynamics in your model.*

2. *Still section 2a: As mentioned by other reviewers as well, the new findings in this paper are marginal compared to the earlier two papers. If you want to show that the role by wind-driven circulation is small, you can consider not including the domain south of 30S in your box model. At the southern boundary, you can impose a boundary condition for q, which is determined by Southern Ocean dynamics, but implicit in this case. Otherwise, extending the model from one-hemisphere to two-hemisphere without good new findings and incorrect representation of the Southern Ocean is meaningless.*

3. *Lines 150 –153: I still do not like the fact that (1) you have to specify q (will not be the case if you have Southern Ocean dynamics), and (2) q is so large. Your Fig. 10 shows that the overall behavior is similar with smaller, more realistic values of q. Why don’t you use those? You should at least have a discussion of why you insist on using 24 Sv, and discuss what are the advantages and disadvantages.*

4. *Equation 7: This is what I have a hard time processing. In your previous papers, you pointed out that inclusion of temperature does not qualitatively influence the behavior of the model. In the spirit of a simple model, that means you should not include temperature at all. To me, the reason why you want to have temperature is that you need to connect them with the “wind”, which is also wrong (see comments from other reviewers from previous round of review, or my comments below).*

5. *Also equation 7: I do not understand the role of qn and qs in the salinity and temperature budgets. In your schematic, there are green dashed arrows in the opposite direction to satisfy mass conservation. They seem to be in the same box as qn and qs and should just cancel their contribution in heat and salt transport? Shouldn’t there be cancellations or add-ons by qn and qs to the transports in deeper boxes instead? Or to extend this into a nine-box model with the Ekman layer separately.*

Comments on how wind-driven circulation is scaled:

1. *Equation (11) is correct.* Thanks.
2. *Equation (12) is confusing: if you express u as a linear function of meridional temperature gradient, why only one term but not the other? This is a non-linear scaling for wind-stress.*

3. *Equation (13)s directly confuse ocean temperature and air temperature. The concerns are at least three folds: (a) your restoring coefficient (~1 yr) is not a very fast restoring, therefore the sea temperature does not follow air temperature exactly; (b) the upper layer is too thick to represent sea surface temperature which may follow air temperature to some degree, even though you have very fast restoring. I pointed this out reviewing your previous paper before, the problem is much more severe here, (c) it may make more sense in a nine-box model with a separate Ekman layer, but that violates the spirit of a simple model by including more boxes and also temperature effect.*

4. *You can consider directly specifying wind stress from some observations instead of trying to connect it (wrongly) with sea temperature. In this case, you do not need to (I) extend to nine boxes; (II) include temperature which you have shown before to be less important, and therefore the spirit of a simple model is saved!*

5. *Sections 4b and 5: these sections do not add important new information, and therefore should be removed, at least from the main text.*

Minor comments

1. *Throughout your manuscript, please make sure that you use upper-class letters for "Meridional Overturning Circulation".*
2. *Line 62: "Deep Water" instead of "deep-water".*
3. *Lines 78–87: My own take-away from the LY22 paper is that the system needs some kind of nonlinearity to realized multi-centennial oscillation. Seems to me that it is not essential whether it is enhanced mixing or how MOC strength is determined, which makes it more like a mathematical behavior. The wrapping-up of LY22 in this paragraph seems to confuse the really important point.*
4. *Line 178: negative sign for the real part of the eigenvalue.*
5. *Lines 324–325: This argument does not make sense since you are focused on Atlantic Meridional Overturning Circulation. Therefore, Pacific is out of the question for this paper.*
6. *Lines 325–328: Move this sentence to be the beginning of next paragraph. It is more relevant there.*

Replies to Reviewer #4:

I have reviewed the current version of the manuscript and tried to assess the responses to the previous reviewers comments. For transparency, this is the first time that I have seen this manuscript.

Overall my feeling was that the reviewers had responded adequately to the many of the previous comments from the other reviewers, in particular on the relative novelty of the work, and the problems related to the inclusion of the winds and mass balance in particular. I think the clarification of the assumptions related to how the wind is responding to the North South temperature gradient was clearer, and largely reasonable.

However, I do share the other reviewers (especially reviewer #1) view that the paper was very incremental and has major issues. I am sympathetic with the authors that this manuscript does present novel, if incremental, results and so could be publishable in the Journal of Climate. Nevertheless, I feel that the authors still need to be clearer about some of the details of how they have formulated their model, but especially also the caveats. The inclusion of the wind is especially not really satisfactory, and I have to agree with reviewer 1 that the conclusions on this part of the model appear rather trivial in how they are currently constructed.

Expanding on the inclusion of the wind, although I think it is justifiable to have a relationship between the upper atmospheric jet and the North-South temperature gradient, how strong this relationship would be at the surface is very unclear. Furthermore, it is not clear where the 0.307Sv per C comes from, and this needs to be explicit. Given that the wind will always be opposing the "thermohaline" component in the model by construction (it doesn't drive variability, only responds to the variability of the "thermohaline" component in the opposite sense), should we really be surprised by that the wind "damps" the thermohaline variability. Additionally, should we be surprised that the wind by itself doesn't lead to variability in the absence of the thing that drives wind variability in this model (namely the "thermohaline" circulation)?

One obvious question that I think should be addressed would be how sensitive are the results to the strength of this relationship. A much more realistic case would likely be what would the role of *the winds be if the variability had a large stochastic component, e.g., does the centennial mode still exist? However, even in this case the model misses processes where by the wind drives the thermohaline circulation (e.g., high-latitude cooling in the North Atlantic, or Ekman driven upwelling in the South Atlantic).*

In the response, the authors repeatedly point out that they are just interested in what is controlling the anomalous centennial variability in a linearized sense. However, they also make the case that their model is not very realistic, e.g., they do not take account of changing winds in the southern ocean, and their assessment of the role of the wind in the mechanisms seems constructed into their results (e.g., wind variability doesn't independently drive AMOC, it just responds to the "thermohaline" modes impact on SST). All models have caveats of course (e.g., and do not resolve all the physics), but, this particular manuscript doesn't really discuss the many caveats of the model, and the shortcomings above leave me wondering what I have learned. For example, I really do not think the speculation on the D-O events or bond cycles is particularly relevant here given the limitations of the model.