We thank the reviewer for the detailed assessment and will address the concerns as listed below

This is a review of "Collapse of deep-sea circulation during an Eocene Hyperthermal Hothouse – A DeepMIP study with CESM1.2" by Winguth and co-authors.

The manuscript is well written and easy to read.

I wonder though what is the real added value of this manuscript. The model results are well described but are mostly a catalogue of known model results and the discussion to the now relatively large literature on the model results of the 1st phase of DeepMIP is very limited. For example, though precipitations occupy a central place in the text and the figures (Figs. 3, 4, 9, 10 and 12), there is no mention or discussion of the paper by Cramwinckel et al. (2023) that precisely focusses on the hydrological cycle in DeepMIP model results, or of the paper by Williams et al. (2022) on African hydroclimate.

In the revision, we have highlighted the importance of this manuscript while focusing on the deep-sea circulation and provide more analysis considering the literature mentioned above.

More importantly, given the title of the manuscript, the case for a collapse of the deep-sea circulation in the simulations described is sloppy at best. The title is misleading because there is no exploration of the mechanisms that make the deep-sea circulation collapse beyond stating that the MOC is less intense. The absence of diagnostics makes it impossible to judge whether the ocean is in "near-equilibrium" (as stated) and fails to provide evidence for the so-called subtropical haline mode that is said to exist at 12x CO_2.

In the revision, we have included a time series of pot. temperature at the surface and 4000 m including a computation of the drift for each scenario and compare this drift with the literature. In addition, we have added pot. density at 3000 m to Figure 13 to better assess the change in the deep circulation.

In my opinion, the manuscript should be significantly revised either to provide clear arguments and diagnostics in favour of the collapse of the deep-sea circulation and/or to clearly demonstrate how the results presented contribute further — than what the different manuscripts by Jiang Zhu and colleagues have demonstrated (using the same model with an updated atmosphere), and more generally than what the community has learned from DeepMIP phase 1 — to the understanding of the Early Eocene warmth and/or the PETM.

The revised manuscript better highlights differences to previous publications and includes a more sophisticated analysis of changes in the deep sea-circulation (see above).

Major issues.

There is no way to evaluate whether the ocean is in "near-equilibrium state". In particular, I seriously doubt that the high CO_2 simulations are really close to equilibrium but there are no time series of temperature in the manuscript that could be used to check if this is the case in the intermediate and deep ocean. For instance, the CESM1.2 DeepMIP simulations of Zhu and colleagues at 6x and 9x

CO_2 in the supplementary materials of Zhang et al. (2022) show that after 2000 years of integration the global mean ocean is definitely warming, and even more so in the intermediate/deep ocean if we assume that the upper ocean is close to equilibrium. I guess the same is happening in your high CO_2 simulations.

In the revision, we have included a temperature time series for both surface and deep-sea at 4000 m and have computed the drift to better assess how far the model has adjusted to the forcing material. In addition, a plot of pot. density at 3000 m give us a better quantification of water mass distributions which is compared to the literature listed above.

That the PETM warming generates a transient collapse of the overturning makes perfect sense but out-of-equilibrium snapshot experiments producing a sluggish circulation are in my opinion only poorly supporting this. For instance, with transient simulations, Alexander et al. (2015) nicely show that the PETM MOC collapses during the first few millennia and slowly reinforces until its intensity exceeds the initial pre-perturbation value (Fig. 5 of their supplementary materials). The simulations of Kirtland-Turner et al. (2024) also show this, although on a much smaller scale because the overturning only weakly slows down (their Fig. 3b).

The temperature time series of surface and deep water gives insights into the strength of the overturning series (see above). We discuss the limitations and reference the literature in the revised paper.

Section 1.2

The implementation of DeepMIP conditions should be better explained. It is described for aerosols but not for the other forcings mentioned l. 65-67. For instance:

- what are the differences in implementation between this manuscript and the simulations of Zhu and colleagues reported in DeepMIP (e.g. Lunt et al. 2021)? Notably, Zhu et al. implemented a specific marginal sea balancing scheme for the Arctic Ocean to conserve salinity in their DeepMIP simulations (Lunt et al. 2021, section 2.2.1). Is this also the case here? It might be useful to provide salinity time series as well.

A marginal sea parameterization for the Arctic has been implemented in this study and the model description has been revised accordingly.

- Herold et al. give a river runoff direction map. I would have thought you would use it directly (it was made for the CCSM/CESM model) but your sentence suggests otherwise.

Runoff was taken from a mapping file provided by co-author Christine Shields as part of the paleoclimate version of CESM1.2. We have clarified this in the revision.

- Eccentricity = 0.06 is not preindustrial. Is this an error?

Orbital parameters are selected for 1950 according to Berger 1989 and we corrected this in the revision of the paper.

- how was tidal dissipation implemented?

Tidal dissipation is set as a diagnostic in this simulation, and we clarify the manuscript accordingly.

- the preindustrial solar constant used is 1361 $W.m^{-2}$ but Winguth et al. (2010) uses a PETM solar constant of 1362 $W.m^{-2}$. Was the reference solar constant updated?

We used the preindustrial solar constant of 1361 W m^{-2} in this simulation, which has been clarified in the text.

- it looks like there is a missing minus sign in the initial temperature profile. And should it be 5000 rather than 6000?

Formula has been corrected accordingly.

Minor points.

l. 15-16. This sentence is correct but not easy to follow. Please reformulate.

We have revised this sentence accordingly.

l. 26, 189, 293. What do you mean by subtropical haline shallow mode exactly? The deepest MLD in the Northern Hemisphere in the 12x CO2 simulation is found westward of Greenland, as in the other simulations at lower CO2 (Fig. 6).

See above; in the revision we have included density at σ at 3000m to provide more insights into the water masses. Although mixed-layer depth deepens in the subtropics due to an increase in sea surface salinity in the $6xCO_2$ and $12xCO_2$ scenarios we have de-emphasized the haline mode throughout the revision.

l. 55-56. Perhaps use some of the diagnostics shown in Zhang et al. (2022) seeing as this paper analyzes the Early Eocene ocean circulation in DeepMIP models.

The revised manuscript has been better compared with Zhang et al. (2022).

l. 75. Easiest said that the Eocene ice sheet boundary condition is no ice.

We have corrected this statement accordingly.

l. 94-96. Remove. That the Eocene aerosol forcing has negligible effect has already been said, plus the simulation PB_PR is actually not shown anywhere.

We have removed this sentence.

l. 137. Unclear why the authors states that the simulated MOC is consistent with previous studies. To the least, Goldner et al. (2014) and Toumoulin et al. (2020) do not simulate bipolar deep water formation and Eslworth et al. (2017) may do so (though it is hard to state for sure based on the figures) but only with a deep Drake Passage.

In the revision, we have reassessed the agreements and disagreements with findings from the literature.

Data availability. Standard good practice today has it that the data should be open-access. Please provide the outputs the replicate the results.

We will provide open-access to data as soon as this paper is approved for publication in this journal.

Figure 10b is not used in the text.

In the revision, we have cited Figure 10b.

Figure 13. At which depth is shown the ideal age? Also, the arrows are hard to catch.

Depth is at 3000 m and we have improved the figure and figure caption accordingly. We changed also the color of the arrows.