Responses to the editor

We appreciate the detailed commonts from the editor Nicolas Jourdain. Below we copy the editor's comments in **bold black** and write our responses in **bold blue**. The changes to the manuscript are in *italic blue*.

L. 13-14: reformulate or explain "which makes it susceptible to ocean temperature changes in the deep ice shelf cavity, due to a low in-situ freezing point temperature". Do you mean that because of the quadratic dependency to the thermal forcing, a low freezing point implies that any ocean temperature change has a stronger effect on melt rates than with a higher freezing temperature? (with a linear dependency, this would not be the case).

Thanks for the suggestion. We have reformulated this sentence.

"This makes AmIS susceptible to ocean temperature changes in the deep ice shelf cavity (Galton-Fenzi et al., 2012; Wang et al., 2022). As ice shelf melt rates quadratically respond to external thermal forcing (Holland et al., 2008), ice with a low freezing point implies that it is more sensitive to any ocean temperature change than with a higher freezing point."

I find it hard to read the text with all the acronyms. While it is all right to indicate PB for Prydz Bay in a small figure, I think the text reads better if you use "Prydz Bay" in the whole text. Same for PC, AD, FB, FLB, GL. Ok for PBG that is used a lot and for PBECC that is quite long.

Thanks for the suggestion. We have removed all the acronyms for the geographic features in the text expect PBG.

L. 23-24: I am not a native speaker but "HSSW is dense and cold (slightly below the surface freezing point), which forms in coastal polynyas within PB during sea ice formation" would probably be more correct as something like "HSSW is a dense and cold (slightly below the surface freezing point) water mass that forms in coastal polynyas within Prydz Bay during sea ice formation".

Thanks. We have corrected the sentence.

L. 38: "a freshening in PB increases vertical stratification, and induces the warming" would be clearer as something like "a freshening at the surface of Prydz Bay increases vertical stratification and induces warming at depth".

Thanks. We have rephrased the sentence.

L. 68: Gurvan et al. (2017) should be Madec et al. (2017), Gurvan is his first name.

Done.

L. 93: "The top boundary is set at 30 m" -> The top boundary-layer thickness is set to 30 m.

Done.

L. 99-100: Are you sure that there is salt restoring in the ocean of UKESM1.0-LL? Usually, salt restoring is only applied in the standalone ocean configurations, not in the coupled models. Not sure that GO7 is relevant here if it is the standalone configuration (and if it is, GO6 rather than GO7 is mentioned in section 2.2). Please clarify this point. See also comment about L. 138.

Thanks for pointing out the mistake. No, there is no salt restoring in UKESM1.0-LL. The SSS restoration is applied in GO7. However, it is irrelevant to this work. We have removed the sentence "However, the GO7 configuration—the ocean core of UKESM1.0-LL—has restoring in the global domain (Storkey et al., 2018)."

GO7 is a standalone configuration. It is identical to the GO6 0.25° configuration except ice shelf cavities are open in GO7. We have added descriptions about the difference between GO6 and GO7 in the text:

"The difference between GO6 and GO7 mentioned before is that GO7 is identical to a higher resolution GO6 0.25° configuration except there are open ice shelf cavities in GO7 (Storkey et al., 2018). The differences between the 1° and 0.25° GO6 configurations are the mixing and boundary conditions, which are adjusted according to time step and grid spacing (Storkey et al., 2018)."

L. 102: "as they are prescribed in the UKESM1.0-LL outputs" -> "as they are in the UKESM1.0-LL outputs.

Done.

L. 98-102: this paragraph could be moved to section 2.3 where further information on the boundary conditions is provided.

Done.

Section 2.2: Consider pointing to Caillet et al. (2025) in which UKESM is very well ranked (Fig. 1 of their preprint) and in which an important natural variability is emphasized in front of Amery (Fig. 3 of their preprint). https://doi.org/10.5194/egusphere-2024-128

We have cited Caillet et al. (2024) in Section 2.2:

"UKESM1.0-LL is well ranked in terms of various Southern Ocean and Antarctic sea properties (Caillet et al., 2024, Figure 1)."

"UKESM1.0-LL also exhibits a large internal climate variability of salinity averaged between 200 m and 700 m in front of AmIS, which is important for HSSW formation in Prydz Bay (Caillet et al., 2024, Figure 3c)."

L. 136: "optimistic" is somewhat subjective -> low emission

Done.

L. 138: is GO7 a standalone ocean simulation? Please provide information on why you did not take the ocean outputs of UKESM1.0-LL?

We do not use UKESM1.0-LL mainly because there are no open ice shelf cavities in it. We have provided the information:

"The reason we chose GO7 rather than UKESM1.0-LL is that open ice shelf cavities are presented in G07 but not in UKESM1.0-LL. The oceanographic properties inside the AmIS cavity and in the open ocean are more physically consistent if the initial conditions are taken from one dataset."

L. 148: the model drift for which variable?

We have added the information: *"...until the model drift for ice shelf melt rate, ocean temperature and salinity becomes small (Figure 2a, b)."*

L. 190-191: "This suggests that our regional model [...] has a stronger response to climate warming" -> This suggests that our regional model [...] produces a stronger climate warming (or a stronger response to increasing emissions of greenhouse gases).

Done.

L. 166-170: it would be more robust to indicate mean values and intervals of confidence over a given period than approximated values at single dates. Same for the conclusion L. 5115-520.

Thanks for the suggestion. We have rephrased this paragraph and provided the projecting numbers here as well as in the conclusion as suggested.

"...The modelled melt rate is 0.75 $\pm$ *0.15* $\mathrm{m \cdot yr^{-1}}$ *(with 99.9% confidence intervals) over the period of 1976-2014. It is drastically increased to 13.14* $\pm$ *2.36* $\mathrm{m \cdot yr^{-1}}$ *over the period of 2076-2100 under the SSP5-8.5 scenario or 8.10* $\pm$ *1.53* $\mathrm{m \cdot yr^{-1}}$ *over the same period under the SSP1-2.6 scenario."*

L. 221: "the SSP5-8.5" -> "the SSP5-8.5 scenario" or "SSP5-8.5".

Done.

L. 221: the definition of water massES.

Done.

L. 260 "the two salinity" -> the two salinities (or the two salinity values)

Done.

L. 261: divergent -> diverge.

Done.

L. 262: "The difference is likely due to the reduction in sea ice". First, you should indicate "reduction in sea ice production" which is what matters for dense water formation. Second, this may not be the only reason: reduced vertical convection could also come from a more stratified ocean due to more freshwater released at the surface of the Southern Ocean in UKESM (for the wrong reason that the ice sheet mass is kept constant, i.e. that additional snowfall on a warmer ice sheet is injected into the ocean). The lateral boundary could propagate this signal in the regional domain.

Thanks for providing another explanation. We have rephrased as suggested.

"The difference is likely due to the reduction in sea ice production, which matters in dense water formation, while the increased mCDW is present on the continental shelf but has not yet reached the deeper cavity (Figure 5b, c). Reduced vertical convection resulting from a more stratified ocean may also cause the difference. Freshwater forcing from ice sheets is prescribed assuming the ice sheet mass is kept constant in UKESM1.0-LL (Sellar et al., 2020), which results in an additional snowfall on warmer ice sheets and increased freshwater released at the ocean surface. This bias is propagated through the lateral boundary to the regional domain."

L. 296: transition -> transitions?

Done.

L. 297: "HSSW becoming less efficient in driving the melting": what does this mean physically?

It is supposed to be "HSSW becoming less efficient in driving the circulation". We have rephrased it:

"This is accompanied by ..., likely due to reduced formation of HSSW outside of the AmIS cavity (Figure 5b, c), which becomes less efficient at driving the cavity circulation."

L. 326: the units should be J K-1 kg-1

Done.

L. 328: provide units for "334".

Done.

L. 342-343: I don't understand "its main flow travels offshore" here. What does this mean for a gyre?

We have rephrased this sentence.

"PBG is anticlockwise and weak before the 2050s, with off-shore transport at PBG transect (Figure 7a-c)."

Fig. 11b: The net sea ice production over the continental shelf would have been more informative than the sea ice volume, and more similar to the quantity used for ice shelves in Fig. 11a.

Thanks for the suggestion. We have replaced the sea ice volume with the net sea ice production in Figure 11b and Figure S6b.

Fig. 11c-e and related text: clarify whether Ekman pumping and the surface stresses are calculated from the stress at the ocean surface, i.e., either from wind or from sea-ice.

Thanks. We have clarified that the surface stresses refer to the stress at the ocean surface in the text.

Eq. (6): shouldn't there be a minus sign on the last term? Why not directly write the equation without the neglected effect of wind stress? If formulated like this, I think that τ_x should be called the Reynolds stress ($\rho\langle u'w'\rangle$) rather than the surface stress.

Thanks for pointing out the error. We have removed the stress term as suggested.

Eq. (7): shouldn't there be an SSH term (+$\rho_{ref} g dSSH$)? Could this be used to discuss the expected wind effect compared to the thermohaline effect?

Thanks for the suggestion. We have reformulated Eq. 7 as suggested to discuss the effect of SSH and salinity changes. We do not think the SSH term is solely controlled by winds, as the pattern of SSH gradient (Figure 10c) does not match well with the surface stresses (Figure 11c, d). Although we noted the surface stresses is different with the wind stresses, this may not fully represent the effect of surface wind. The changes in SSH gradient (Figure 10c) exhibit consistence with the changes in salinity (Figure 10b, mainly due to the outflow of the ice shelf meltwater), sea ice production (Figure 11b) and Ekman pumping (Figure 11e). It may suggest that the combined effect of the meltwater outflow, sea ice and surface winds causes the SSH change. To align with the modification of Eq. 7, we have also changed Eq.9 and Figure A1, and rephrased the paragraph to discuss the effect of salinity gradient and SSH gradient on the gyre reversal:

"The estimates are shown in Figure A1. The term of salinity gradient ($\frac{Hb}{\rho_{ref}}\frac{dS}{dx}$) has a magnitude of 8$\times$10^{-6} (Figure A1a). The magnitude of the horizontal SSH gradient ($\frac{dSSH}{dx}$) is up to 4$\times$10^{-6} (Figure A1b), which is less than 50% of the salinity effect. The reconstructed meridional velocity based on Eq 9 agrees with the modelled velocity (Figure A1c). This analysis suggests that the effect of salinity gradient and SSH gradient compete (Figure A1a, b). However, the reversal of PBG is a consequence of the reversal of horizontal salinity differences between PBG and its western regions."

L. 452-453: "overestimated sea ice" what? Production, concentration, . . . ? Same with the overestimation L. 454-455, with the reduction L. 471, with the decrease L. 476-477.

We have clarified the sea ice variable in the text.

L. 478: "are more possible" -> exist

Done.

L. 502: "the r2 ensemble of UKESM" -> The r2 member of the UKESM1.0-LL ensemble.

Done.

L. 502-503: "the r2 ensemble" -> the r2 member ; "the r1 ensemble" -> the r1 member.

Done.

L. 580-584: Rosevear et al. (2022) reported a 400% overestimation of the three equations of Jenkins et al. (2010). Here, in NEMO, the value of $\sqrt{C_d}\Gamma_T$ is 1.6 times smaller than in Jenkins et al. (2010). But Rosevear et al. (2022) also used a different velocity in the three equations (averaged from 7m to 19m below the ice) than what is implemented in NEMO. In addition, you tuned the tidal velocity to match observational melt rates (Fig. S1). Therefore, while it is important to raise the caveat of the melt calculation and to cite Rosevear et al. (2022), I would not mention the value of the overestimation (200-400%).

Thanks for the suggestion. We have removed the value.