Replies to Reviewer #3:

Thank you very much for these constructive comments. We have revised the manuscript carefully based on these suggestions. The followings are our point-to-point replies.

I thank the authors for their thoughtful replies and revisions, addressing almost all of my points from the initial review. In my view, the paper can be published subject to some minor revisions.

As a general point, since Climate Dynamics does not (to my knowledge) make the reviews available with the article, it would be good to consider incorporating some of the valuable material from the replies into the appendix or supplement, specifically the extended explanations of parameter choices as well as Figs. R3 and R5 (and some of the accompanying text).

Responses: Details about parameter choices have been added or revised in the main text (Lines 154-159, 186-187, 215-217, 522-527, and 640-644). The list of "selection reasons" has also been included in Table 1. A detailed introduction to the selection reasons is given in Appendix B. In addition, Figures R3 and R5, together with their explanations, have been added to Appendices C and E, respectively.

Major Comments:

1. *My only remaining major comment still relates to the issue of parameter values. Some choices are already better motivated, but others remain unclear in the text. Most importantly, it is still unclear how the values of lambda ("slightly lower" than in the GCMs according to the author replies) were chosen for the different model configurations. "To reflect differences in basin geometry and included processes" is not a satisfactory answer to that question because it might explain why they could be lower than in the GCMs, but if they were not estimated from these GCMs, how then? 21.3, 24.6 and 31.3 are such specific numbers that they must have been chosen according to some criteria, but as far as I can see they still appear out of the blue in the text without specifying how they were derived or chosen. It would also be good to report the CESM/EC-Earth values already here for context.*

Responses: Thank you for your comments. The specific values of λ are chosen to obtain the minimum period, as Figure 6 shows maxima in the imaginary part. This choice ensures a relatively robust oscillation period, since the imaginary part varies only slightly near this λ. The statement is added in Lines 215-217 and 522-527.

We also report the λ values obtained from the CESM and EC-Earth models. For CESM, with λ = 34.8 Sv kg^{-1} m^3, the multicentennial mode exhibits a period of 390 years and an e-folding time of 102 years in the 6S model, a period of 347 years and an e-folding time of 134 years in the 6TS_THC model, and a period of 344 years with an e-folding time of 283 years in the 6TS_THC+WDC model. For EC-Earth, with λ = 38.7 Sv kg^{-1} m^3, the multicentennial mode shows a period of 437 years and an e-folding time of 82 years in the 6S model, a period of 364 years and an e-folding time of 105 years in the 6TS_THC model, and a period of 351 years with an e-folding time of 186 years in the 6TS_THC+WDC model. All these eigenmodes fall within the multicentennial timescale. These statements have been included in Appendix B.

In L148 and following, it would improve clarity to distinguish parameters that were simply set to the same value as in CESM (e.g., mean AMOC strength) and parameters that were tuned so that another (related) property agrees well with CESM. If I understand correctly, that's for example the case for the F_w{1,2,3} parameters, which were optimized in a way that the mean salinities S_{1,2,3} match the CESM climatology, correct?

You are correct. The statement has been added in Lines 154-159.

There were some good explanations about some of the coefficients (e.g., "The is set to produce the desired amplitude of the self-sustained oscillation") in the author replies that would be useful to have in the paper itself.

These statements have been added in Lines 186-187 and 245-246, and are also mentioned in Appendix B.

Similarly, it would be good to also mention in the paper that the box model was never tuned to the oscillation period.

The statement has been in the discussion (Lines 640-644). It is also stressed again in Appendix B.

Finally, the authors could consider specifying the method of parameter estimation (literature, same as in CESM1, tuned to XXX) in Table 1.

The list of “selection reasons” has been added in Table 1 to specify the method of parameter estimation.

Specific comments:

1. *L228 and following: the enhanced vertical mixing parametrization could still be better justified in this paper. Currently, it is very hard to understand the motivation behind this formulation, which seems to be an essential ingredient for the oscillations. Instead of only referring the reader to LY22 in L239, it would be very helpful to give at least a brief, intuitive explanation here why vertical mixing is parametrized this way.*

Responses: Thank you for your comments. We agree that an introduction to this process is needed. The quadratic form is derived from regression analysis of the coupled model, which indicates that the effective mixing strength increases with the magnitude of AMOC anomalies. Physically, this reflects the fact that both strong and weak circulation states enhance mixing, albeit through different mechanisms: weakened stratification favors vertical diffusion when AMOC is strong, while intensified mesoscale and sub-mesoscale eddies activity enhances mixing when AMOC is weak. A squared term therefore provides a natural way to capture this symmetric dependence. We have incorporated this explanation into the revised manuscript (Lines 232-239).

2. *L352: "total basin volume is equivalent to the basin width" do you mean proportional?*

Responses: Thank you for your comments. You are correct, and we have replaced “equivalent” with “proportional”.

3. *L631: "for any plausible choice" sounds a bit strong, "for plausible choices" might be sufficient.*

Responses: Thank you for your comments. We have revised the expression as suggested.

4. *L643: I am unsure that the existence of an intrinsic ocean mode "strengthens the theoretical foundation for linking AMOC oscillations to Holocene climate swings". This would equally apply for a coupled (e.g., ocean-atmosphere-sea ice) mode of variability in which the AMOC plays a major role in mediating heat transport.*

Responses: Thank you for your comments. We agree that it should be presented as a supplement, not a strengthening, and have revised the expression accordingly.

5. *L656: "When the thermohaline circulation is disabled": add "in the box model"*

Responses: Thank you for your comments. We have revised the expression as suggested.

6. *L667: "Spatially resolved studies" -> "studies with spatially resolved models"*

Responses: Thank you for your comments. We have revised the expression as suggested.

7. *L690: "Similar caveats apply to idealized studies such as Mehling et al. (2023) and Vellinga and Wu (2004)" - why idealized? Of course they used GCMs which cannot be considered state-of-the-art today, but the more important point here is that there may be atmospheric water cycle feedbacks as (additional) drivers of centennial variability, which cannot be captured with fixed surface freshwater fluxes.*

Responses: Thank you for your comments. We agree that this point is not important in this context and have revised the related expression.

8. *The last paragraph sounds a bit (over-)enthusiastic at times, e.g. "precisely" (L699), "fully" (L698), "myriad" (L700), "unambiguously" (L701), "fundamental" (L707) could all be removed.*

Responses: Thank you for your comments. We have removed these words to present the paragraph in a clearer and more balanced tone.