First of all, many thanks for your detailed comments, which will enable us to significantly improve our manuscript.

Our point-by-point responses are detailed below.

The original review comments are shown in red and our responses in **black**.

1. Title: Perhaps including "in HadCM3-M2.1" would help to clarify the scope for readers?

A: We agree with the suggestion and will add "in HadCM3-M2.1" to the revise paper title.

2. 3: >"the effects of AIS meltwater are not considered by most existing coupled climate models": you do have to be careful here, because ice sheet dynamics are not considered by these models, but most if not all of them do permit surface melt which does add additional freshwater under warming (indeed, this can be an infinite source of freshwater in many models).

A: We will rewrite this sentence to clarify that ice-climate feedbacks are not considered by most existing coupled climate models (Swart *et al.*, 2023).

3. 18-25: Between para's 1 and 2 there is an implicit link between sea level rise and meltwater input to the ocean. Just noting that these are not strictly the same thing (because melt of floating iceshelves is a freshwater forcing, that does not influence SLR). Not a huge deal, more of a note.

A: Following this suggestion, we will rephrase these paragraphs to emphasise the importance of non-iceshelf components in contributing to SLR.

4. 40: It is also important to use a consistent design across the community - which you are contributing to (and is a valuable part of the paper) - I note this is mentioned near ln 70

A: We agree that using a consistent experimental design is an important and valuable aspect of this study. To further emphasise this, we will add the following sentence to paragraph ln65-70;

"As mentioned previously, inconsistent experimental design across the existing literature is creating uncertainties over the magnitude of the global climate system's response to AIS meltwater, inhibiting our ability to constrain the climate impacts of AIS meltwater. By drawing upon and contributing to the SOFIA Initiative, this study aims to improve our understanding of the global climate response to AIS meltwater through the use of a common experimental design."

5. 54: I wonder if there is a slightly more constructive way to frame this? Such as "it's unclear whether results from simplified EMICs would hold in fully coupled AOGCMs".

A: We agree with this suggestion, and will rewrite line 54 to; "However, it is unclear whether results from simplified EMICs and ocean-only models would hold in fully coupled AOGCMs, as these models do not fully account for important atmospheric or cryospheric feedbacks …"

6. 108-109 / 119: It would be useful to provide more detail on the converting of the FW flux to a virtual salt flux, since this is presumably the same mechanisms used to implement the SOFIA experiments (not mentioned on 119 but should be).

A: a) We will provide more detail on how freshwater fluxes are represented as virtual salt fluxes in the HadCM3-M2.1 model. The following will be included in section 2.1 Model Description;

"As HadCM3-M2.1 uses a rigid-lid, the ocean surface is not allowed to deform and, as a result, total ocean volume remains constant. As such, any freshwater fluxes simulated in the model do not physically increase total ocean volume, but are instead applied as virtual salinity fluxes that reduce ocean surface salinity. These virtual salinity fluxes effectively remove salt mass from the model ocean, mimicking the dilution effect that would occur with a physical ocean volume increase."

b) Following this, we will clarify that the freshwater fluxes added in "antwater" and "antwater60S" are implemented in same way as detailed above, and conform with experimental designs detailed by the SOFIA initiative.

7. 121: >"although run over a longer time scale (Table S1)" - the referenced experimental design specifies a length of >=100 years. It is good that this work exceeds the minimum, but its seems entirely consistent with the proposed design.

A: We will rewrite ln 121 to clarify that our experiments are consistent with the SOFIA initiative's "antwater60S" and "antwater" experiment designs.

8. 125: Please state what the magnitude of this climatological flux is. In most similar models, this flux is roughly specified to balance P-E over Antarctica. If that is true, it is not a small flux.

A: We will include the magnitude of the base climatological flux included in all experiments.

9. 143: As written, this does not seem to quite make sense to me. A positive salinity trend cannot be due to (positive down) freshwater flux. A positive salinity trend might indicate that the piControl freshwater flux is smaller than P-E over Antarctica, and hence, P-E-R flowing over the ocean is less than 0 (i.e. effectively a negative freshwater flux). That is assuming everything else in the model is conserving of freshwater, which is not clear.

A: We will rewrite ln 141-143 to provide further clarity over the role of snow build-up in creating the positive salinity trend simulated in the "piControl" experiment. We will explain how the background flux applied in the HadCM3-M2.1 model is not quite enough to completely balance the build-up of snow on ice sheets simulated in the model, as the background flux is a fixed field, rather than being prognostically calculated (Valdes *et al.*, 2017). Alongside a more detailed explanation of the virtual salinity flux detailed in Comment #6, these changes should provide clarity over the positive salinity trend simulated by the model.

10. Fig 3a/b and timeseries plots in general: The annual scale of variability is not really discussed in this manuscript, and it is very noisy. I think the results would be clearer is the annual mean lines were de-emphasized, and the 10 yr running averages were made more prominent.

A: We agree that annual variability is difficult to read, and so we will de-emphasise the annual mean lines and emphasise the 10-year running averages in all timeseries plots.

11. 170 and surrounding. >"in line with observations from the same time period shown here". I find using the term "observations" confusing (with actual real world data). Also, the magnitude of freshening is compared with other studies, but it is not mentioned how the rates of forcing match or do not. If the freshening is similar, but the forcing is very different, this does not indicate a consistency. Please clarify.

A: a) Following your suggestions, we will replace the term "observations" with "simulations". b) We will state that the experiment run by Li *et al.*, (2023) added 0.08 Sv of AIS meltwater for 50 years under a RCP8.5 scenario. As such, we will clarify that whilst our experiments use a similar freshwater forcings, the Li *et al.*, (2023) study is likely influenced by additional forcings.

12. 179: Is that the Labrador Sea? The map is small, but it looks more like Baffin Bay or Davis Straight to me.

A: We agree with your suggestion, and will correct ln 179 to say "Baffin Bay".

13. 193: Global sea-ice thickness is not really such a useful metric, given the massive climatogical thickness differences across the hemispheres. Perhaps reporting as a change per NH/SH, and perhaps even a relative (%) change would be clearer?

A: We agree with this suggestion, and we will report changes in sea ice thickness in the NH and SH in addition to global sea-ice thickness.

14. Figure 6 c-h: I think it would be more informative to saturate the high end of the colorbar, so that the broader pattern of thickness increase is visible. As it stands, we only see what happens near the edges. This is subjective, but I feel like we are missing the broader picture.

A: Following your suggestion, we will adjust the colour bars of Figures 6 c-h.

15. 235: What is the climatological AMOC rate in this model (in piControl)?

A: We will report the climatological AMOC rate in "piControl" in the revised paper.

16. 230-260: It might be worth testing whether the AMOC/ACC timeseries are statistically different. As you say, there is a lot of variability.

A: We agree with this suggestion, and will conduct two-way student t-tests on the AMOC, AABW and ACC timeseries data to determine statistical significance at the level 95%.

17. Figure 12: The changes in mid latitude precip are lost in the large changes near the equator. If you present the precip change as relative to the piControl (% as opposed to mm/day), it will take care of the climatological differences across latitudes, and perhaps be more meaningful (again, subjective).

A: Following your suggestion, we agree that mid latitude precipitation is lost in the large changes near the equator. To resolve this, we will adjust the colour bars of Figure 12 b-d to more clearly show climatological differences across latitudes.

18. 290: this explanation does not make physical sense to me, or I'm missing something. If injecting water around the coast makes it "able to spread horizontally across the surface of the SO before diffusing into the ocean depths", then injecting it even south of 60S would surely enable even more spread. Might a possible explanation might be that injection directly around Antarctica in antwater influences SO deep convection more directly than antwater60S, leading to more pronounced salinification at depth. I know Fig 18 shows similar AABW responses, but deep convection changes can be quite different to AABW (e.g. Chen *et al.*, 2023)

A: We agree with the explanation that you have provided here, and would like to thank you for this helpful contribution to our paper. Having revisited our work, we now argue that restricting AIS meltwater to the coastline creates a freshwater cap at the ocean surface, preventing convection and inhibiting deep water formation. This directly inhibits mixing, resulting in more pronounced salinification at depth. To confirm this, we will edit Figure 9 to also show a map of annual mean mixed layer depth (MLD) from "antwater", to assess whether a change in horizontal distribution results in a change in the rate of downwelling around the AIS coastline.

19. 295: Could this just variability, even in a 50 year mean?

A: We argue that the results from the two-way student t-test indicate that the anomalous warming simulated in the northern parts of the Arctic Ocean is statistically significant, and therefore unlikely to be a result of variability.

20. 314: This "significant impact" statement seems to contradict the earlier results that AMOC changes are very small, and even AABW changes are not huge (but we do not know the relative changes). I would contend that the stratification mechanism that you mention next is likely more important.

A: We will first conduct statistical significance tests on the AMOC and AABW timeseries, and will amend ln 314 depending on our findings. If our results indicate that AMOC and AABW changes are in fact statistically significant, this will be reflected in our conclusions.

21. 319: Sea-ice changes vs SAT changes are a bit chicken and egg. How much surface cooling could be explained by the ice-albedo feedback alone?

A: We agree with this comment, and we will rewrite ln 319 to reflect the dynamic relationship between sea ice change and SAT changes; "An expansion and thickening of sea ice is likely both a response to and cause of cooler surface temperatures and enhanced surface freshening, with the ice-albedo feedback further contributing to cooler SATs".

22. 359: DATA availability: The authors are describing the results of running a standardizes experimental protocol in their model. The SOFIA project has a data archive and active working group, but it would seem this data has not been contributed to that open data archive. It would be added value to contribute that data:https://sofiamip.github.io/data-access.html

A: We are happy to share our data with the SOFIA project, and will contribute to SOFIA's open data archive.

References:

Chen, J.-J. *et al.* (2023) "Reduced Deep Convection and Bottom Water Formation Due To Antarctic Meltwater in a Multi-Model Ensemble," *Geophysical Research Letters*, 50(24), p. e2023GL106492. Available at: https://doi.org/10.1029/2023GL106492.

Li, Q. *et al.* (2023) "Abyssal ocean overturning slowdown and warming driven by Antarctic meltwater," *Nature*, 615(7954), pp. 841–847. Available at: https://doi.org/10.1038/s41586-023-05762-w.

Swart, N. *et al.* (2023) "The Southern Ocean Freshwater release model experiments Initiative (SOFIA): Scientific objectives and experimental design," *EGUsphere*, 2023, pp. 1–30. Available at: https://doi.org/10.5194/egusphere-2023-198.

Valdes, P.J. *et al.* (2017) "The BRIDGE HadCM3 family of climate models: HadCM3@Bristol v1.0," *Geoscientific Model Development*, 10(10), pp. 3715–3743. Available at: https://doi.org/10.5194/gmd-10-3715-2017.