Response to RC2 by an Anonymous Referee

We thank the reviewer for their constructive feedback and thoughtful suggestions. Below, we address each comment with the reviewer's text in grey/indented followed by our response.

> **Major comments**
>
> 1. **Absence of atmospheric forcing anomalies for the AIS.** This seems like a significant limitation and should therefore be acknowledged more explicitly already in the Methods (around L145). It is good that this aspect is discussed in some detail in L428 and following, but the discussion could be improved. For example, it would be very useful to add a supplementary figure that shows precipitation changes (currently "not shown" in L434). Then, could you indicate how including these temperature and precipitation changes might qualitatively influence the ice sheet mass changes (negatively or positively?). Could they even explain the (as you say, somewhat unexpected) absence of a significant AIS mass change?

Thank you for raising this point. We clarified this limitation in the Methods.
As suggested, we added the precipitation fields over Antarctica to the Appendix. We refined the discussion with a focus on the impact of the missing coupling at the atmosphere - ice-sheet interphase.

> 2. **Deep convection and AABW formation.** A maybe even more important limitation that likely influences some of the results is the typical but unrealistic AABW formation via open-ocean deep convection instead of on the shelves. One possibility to add better context would be to analyze salinity and density changes on the shelves (compare e.g. Li et al. 2023), but – as you write – this might be difficult to disentangle from the effects of the open-ocean convection. While it is adequately addressed in the Discussion, this shortcoming should be emphasized more clearly in other parts of the manuscript, especially in the model description and in the conclusions, e.g. around L457.

Our ocean model has a resolution of 3°x3° and consequently does not resolve the continental shelf around the Antarctic Ice Sheet. Therefore no analysis of salinity and density changes on these shelves is possible. We added this (common) low ocean resolution caveat in some parts to remind the reader.

> Please also consider mentioning the caveats related to points (1) and (2) in the abstract; they might be more important than the currently mentioned caveats (parametrizing eddies and resolving ice-shelf cavity flows [although I am not an expert on the latter])

We changed the Abstract accordingly.

3. **Model evaluation.** Since this is, from my understanding, a new model configuration, it would be good to add a short section (maybe in the Appendix) for model evaluation, especially in terms of key Southern Ocean characteristics such as T and S profiles in the relevant ice shelf forcing regions, sea ice extent and AABW strength. The initial AMOC strength, which appears a bit high, should also be compared to observations.

It is true that no model evaluation was published for this model so far. A new Appendix section was added to the revised manuscript, showing key variables compared to observational/reanalysis data.

4. **No salinity compensation.** According to L181, the experiment follows the protocol of NAHosMIP (Jackson et al. 2023), but this is not the case for salinity compensation. Jackson et al. (2023) use a volume compensation term which compensates for the hosing throughout the global ocean, whereas no compensation is applied here. This should be mentioned clearly (and maybe justified, if there was a specific reason to not apply any compensation). In addition, it should be mentioned more explicitly that, when applying a large hosing term without compensation, the ocean continuously (and artificially) freshens.

You are right, both suggestions are added to the manuscript. We describe more explicitly the compensation derivation compared to the NaHosMIP protocol. MOM5 has no option for volume compensation. Additionally surface compensation has a strong impact on sea surface salinity in the Southern Ocean and therefore open ocean convection around Antarctica. Therefore no compensation was applied in our experiment. The resulting sea level rise was more explicitly mentioned in several parts of the manuscript.

This makes me wonder if any of the results presented in the manuscript are actually a direct effect of this global freshening. For example, the freshening in Fig. A13b looks suspiciously linear. It might be worth performing a back-of-the-envelope calculation how much the global mean salinity changes due to the hosing and then compare salinity changes against this number.

For the ice-sheet response, we added (to the Discussion section of the revised manuscript) a quick estimate of the impact of temperature vs. salinity changes on basal melting. It shows clearly that we expect the impact of salinity changes to be minor. The decreasing salinity could however be crucial for convection changes which we see in the ocean model. This is discussed in the Discussion section.

5. **Significance testing.** Currently, no significance testing is shown in the figures, but this would be important especially for the first period (years 100-200). Which of these features are significant signals and which ones could as well have arisen due to (low-frequency) internal variability (when compared to the control simulation)?

We tested the significance of the signals in the spatial fields (Figures 3, 5, 6) and marked the regions where the HOSING results deviate significantly from CONTROL with stipples. The new plots have been added to the revised manuscript. This significance testing is based on a bootstrapping method, the details of which are added to the Method section of the revised paper. Indeed there are only a few regions in the first time period, where we can distinguish our signals clearly from the internal variability of the CONTROL.

Specific comments

L16: “first step”: I think that some work in this direction has been done for Greenland, so maybe “an important step” is enough (or add “both hemispheres” somewhere in this sentence).

Thank you, your suggestion “both hemispheres” added to the revised manuscript.

L19-36 presents several contentious issues (has the AMOC already declined during the historical period, how important is GrIs meltwater, what is the risk of 21st-century AMOC tipping) almost as facts, but there is often “another side” of the debate, which should also be referenced.

We agree that the language here could be more nuanced. This section has been re-worked with additional references to Baker *et al.*, 2025, Latif *et al.*, 2022, Terhaar *et al.*, 2025 and Xing *et al.*, 2026. Additionally, as the impacts of an AMOC collapse are expected to be severe, it is worth studying it, no matter how large or small the collapsing probability is. This was clarified in the paragraph.

L38 could also reference the review by Lynch-Stieglitz (2017)

The suggested reference to Lynch-Stieglitz *et al.*, 2017 was added to the revised manuscript.

L51: “open-ocean convection is hardly observed” should be referenced

Bennetts *et.al.*, 2024 was added as a reference for this statement.

L67: Is Berdahl et al. 2024 based on proxy or model data?

We specified this point in the manuscript. It is a model study.

> L159: “pre-industrial atmospheric conditions” – as a caveat that arises from this, it could be mentioned somewhere that impacts of AMOC weakening have been shown to be quite strongly state-dependent (e.g. Bellomo & Mehling 2024).

Yes, it impacts the timing of AMOC weakening, however, our aim here is to simulate consequences of an AMOC shutdown and we therefore are not discussing the transient processes during the AMOC weakening. Consequently we did not add this suggestion to the manuscript.

> L166-180: I see that the spin-up is already documented in Kreuzer et al. (2025) – this could be pointed out more clearly. Could you comment here on how equilibrated the model is at the end of the spin-up? (e.g. TOA imbalance, global ocean heat content trend or similar)

Kreuzer *et al.,* 2025 does not use CM2Mc and thereby does not provide any documentation about the spin-up state. The model state in Kreuzer *et al.,* 2021 is related but not exactly identical. An additional Appendix section has been added to the revised manuscript including model evaluation and the spinup state

> L222: What is the role of salinity for basal melting? For temperature it is intuitive, but less so for salinity. It could be mentioned here (and maybe in other places, see above) that at least some of the salinity decrease appears to come from the (non-compensated) freshwater input.

Yes, this is a major issue that has been raised in both reviews. We made it much clearer in the Discussion and compared the impact of temperature and salinity changes on basal melt by using the equation of state implemented in PICO (Reese *et. al.*, 2018)

In this line we added a sentence to remind the reader of the limitation of non-compensated artificial freshwater forcing. In the Discussion a paragraph which estimates the impact of temperature vs. salinity was added.

> Fig. 2b: Worth repeating the depth range over which the temperature was averaged in the caption

We added your suggestion to the manuscript.

> L239: A reference could be added for the South Atlantic salinification, e.g. Zhu & Liu (2020)

Yes, we added this reference.

> L260: Is this change in convective activity in the Weddell Sea forced or linked to internal variability? Maybe to a mode similar to the well-documented centennial variability in the GFDL model (e.g. Zhang et al. 2019)?

Thank you for this question. As suggested in your last major comment, we added results of a significance test to Figure 3. This shows that changes in convective activity (in this case mixed-layer depth, MLD) in the Weddell Sea mainly lie within the variability of the CONTROL, however in a few cells the convection depth is significantly deeper. Therefore the change could indeed be connected to the centennial variability, however the forcing might change the regularity of the process. A change of this variability process is also suggested by the fact that Antarctic Bottom Water formation in HOSING shows much less variability than CONTROL (see Figure 1b). We added the suggested reference to the manuscript.

> L264 and following suggests that atmospheric temperatures influence SSTs. So, is there an atmospheric mechanism for the surface air temperature cooling? More generally, could you comment in the Discussion about the role/importance of atmospheric mechanisms?

This part was rephrased. We did not want to say that the process is driven by the atmosphere. Our analysis mainly focuses on the oceanic responses, as they are our only drivers of changes in the Antarctic Ice Sheet. Atmospheric processes are expected to be much faster than 600 years and therefore are not direct consequences of the AMOC collapse in this time interval, but rather respond to oceanic changes. However we also added a sentence about this in the Discussion section.

> L271, L278, L288, L305 etc.: You often refer to properties of upwelled water. Could this be more clearly demonstrated by showing Atlantic Ocean/SO sector cross-sections (latitude vs. depth) with isopycnals overlaid?

Thanks for this suggestion. We added a cross-section for each time period to the Appendix. Additionally there will be a video supplement showing the time evolution of temperature and salinity anomalies shown in a latitude vs. depth plot.

> Section 3.3: Could you remind the reader at the beginning of this section why this period was selected for analysis?

Yes, we added one introduction sentence in this section.

> L369: Stocker et al. (2007) compared surface compensation vs. no compensation, but here the relevant difference would be volume compensation (as in the NAHosMIP protocol) vs. no compensation. This should be mentioned somewhere in this paragraph as it likely makes quite a difference.

Yes, you are right. We added a comment on the limitation, that the comparison might be influenced by different hosing methods. The reference comparing different compensation methods (Stocker *et. al.*, 2007) was removed here and added to the Methods. In our model, no volume compensation is possible and we also saw strong implications in the SO when using surface compensation.

> L373: For context, it could be added here how much SLE the Greenland ice sheet holds.

Yes, we added that.

> L391: The recent study by Aguiar et al. (2025), who argued for a role of vertical resolution, could be mentioned here

A sentence about this study was added.

> L404-420: The relevance of this paragraph for the conclusions is not so clear, especially since there are few citations and it is concluded that you "do not expect high impacts due to this limitation". I think that such minor limitations can be covered in the methods section and that this paragraph could be removed to make the discussion a bit less extensive.

Thank you for this comment. We removed this paragraph and merged the important part about the impact of temperature and salinity changes on basal melt with another section in the Discussion.

> L432: "most changes in Antarctica are due to interactions at the ice-ocean interface" could use a reference

A reference to Rignot *et. al.*, 2019 was added.

> L451: It could be mentioned that there are now some models that do couple the AIS in their future projections, e.g. UKESM (Smith et al. 2021).

The reference was added as a model example which could be used to do the experiment again.

> **Technical corrections**
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> L335 and elsewhere: Wunderling 2023 should be 2024
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> L339: warms temperatures -> increases temperatures
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> L367: Jackson & Wood 2018 should be Jackson et al. 2023?

L376: strictly idealized -> highly idealized

L431: no comma

Thank you, all these technical corrections were applied to the manuscript.

Additional remarks:

- Figures 4, A9, A10 have been regenerated after a minor error in the plotting code was corrected. The results remain qualitatively the same.
- Refinements have been made in subsection 3.3 to enhance clarity of the results

References:

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