Editor's final comments on EGUSPHERE-2024-1287

Line numbers refer to the track-change version

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).

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.

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".

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".

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

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

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.

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

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

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

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

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

L. 148: the model drift for which variable?

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).

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.

L. 221: “the SSP5-8.5” -> “the SSP5-8.5 scenario” or “SSP5-8.5”.

L. 221: the definition of water massES.

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

L. 261: divergent -> diverge.

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.

L. 296: transition -> transitions?

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

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

L. 328: provide units for “334”.

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

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.

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.

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.

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?

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.

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

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

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

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%).