Answer to Referee #1

General comments

I thank the authors for their detailed responses and for the substantial revisions made to the manuscript. The revised version is significantly improved compared to the previous submission. In particular, I appreciate the additional information provided regarding the background freshwater fluxes in the control simulation, the clarification of the experimental design, and the expanded discussion of the mechanisms involved in the AMOC response. The distinction between the rapid high-latitude adjustment and the later response associated with Labrador Sea convection is now much clearer.
I believe that most of my previous concerns have been satisfactorily addressed. However, a few issues remain and should be clarified before publication.

> We thank the Referee for their time dedicated to review the manuscript again and their new feedback.

Main comment

Interpretation of the freshwater forcing (l. 98–105): The manuscript now provides the total freshwater input applied around Greenland (~0.067 Sv), which is a substantial improvement. However, I still find it difficult to assess how this forcing relates to the observed increase in Greenland Ice Sheet freshwater discharge over recent decades. One of the motivations for freshwater hosing experiments is that many climate models do not fully capture the recent acceleration of Greenland mass loss and the associated freshwater input into the North Atlantic. Therefore, rather than only comparing the imposed flux with historical estimates from Bamber et al. (2018), I encourage the authors to discuss more explicitly how the imposed freshwater anomaly compares with the observed increase in Greenland freshwater discharge over recent decades. Such a discussion would help readers better understand the physical relevance of the chosen forcing and clarify which aspect of the recent increase in Greenland meltwater input this experiment is intended to represent.

> We have found it difficult to discuss our imposed flux in the context of other historical estimations, as they were not directly comparable. For instance, many estimations only reflect the loss of ice mass in Greenland (Mouginot et al., 2019; The IMBIE Team, 2020), which thus misses other important components (e.g. water runoff due to precipitation). We have therefore kept the main discussion around the imposed forcing of 0.0419 Sv, which corresponds to an anomaly in the freshwater flux. The main change is that we now put our fluxes in context with the different values estimated by Bamber et al. (2018) over the last decades. The paragraph now reads as follows:
>
>> [...] This is equivalent to an annual average of 0.0419 Sv or a total of 1322 $km^3\ yr^{-1}$. This quantity is slightly smaller than the idealized 0.05 Sv on annual average used in other similar studies (Martin et al., 2022; Martin and Biastoch, 2023; Wei and Zhang, 2024). The imposed flux is almost 4 times larger than recent observational estimates: The IMBIE Team (2020) gives a peak of 345 ± 66 Gt yr^{-1} of Greenland ice lost in 2011, equivalent to 0.011 ± 0.002 Sv. Taking into account the climatological river runoff and iceberg calving fluxes in the control simulation (0.0047 ± 0.0016 Sv and 0.0205 ± 0.0060 Sv respectively, computed over 53 years), the total annual average freshwater injection around Greenland in the hosing experiments increases up to 0.0671 ± 0.0062 Sv (i.e., 2120 ± 200 $km^3\ yr^{-1}$; see Fig. B1a for associated monthly climatologies). This amount is about 2.3 times the estimations for the total freshwater flux from Bamber et al. (2018) for the 1962-1995 period and about 1.5 times the most recent estimates i for the 2010-2016 period.

It would also help justify the use of the term quasi-realistic and clarify which aspects of the forcing are considered realistic (e.g. spatial distribution, seasonality, pathways) and which remain intentionally idealized (e.g. amplitude).

We have added a clarification at the beginning of the paragraph to justify the use of the term 'quasi-realistic'.

> We leverage the high-resolution of the model to propose a quasi-realistic approach to freshwater hosing. This includes realistic spatial and seasonal distributions, as well as an idealised magnitude that is stronger than the expected discharge under current global warming scenarios. The larger amplitude is intended to increase the signal-to-noise ratio in the response. We apply the freshwater…

Minor comments

l. 8: I suggest replacing "released together with the simulated model river runoff" by wording such as "released on top of the simulated model river runoff". The current wording may suggest that the imposed freshwater flux is part of the model runoff itself rather than an additional forcing.

Changed.

l. 13-14: I remain unconvinced by the statement that meltwater fluxes weaken the AMOC "via thermal wind balance". Thermal wind balance provides a diagnostic relationship linking density gradients and geostrophic shear, but it is not the physical mechanism itself. I recommend rephrasing this sentence in terms of salinity-induced density anomalies and the associated circulation adjustment.

We have replaced “via thermal wind balance” by “due to circulation adjustments forced by the changes in density gradients”. We have also reviewed the other mentions to the thermal wind balance.

l. 18-20: The paragraph describing the AMOC response remains difficult to follow. The text combines a local reduction of ~3 Sv at 60.2°N, an average reduction of 1.3 Sv, a density interval, and a latitude interval without clearly explaining the relationship between these quantities. In addition, the reference to specific density classes appears unnecessarily technical for the abstract. I encourage the authors to simplify this paragraph and clearly distinguish local and basin-scale AMOC metrics.

We have simplified the paragraph to more clearly distinguish between the local AMOC weakening at subpolar latitudes and the basin-wide response. We have replaced the original sentence with the following two sentences:

> After the 21 simulated years, the AMOC has weakened by almost 3 Sv at 60.2° N in density space, which represents a ~20 % reduction of the climatological value in the control, 14.9 Sv. In the context of the North Atlantic, a basin-wide decrease of 1.3 Sv is measured between 10-65° N, which represents about 10 % reduction of the control reference, 13 Sv.

l. 29-30: The sentence beginning with "Previous studies have highlighted important global impacts..." appears incomplete and should be revised.

We have rephrase it as it follows

> “Previous studies with different climate models have highlighted important global climate impacts following a collapse or substantial weakening of the AMOC, characterized by widespread changes in surface temperature and precipitation”.

l. 70-73: The statement that a uniform freshwater forcing "can result in an unrealistic spatial distribution" appears unnecessarily cautious. Such a forcing inherently produces a spatial distribution that is inconsistent with observed Greenland freshwater pathways and seasonality.

We have replaced “can result in” by “results in”.

l. 109-113: The manuscript states that freshwater is injected over depths ranging from less than 1 m to more than 240 m. However, no explanation is provided regarding the origin of this large spatial variability in the vertical distribution of the freshwater input. A brief clarification would be useful.

The distribution is chosen to avoid numerical and physical problems (see Gurvan et al., 2017). Globalwise, the distribution is deeper in the basins with higher water discharge into the ocean (e.g. Amazonas). We have added the following sentence for clarification and “around Greenland”:

> “The runoff is uniformly distributed throughout the upper ocean until a given depth in order to avoid numerical and physical problems (Gurvan et al., 2017). The depth of the distribution depends on the water discharge of the basin; the larger the basin discharge, the deeper the distribution. More specifically, around Greenland, most of the water is added to the surface within the upper 0.94 m, with an average depth at which runoff is injected of 1.78 m, reaching 243.76 m at some coastal points.”

l. 134-136: The manuscript refers to a "residual model drift", but no quantitative information is provided regarding its magnitude. A brief estimate, or a statement explaining why it is negligible compared to the forced signal, would strengthen the methodological description.

The model drift of the global ocean temperatures in control simulation is shown in Fig. 3 of Moreno-Chamarro et al. (2025). Note that for the whole ocean average temperature there is still a drift at the end of the control experiments. During the used control period, almost 50 years after the start of the control, no significant model drifts were found in atmospheric or surface oceanic variables. This was considered to remove the leading years of the control also in Martin-Martinez et al. (2025) (commented in *Experimental setup* Section of that paper). We have added a cite to Moreno-Chamarro et al. (2025):

> “This is intended to account for any residual model drift (see Fig. 3 from Moreno-Chamarro et al., 2025) and align the phase of internal climate variability”.

Answer to Referee #2

The authors have revised their manuscript thoroughly addressing all of the initial reviewer comments. While not all suggested changes made it into the revised version, they provided good reasoning for their decision. Overall the paper has significantly improved and is ready for publication.

I particularly appreciate the water mass transformation analysis and the summary in the new table 2. This is very insightful and provides a better, more quantitative assesses of the hosing impacts. It is interesting to see that the overturning changes focus on the Labrador Sea. As discussed in section 3.4 Time Evolution, the vicinity to the freshwater source may be a good explanation. Alternatively, the limited temporal extent of the simulations may prevent for other regions showing a stronger response (though the subpolar gyre redistribution time scale is on the order of a decade, as pointed out by the authors), or Labrador Sea deep convection is dominating AMOC variability in the coupled model – as discussed by Yeager et al. (2021) for a different but basically similar model.

> We thank the Referee for their time dedicated to review the manuscript again. We have included a reference to Yeager et al. (2021) in the *Conclusions*.