

Response to Reviewer #1

This manuscript investigates the impact of enhanced meltwater associated with dynamic mass loss from the Antarctic ice sheet on Antarctic sea ice. The authors assess the response to a 0.1 Sv meltwater input applied uniformly at the ocean surface, using a new suite of simulations from 11 models comprising 43 ensemble members. The results show that, although the imposed freshwater forcing is substantially larger than the present-day observed increase in ice sheet and ice shelf meltwater, the simulated sea ice response exhibits a wide inter-model spread. This spread is influenced by differences in model mean-state sea ice area and volume, the prevalence of open-ocean deep convection, and the background ocean stratification. Overall, this is a well-written and interesting study that provides useful insights into the role of Antarctic meltwater in shaping sea ice variability. But several aspects need clarification before publication.

We thank the reviewer for their thoughtful and helpful comments on the manuscript. We provide a point-by-point response below. The reviewer comments are in black, our response is in blue, and quoted text is in italics.

Major:

Line 176-181. The reduction in sea ice concentration is primarily attributed to open-ocean deep convection. It may be helpful to further discuss whether the response of near-surface atmospheric circulation to the freshwater forcing could also contribute to the spatial pattern of sea ice concentration anomalies.

We thank the reviewer for this helpful suggestion. We agree that the sea-ice response will also be affected by atmospheric processes and feedbacks. In these Tier 1 antwater simulations there is no forcing due to anthropogenic GHG forcing, so any atmospheric influence can only be through feedbacks from meltwater-induced changes. While our analysis indicates that changes in upper-ocean stratification and open-ocean deep convection are a leading control on the magnitude and some aspects of the spatial pattern of the sea-ice concentration response, the associated changes in SST, sea ice, and air–sea fluxes can also drive a near-surface atmospheric circulation response. This atmospheric adjustment may in turn affect sea ice further through changes in surface winds, sea-ice drift, Ekman transport, heat and moisture advection, and turbulent heat exchange.

We have revised the discussion to make this point clearer and to cite the recent literature highlighted by the reviewer. In particular, Beadling et al. (2024) and Xu et al. (2025)—with the latter based on the same set of simulations used here—show that Antarctic freshwater forcing can produce a robust atmospheric response over the Southern Ocean, while Zhu et al. (2026) show that, when more realistic spatially distributed basal melt forcing is used, atmosphere–ocean–sea-ice interactions can make an important contribution to the sea-ice response. These results support the interpretation that atmospheric feedbacks may modulate the regional pattern of sea ice concentration anomalies in our simulations. However, because the SOFIA Tier 1

protocol uses an idealized, spatially uniform coastal surface freshwater perturbation and our experiments do not include atmosphere-fixed or wind-fixed sensitivity experiments, we cannot quantitatively separate the atmospheric contribution from the direct oceanic response in the present study. We therefore now state this as an important caveat and identify it as a motivation for future SOFIA experiments using spatially distributed and/or depth-dependent freshwater forcing.

We have added the following text to the discussion on lines 289-310 on this point:

“Although we interpret much of the sea-ice response as arising from the direct oceanic effect of freshwater forcing on upper-ocean stratification, and open-ocean deep convection, the fully coupled nature of these simulations means that atmospheric feedbacks may also contribute to the sea ice response. Freshwater input modifies SST and sea-ice cover, thereby altering the surface albedo of and turbulent heat fluxes over the Southern Ocean, and these surface changes can drive a near-surface atmospheric circulation response. Such circulation anomalies may influence sea ice dynamically, through changes in surface winds driving sea ice transport, and thermodynamically, through changes in near-surface temperature, moisture advection, and air–sea heat exchange. Recent analysis of the atmospheric response in the multi-model ensemble used here (Xu et al., 2025) and similar idealized freshwater-forcing experiments (Beadling et al., 2024) show that Antarctic meltwater can generate a large-scale atmospheric response over the Southern Ocean and beyond. Therefore, while changes in ocean deep convection and stratification provide a physically consistent explanation for much of the simulated response, atmospheric circulation changes may help shape the regional structure of the sea ice concentration anomalies, particularly near the ice edge and in sectors where the sea-ice response is less directly tied to regions of open-ocean convection.

The role of atmospheric feedbacks is particularly relevant when comparing the present Tier 1 experiments with studies that use more realistic freshwater forcing. Zhu et al. (2026), for example, applied spatially distributed Antarctic ice-shelf basal melt forcing for 2006–2016 and found that the coupled atmospheric response can significantly affect the sea-ice response. Their results suggest that the spatial distribution and vertical placement of freshwater forcing can influence not only the local ocean stratification response, but also the atmospheric circulation anomalies that feed back onto sea ice. The idealized protocol used here is therefore best interpreted as a controlled multi-model test of the sensitivity of Antarctic sea ice to a consistent freshwater perturbation, rather than as a complete representation of the regional sea-ice response to realistic Antarctic ice-shelf basal melting. Quantifying the relative roles of ocean stratification, deep convection, and atmospheric feedbacks will require targeted diagnostics, and ideally sensitivity experiments with spatially distributed and depth-dependent freshwater forcing that will be conducted as part of Tier 3 of the SOFIA protocol, discussed further below.”

Line 195–197. In CanESM5 and GFDL-ESM4, regions of increased salinity appear to coincide with regions of increased sea ice concentration. However, in EC-Earth3 and FOCI, sea ice decreases at the north and northeast of the Weddell Sea, while salinity

increases in these regions. This suggests that the salinity increase may not directly linked to sea ice formation. It would be helpful to clarify this inconsistency by additional analysis or discussion of whether the sea ice reduction is driven by atmospheric or ocean dynamical processes.

We thank the reviewer for this comment. The regions of decreased sea ice (and increased surface salinity) in the Weddell Sea in EC-Earth3 and FOCI are associated with regions of open-ocean deep convection in that region in those models. The deep mixing in these regions brings relatively warm, salty water to the ocean surface from depth, resulting in the decreased sea ice and increased salinity. The increased sea surface salinity in CanESM5 and GFDL-ESM4, on the other hand, we attribute to increased sea ice formation and/or reduced sea ice melt in those regions resulting in increased salinity due to greater brine rejection and/or reduced input of relatively fresh sea ice meltwater. The text has been modified on lines 208-211 to read:

“There is general surface freshening overall, with some models showing regions of increased salinity due to regions of open-ocean deep convection bringing relatively warm, salty water to the surface (EC-Earth3 and FOCI), or increased brine rejection from sea ice formation or decreased freshening due to reduced sea ice melt (CanESM5 and GFDL-ESM4).”

Minor :

1. Line 93-94. Is the analysis for both the piControl and the antwater experiments based on the last 30 years of the simulations? Also, for the time series analyses of sea ice area and volume, are the changes in the antwater experiment calculated relative to the corresponding years of the piControl simulation, or relative to a 100-year mean? In general, it is more common to use a longer-term mean of the piControl simulation (e.g., Chen et al., 2023; Beadling et al., 2024; Xu et al., 2025), as this can help reduce the influence of inter-annual and decadal variability and improve the robustness of the response. While this choice is unlikely to substantially affect the main results, but it needs a proper explanation of the choice.

We thank the reviewer for this comment. The final 30 years of both the piControl and antwater simulations were used in the analysis, and this has been clarified in the Methods section on lines 97-100 as follows:

“The time averages of the response are always based on the last 30 years of monthly-mean output of the 100-year-long experiment and the overlapping period of the piControl run, if not stated otherwise. This averaging period was chosen to be long enough to reduce the influence of the initial adjustment to the forcing and internal variability, yielding a quasi-equilibrated upper Southern Ocean.”

We deliberately chose to use overlapping periods from the piControl rather than a long-term average in order to make best use of the ensemble member branching method outlined in the SOFIA protocol (Swart et al., 2023). In the SOFIA protocol the ensemble members are branched from different years of the piControl run, rather than by using small perturbations from a single year, to sample across low-frequency ocean

variability in the piControl. Using a long-term average as our baseline would reduce the impact of this branching choice. We have clarified this in the Methods section on lines 100-102 as follows:

“We choose to use overlapping periods from the piControl run for each ensemble member as the baseline, rather than a long-term average, to maintain the sampling across low-frequency ocean variability as outlined in the SOFIA protocol.”

2. Line 131. It is difficult to identify the period used in this study (years 50–70) in Fig. 5 of Beadling et al.. Please clarify how the years analyzed here correspond to those shown in Fig. 5 of that study.

We thank the reviewer for noting this. The 100 years in the SOFIA simulation correspond to years 101-200 in Beadling et al. (2022). This has been clarified in the text in the revised manuscript on lines 140-142:

“The large polynya in the piControl simulation was previously noted in Beadling et al. (2022), with the 100 years of the antwater simulation corresponding to years 101–200 in the piControl shown in their Figure 5.”

3. Line 170-171. According to Fig. 1b–c and 3b–c, it is difficult to directly compare ensemble spread and model spread. It would be helpful to quantify these spread (e.g., using standard deviation) and present them in a table. In addition, I suggest adding the multi-model mean to Fig. B1 and B2 to facilitate comparison with Fig. 1 and 3.

We thank the reviewer for this comment, and note that we have quantified the spread in the sea ice area and volume responses for each model individually and the multi-model ensemble in Table 2. We have also added the multi-model mean to Figures B1 and B2 in the revised manuscript as suggested.

4. Line 241-242. The statement that “models with the thinnest mean-state sea ice in their piControl runs have the largest percentage changes” may not apply in all cases; for example, EC-Earth3 appears to be an exception. It may be helpful to either qualify this statement or briefly discuss such deviations.

We thank the reviewer for this comment, and agree that this statement is not true for all models. The sentence on lines 255-260 has been modified to read:

“Furthermore, models with low sea ice volume in their piControl runs typically have the largest changes in sea ice area (see Figure 10d, aside from CanESM5 and EC-Earth3). This indicates that models with lower mean-state sea ice volume are generally more sensitive to surface cooling driven by the addition of the meltwater and are able to grow a more extensive ice cover because of the reduced insulating effect of the existing sea ice. The fronts associated with the ACC form a natural barrier for northward sea ice expansion (Goosse et al., 2025) limiting the response amplitude in models with larger mean-state sea-ice cover.”

5. Line 268-270.

“Furthermore, in the EC-Earth3, FOCI, and HadGEM3-GC31-LL models, regions of deep convection form near the end of the antwater simulation, explaining the reduction in sea ice area observed in those models.

” However, based on

Fig. 1, there does not appear to be a clear decreasing trend in sea ice area during the final 30 years of the antwater simulation in these models. A clearer explanation of this point would be helpful. The correspondence between reduced sea ice and enhanced deep convection seems most evident in EC-Earth3 and FOCI, particularly in the northern and northeastern Weddell Sea.

We thank the reviewer for this comment, and agree that this summary does not reflect the results for these three models. The sentence has been modified on lines 283-285 to read:

“Furthermore, in the EC-Earth3, FOCI, and HadGEM3-GC31-LL models, open ocean deep convection forms near the end of the antwater simulation, explaining the regions of reduced sea ice area concentration in some ensemble members observed in those models.”

6. Line 273-278. Based on aforementioned analysis, the relatively weak response in AWI-ESM-1-REcoM may be related to its use of a fixed freezing point and relatively strong stratification. It may be helpful to briefly conclude this interpretation.

We thank the reviewer for this note, after further investigation we have realised an error was made in identifying the freezing point equation in AWI-ESM-1-REcoM, and the relationship used is in fact non-linear. As such, the note about the constant freezing point has been removed from the revised manuscript.

7. Line 286-289. A recent study (Zhu et al., 2026) using spatially distributed freshwater forcing suggest that the atmospheric response associated with Southern Ocean air–sea interactions can also have a significant impact on sea ice. This aspect may deserve some discussion.

We thank the reviewer for this comment and refer to our response to major comment #1 where we address this.

Reference:

Beadling, R. L., Lin, P., Krasting, J., Ellinger, W., Coomans, A., Milward, J., et al. (2024). From the surface to the stratosphere: Large-scale atmospheric response to Antarctic meltwater. *Geophysical Research Letters*, 51, e2024GL110157. <https://doi.org/10.1029/2024GL110157>

Chen, J.-J., Swart, N. C., Beadling, R., Cheng, X., Hattermann, T., Jüling, A., et al. (2023). Reduced deep convection and bottom water formation due to Antarctic meltwater in a multi-model ensemble. *Geophysical Research Letters*, 50, e2023GL106492. <https://doi.org/10.1029/2023GL106492>

Xu, X., Martin, T., Beadling, R. L., Liu, J., Bischof, S., Hattermann, T., et al. (2025). Robustness and mechanisms of the atmospheric response over the Southern Ocean to idealized freshwater input around Antarctica. *Geophysical Research Letters*, 52, e2024GL113734. <https://doi.org/10.1029/2024GL113734>

Zhu, Z., Liu, J., Liu, Y., Martin, T., Song, M., Yang, C.-Y., et al. (2026). Implications of realistic Antarctic ice shelf basal melting during 2006–2016 on Southern Ocean climate. *Geophysical Research Letters*, 53, e2025GL119237. <https://doi.org/10.1029/2025GL119237>

Grammar:Line 68: Please delete one “evenly”

Corrected

Response to reviewer comment #2

Pauling et al. present the results of the Tier 1 SOFIA experiment: a multi-model ensemble simulating how Antarctic sea ice responds to spatially uniform meltwater flux from ice discharge. They find that the models exhibit a wide spread in the sea ice response, which can be attributed to mean-state sea ice area and volume, deep ocean convection, and mean-state ocean stratification. Overall, this is a well-written and scientifically valuable manuscript that fills a notable methodological gap. The limitations in the methodology are also appropriately qualified. My comments are primarily minor, and mostly pertain to presentation as opposed to methodology.

We thank the reviewer for their thoughtful and helpful comments on the manuscript. We provide a point-by-point response below. The reviewer comments are in black, our response is in blue, and any quoted text is in italics.

General comments:

The authors should discuss the influence of spatial resolution on their results. As shown in Table 1, the different models used in SOFIA have varying spatial resolutions. The authors allude to the influence of spatial resolution in lines 275-278, where they discuss the AWI-ESM-1-REcoM model and its high resolution near the coast. Since the meltwater experiment is dependent on specifically the grid cells adjacent to the coastline being perturbed by meltwater flux, I would think that differences in coastal grid cell resolution could have a significant influence on the results (e.g. finer resolution of continental shelf bathymetry and mesoscale circulation). The authors can also utilize existing literature on this topic (e.g. Rackow et al., 2022 (<https://doi.org/10.1038/s41467-022-28259-y>), who find a delayed sea ice response in higher-resolution CMIP5 simulations).

We thank the reviewer for this comment, and have added the following discussion of model resolution to the revised manuscript on lines 318-329:

“The models used in this study vary substantially in the horizontal resolution of the atmosphere, ocean and sea-ice model components (Table 1). Model resolution has previously been shown to impact both sea ice and the ocean response to Antarctic meltwater. Rackow et al. (2022) showed that the response of Antarctic sea ice to future warming projections is delayed in models with higher-resolution ocean and sea ice components. They attribute this to resolved ocean eddies that increase equatorward heat transport and moderates warming around Antarctica. The study of Beadling et al. (2022) employed the same GFDL-CM4 and GFDL-ESM4 models used in this study and showed

that the GFDL-CM4 model, with its higher horizontal resolution of $\sim 0.25^\circ$, better resolves the Antarctic Slope Current which effectively traps the meltwater on the continental shelf, while in the lower-resolution GFDL-ESM4 ($\sim 0.5^\circ$) more meltwater is able to escape to the open ocean. The model with the highest resolution ocean-sea ice components used here are, in order, AWI-ESM-1-REcoM, GFDL-CM4, GFDL-ESM4, and FOCI, with the rest all at approximately 1° resolution. There is no clear resolution dependence of the overall sea ice area or volume response, with these four higher-resolution models spanning the full range of responses (Figures 1 and 3). Nevertheless, as shown in Beadling et al. (2022), model resolution is important for smaller-scale continental shelf processes.”

The usage of “SI” to mean stratification index in a paper about sea ice (and where SIA and SIV are used as acronyms for sea ice-based metrics) is extremely confusing (e.g. in Fig. 10), I’d recommend changing that acronym to something more differentiated.

We thank the reviewer for noting this, and we have replaced “SI” throughout the revised manuscript with I_{strat} to avoid the ambiguity.

Specific comments:

Table 1: The resolution column is confusingly formatted. When only one number is written, does this mean that the latitude and longitude spacing are on the same resolution? It would be better to explicitly write out all four numbers (both lat and lon for both ocean and atmosphere, even if they are the same), to avoid this confusion.

We thank the reviewer for this comment and we have made the suggested change in the table in the revised manuscript.

77-86: The specifics detailing the experimental design of SOFIA seems better suited to the “Data and Methods” section, rather than the Introduction. Instead, the Introduction may benefit from some more general contextualization of the SOFIA initiative.

We thank the reviewer for this suggestion and agree that the implementation details are better placed in the Data and Methods section. We have shortened the final paragraph of the introduction on lines 77-81 to read:

“In this study, we present results from the “Tier 1” antwater SOFIA experiment, where a constant meltwater perturbation is evenly distributed at the ocean surface across all grid cells immediately adjacent to the Antarctic coastline, under pre-industrial climate conditions. Comparing the response of Antarctic sea ice to this meltwater perturbation

across the 11 participating models allows us to investigate the factors contributing to the inter-model spread in the response of Antarctic sea ice and the Southern Ocean to meltwater from Antarctic ice sheet mass loss.”

We have also moved the SOFIA implementation details to the beginning of the Data and Methods section on lines 83-90, which now reads:

“The Tier 1 antwater experiment defined in the SOFIA protocol prescribes a meltwater flux of 0.1 Sv (3154 Gt yr^{-1}) evenly distributed at the ocean surface across all grid cells immediately adjacent to the Antarctic coast. Although this amount of meltwater is larger than contemporary observational estimates of ice shelf basal melt rates (0.035-0.050 Sv, Adusumilli et al., 2020; Davison et al., 2023) or Antarctica's current mass imbalance (0.006-0.016 Sv, Slater et al., 2021), the antwater experiment was designed to produce a robust signal for the purposes of model intercomparison, not to match observations over the historical period. This magnitude of meltwater input is comparable to the projected meltwater flux in the mid-to-late 21st century (e.g., Swart et al., 2023). The idealized nature of the antwater experiment allows us to examine the response to an increase in Antarctic meltwater without the additional interaction with anthropogenic greenhouse gas or aerosol forcing.”

91: Specify that piControl is a pre-industrial control run, and briefly describe the experiment and why you choose it, for glaciology-oriented readers less familiar with the CMIP world.

We thank the reviewer for this suggestion, and in the revised manuscript on lines 93-95 this now reads:

“For SOFIA, the antwater experiments were branched from each model's CMIP6 piControl (pre-industrial control simulation). The SOFIA protocol requested that the ensembles be generated by branching each member from a different year in the model's piControl run, where the years are chosen to have different phasing of low-frequency ocean variability.”

93: Is there a motivation behind the particular choice of the last 30 years? Is this selected because the model with the largest τ value has $\tau = 69.2$, and 70 is the smallest number greater than that—so are you trying to select the greatest number of years that have all models in an equilibrium state? Whether that's the reason or it's something different, you should specify why you choose 30 years.

We thank the reviewer for this comment, and we have added the following text on lines 98-100 to the revised manuscript:

“This averaging period was chosen to be long enough to reduce the influence of the initial adjustment to the forcing and internal variability, yielding a quasi-equilibrated upper Southern Ocean.”

157: “Changes in sea ice volume are important for ocean–atmosphere fluxes of heat, moisture, and carbon as well as biology in this region”; add some representative citations here.

We thank the reviewer for this suggestion. The revised sentence on lines 169-170 now reads:

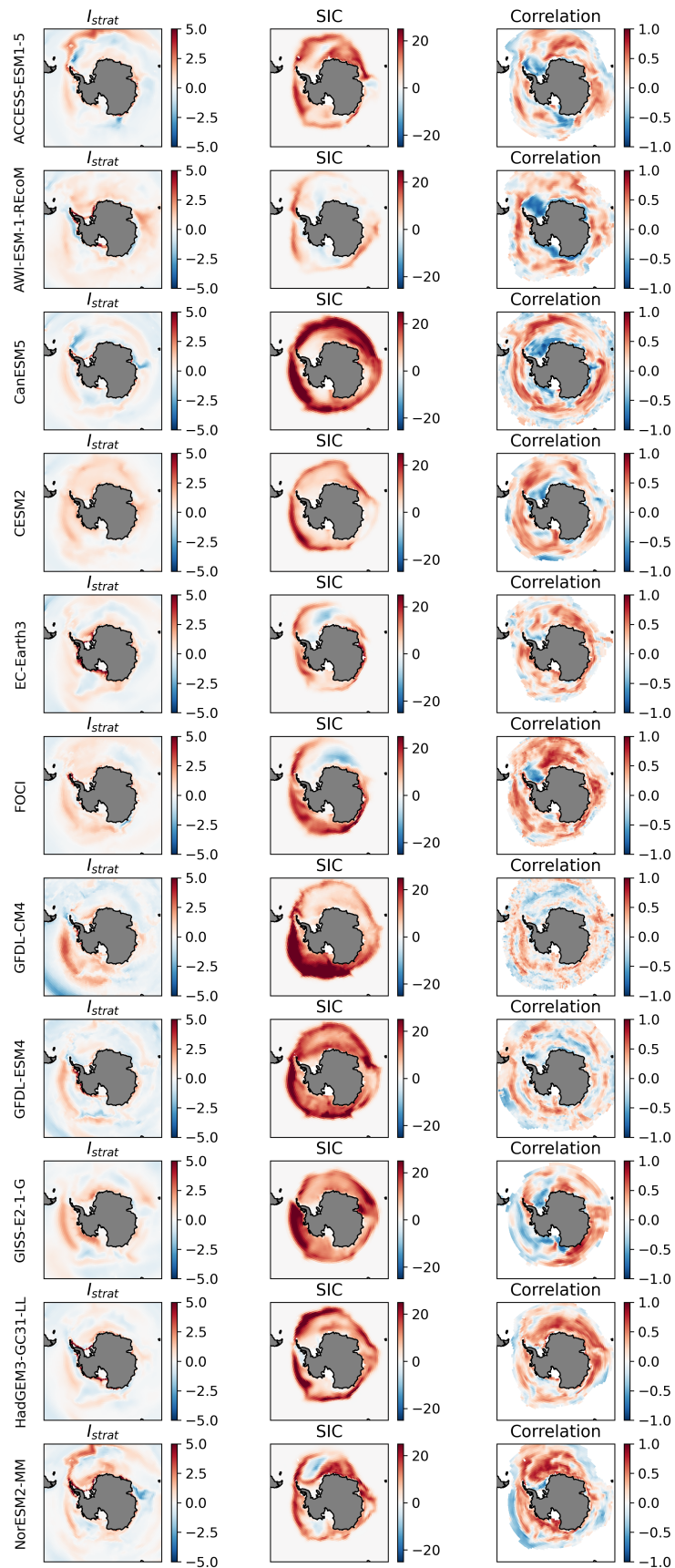
“Changes in sea ice volume are important for ocean–atmosphere fluxes of heat, moisture, and carbon as well as biology in this region (e.g., Massom and Stammerjohn, 2010; Gupta et al., 2020; Swadling et al., 2023)”

Fig. 10: In the caption, when you describe that the y-axis is “ensemble-mean Antarctic sea ice response,” clarify that this is the area response and not the volume response. Also, the legend overlaps with a datapoint in Fig. 10d.

We thank the reviewer for catching this. In the revised manuscript we have updated the figure caption to explicitly say sea ice area response, adjusted the legend, and updated the axis labels to use the new notation for the stratification index.

Fig. 10b: I’d be curious to see what this figure looks like as a correlation map, where you plot the correlation between ΔSIA and ΔSI for each grid cell. Are there geographic areas where this relationship better holds than others, and if so: does that provide you with any physical insight?

We thank the reviewer for this suggestion and include the figure showing the change in stratification index, the change in sea ice concentration, and the correlation map between the two over the last 30 years of each simulation below. We see that the stratification index and sea ice concentration changes are in general positively correlated over most regions of sea ice, with little consistent spatial pattern between models. Since no consistent spatial pattern emerges from this analysis we do not include it in the revised manuscript.



241-242: I'm unsure where the support for this statement is coming from in the paper. Fig. 10d doesn't show any significant trend between piControl sea ice volume and the change in sea ice area. Is it that the trend becomes significant when it's reported in percentage units, as opposed to km²? If so, make that clear, and provide a plot and/or quantitative support in the text. Also, is this statement assuming that thinness is interchangeable with sea ice volume?

We thank the reviewer for noting this. While there is no significant trend in figure 10d, if the outliers of CanESM5 and EC-Earth3 are removed there is a general relationship between mean-state sea ice volume and sea ice area response. We agree that we should say “smallest sea ice volume” rather than “thinnest” here. The revised sentence on lines 255-258 now reads:

“Furthermore, models with small sea ice volume in their piControl runs typically have the largest changes in sea ice area (see Figure 10d, aside from CanESM5 and EC-Earth3). This indicates that models with smaller mean-state sea ice volume are generally more sensitive to surface cooling driven by the addition of the meltwater and are able to grow more ice because of the reduced insulating effect of the existing sea ice.”

248: Fig. 4 does appear to show that there is a varying response between different models. Some models show a much more spatially homogeneous response (as a function of longitude) than others. You should mention this wide spread between the models. Which of these models are “consistent” with the simulations of Ashley et al. 2021? And are you able to attribute the differing behavior to different model representations of ocean circulation?

252: You refer to “sea ice volume changes shown here” but you are referencing Fig. 4, which is a figure of sea ice concentration. Shouldn't you be referencing figure 5?

We thank the reviewer for noting this and respond to both of the above two comments here. The comparison with Ashley et al. (2021) was indeed intended to refer to the sea ice volume response and so we have updated this paragraph on lines 262-270 to read:

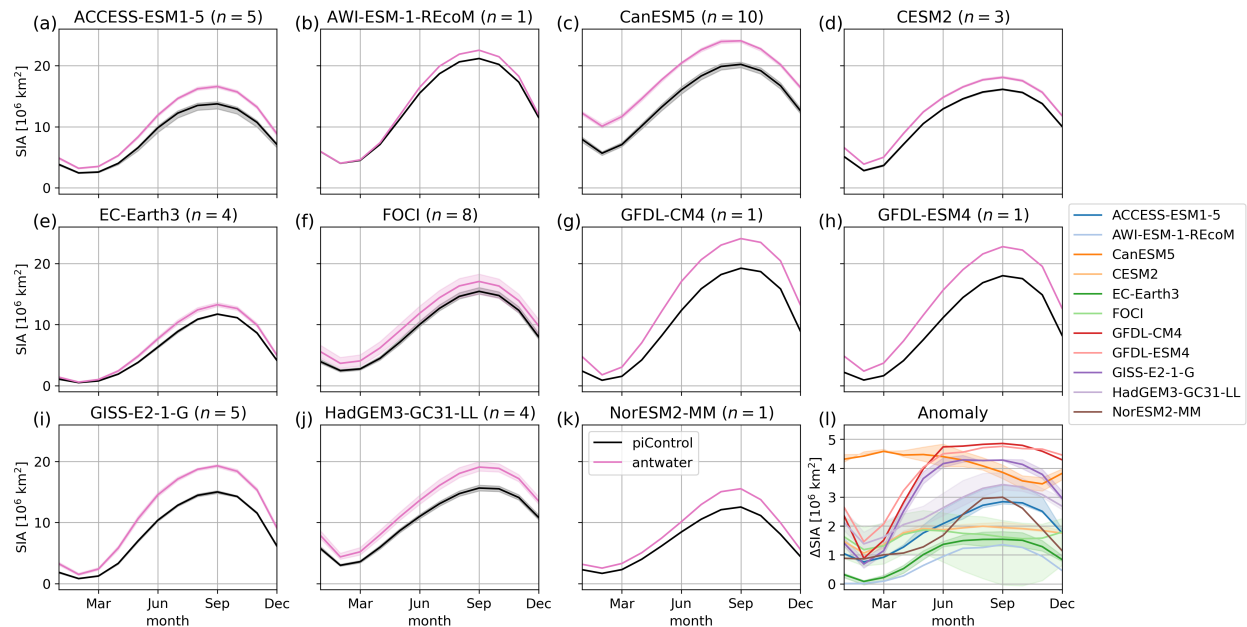
“While the spatial response of sea ice concentration and volume (Figures 4 and 5) varies widely across models, some features are consistent with ocean model simulations of

meltwater release (see supporting information of Ashley et al. 2021) showing that meltwater released as far away from the Peninsula as the Ross Ice Shelf front follows the Antarctic coastal and slope currents anticlockwise around the continent, before separating at the tip of the Antarctic Peninsula. Some of the meltwater continues around the Antarctic coast, with the remainder flowing into the Weddell Gyre and out into the Atlantic Ocean, in a pattern similar to the sea ice volume changes in several models shown here.”

256: I'm not sure this is immediately obvious from visual inspection of Fig. 2. Why not make a plot showing the derivative of this plot (i.e. the change in SIA with respect to time)? In lines 152-154, you should also quantify the difference between the magnitudes of the September and February increase, which can support this assertion.

We thank the reviewer for this suggestion and have added a panel with the difference between antwater and the piControl for each model to Figure 2 for each model (see below). For all models except CanESM5, the smallest change is in February, and the largest is around September. The average difference between the minimum and maximum monthly anomaly is 2.06 million km². We have also added a sentence to the results section on lines 163-167 so that the relevant section reads:

“In most models, the increase in sea ice area is largest around the time of the annual sea ice area maximum in September, with a much smaller increase around the time of the annual minimum in February (Figure 2l). The multi-model mean difference between the minimum and maximum monthly anomaly is 2.06×10^6 km². However, in CanESM5 and FOCI the increase in the sea ice area is approximately the same year-round. Particularly the FOCI ensemble (green shading in Figure 2l) demonstrates the limiting effect of open ocean deep convection, which is present in some of its members, on the winter sea ice response.”



Revised Figure 2

280-283: Can you provide numbers quantifying these statements? E.g. what is the spatial variability in meltwater flux, and how much of the meltwater flux actually enters at depth instead of the surface?

We have added the following sentence to the revised manuscript on lines 333-336 to quantify the contribution of the Amundsen/Bellingshausen sea sector:

“For example Kim et al. (2024) showed that over the period 2003-2020, the Amundsen/Bellingshausen sea sector lost $173.9 \pm 6.9 \text{ Gt yr}^{-1}$, while the Antarctic continent overall lost $122 \pm 22 \text{ Gt yr}^{-1}$ (the overall loss is smaller than the Amundsen/Bellingshausen Sea loss due to compensating regions of mass gain).”

For the partitioning between meltwater input at the surface vs at depth we have modified this section on lines 336-340 to read:

“Meltwater also only enters the ocean surface in antwater, whereas in reality it enters the ocean through a combination of basal melting of ice shelves, where the meltwater enters the ocean at depth, and calving and subsequent melting of icebergs, where the meltwater enters the ocean far from the coastline. For example, Davison et al. (2023) found that, of the Antarctic ice shelves that lost mass over the period 1997-2021, 68% of that melt entered the ocean at depth through basal melting, with the remainder entering at the surface through melting of calved icebergs.”

290-291: This would benefit from some more context about the SOFIA protocol. Particularly as it pertains to future work building on this paper, you should briefly describe the design of the Tier 2 and Tier 3 SOFIA experiments.

We thank the reviewer for this suggestion, and we have added additional discussion of the SOFIA protocol and the additional experiment tiers to the revised manuscript on lines 345-352 as follows:

“Future SOFIA experiments will provide an opportunity to further understand the mechanisms linking Antarctic meltwater and sea ice. The SOFIA protocol outlines additional “tiers” of meltwater experiments designed to improve our understanding of the impact of Antarctic mass imbalance in climate models (Swart et al., 2023). Tier 2 consists of experiments with estimates of historical meltwater forcing, and projections of future meltwater input under different emissions scenarios. Tier 3 consists of sensitivity tests to factors such as the spatial distribution and the inclusion of the latent heat effect. At the time of writing, the running and analysis of these additional tiers by multiple modeling groups is ongoing. The results presented here are an important first step in understanding the inter-model spread in the response of Antarctic sea ice to meltwater from the ice sheet, and can be built on by this future work.”