We thank the reviewer for their time and effort in providing feedback on our manuscript. Our responses to their comments are given below, in red. In the case where no direct response is given, we will implement the reviewer's suggestions directly into the revised manuscript without further change.

Overview:

In this research article, Roiser et al. present numerical ice sheet modeling results of the Amundsen Sea Embayment (ASE), a dynamic region of West Antarctica that is rapidly contributing to global sea level, to 2100 and 2250 under RCP8.5 and Paris-2C emission scenarios. Central to the paper is the development of a new framework for quantifying uncertainties associated with these modeling results by training a surrogate model. Overall, the authors find that the sea level contribution of the ASE through 2100 is nearly identical in both RCP8.5 and Paris-2C, tending to the lower range of other published sea level projections of this region. Grounding line migration is minimal through 2100; however, iceberg calving drives near-total loss of all floating ice shelves. Beyond 2100, the simulations diverge as snowfall ramps up in RCP8.5. In terms of the uncertainty quantification, the authors found that parameters related to model initialization (e.g., hE and the inversion coefficients) comprised a majority of the uncertainty, followed by ice flow parameters, basal mass balance parameters, and surface mass balance parameters.

Overall, I find this manuscript to be of very high quality and I expect the results will be of wide interest to the glaciological and cryosphere community. I also believe that the methods of uncertainty quantification are exceptional and provide critical insight into ice sheet modeling parameters. I do share some similar concerns with the other reviewers in that I would like to see further interpretation of the forward simulation results. In particular, I find it very interesting that there is minimal grounding line retreat but near-complete loss of floating ice shelves through 2100 – I would like to see the authors dig into the reasons for this as well as into the reason why their estimates are on the lower end of published sea level estimates of this region. I also have a note in the general comments section that I think it would be appropriate for at least a subset of the ice sheet modeling results to be made publicly available given the broad interest in this region. Lastly, I also have a couple more minor line comments that should be easily addressed. Once these issues are addressed, I would be very happy to support prompt publication of this work in The-Cryosphere.

All reviewers agreed that the paper would be improved by further discussion and interpretation of model results and we will add this to the revised manuscript. Regarding the lack of grounding line retreat despite loss of ice shelves specifically, as we point out in the discussion this lack of sensitivity to ice shelf loss has already been shown for Thwaites glacier by Gudmundsson et al. (2023), and loss of floating ice shelf in front of Pine Island Glacier is less dramatic than Thwaites in our simulations, so this result is perhaps not entirely surprising. Following the reviewers request, we will also upload a subset of model results to a public repository.

General Comments:

Additional discussion related to modeling results: Like the other reviewers highlighted, I think that the results of this paper can be bolstered by diving deeper into analysis of the Ua_fwd projections at both 2100 and 2250. In particular, the authors present a unique implementation of the buoyant plume parameterization and a new way of prescribing oceanic temperature and salinity inputs to this parameterization (using the modeled depth of the thermocline as a depth-cutoff in applying T/S). Given that this region of Antarctica is primarily forced by the ocean, I would like to see how these modeled melt rates compare to contemporary estimates and how they change in time with evolving cavity geometry. I also think that comparisons of how much ice mass is added by atmospheric processes versus how much ice mass is advected across the grounding line in the different simulations would be helpful to diagnose why mass loss is generally low in these simulations. Lastly, as I mentioned below in the line comments, I would like to see the introduction of the manuscript expanded to provide an overview of ASE modeling efforts.

Some consistent formatting fixes: Throughout the manuscript, I noticed that in-text citations were not formatted correctly (e.g., in-text citations should be Lazeroms et al. (2018) and not Lazeroms et al. 2018). Also, there are many times in the manuscript where there needs to be a space between the value and the unit (e.g., 1mm should be 1 mm).

We apologise for the inconsistencies and mistakes that were missed in our initial submission and will fix these for the revised manuscript.

Data Availability: While the authors state that no new datasets were used in this article, they did generate ice sheet numerical modeling outputs that would be of great interest to the cryosphere community given how active numerical modeling efforts are in this area. While I understand that depositing all of the data might be challenging since there are so many model runs, I would implore the authors to deposit a representative subset of the results (or at least the data needed to replicate the figures) in a publicly curated data repository.

We will upload model results to a public repository. We are limited (a) by the way in which model outputs were saved for our two large ensembles, since most fields were only saved at the beginning and end of each simulation and (b) by the size of this dataset, which consists of thousands of simulations. However, we will upload a representative sample that can hopefully be useful to the community.

Line Comments:

Abstract: Much of the paper focuses on the development and implementation of the surrogate model and Bayesian calibration, so I was surprised to see that there was no mention of this in the abstract. If there is room, I would add a sentence about this.

We will add a sentence to this effect in the abstract.

L17: Capitalize "Pine Island"
L20: Change "rate" to "rates"

L27: What do you mean by " . . . the complex response of the ocean and ice shelf cavities to atmospheric forcing"? Is this referring to changes in wind stress that impact ocean circulation? For the ice shelves, does this mean enhanced surface melt that might lead to hydro-fracture? Are there studies that you can highlight that show the connection between atmospheric circulation and sub-ice shelf ocean cavity circulation for ASE glaciers? It would be good to specify and connect this to the next sentence because it feels a little ambiguous and out of place currently.

This is expanded on in the next sentence with citations but we will make the link clearer in the revised manuscript.

L37: Please add a citation for the plume model.

L15:42: I was hoping to get a little more background on ASE glaciers and recent ice-ocean model studies of this region. I think this will be particularly important if you choose to include additional discussion regarding the results of your ASE sea level rise projections. For example, this would be a great place to discuss what sea level projections of this region exist, what are their shortcomings, what are the partitions of sea level contribution between Thwaites and Pine Island, what ocean and atmospheric forcing datasets and parameterization have been used, have tipping points been found in previous studies (note that these are examples and you can select what you think would be appropriate)? A bit more background information to help put your research in the context of what has been done already would be very useful.

We agree with the reviewer that a background on ASE glaciers and putting our results in the context of existing studies is important. Currently this is done quite extensively in Sect. 5 of the paper, and we prefer to keep the bulk of this here to avoid repeating things, but we will add more background on ASE glaciers to the introduction following some of the suggested points above.

L44: Add a comma after "uncertainties"
L45: Change comma to a period after "together"

L62: What was the minimum and maximum mesh resolution?
We will add this information to the section dedicated to describing the mesh in Sect. 2.1.7.

L65: Please add a citation for Glen's flow law.
L72: Please add a citation for the friction law. Also, what does *m* control in this friction law?

L100: Where did the value of 1.66 come from?
This is a result from the Wake and Marshall (2015) reference given in the next sentence.

L127: ". . . in a coordinate system X oriented along the . . ."

L138: What was the reasoning for picking C_d^{1/2}Gamma_{TS} as the uncertain model parameter rather than C_{d}, the ice-ocean drag coefficient?
The Stanton number is a turbulent exchange coefficient that determines the exchange of heat across the ice-ocean interface. This is a process that is poorly represented and a source of considerable uncertainty, and although part of that relates to uncertainty in roughness at this interface (hence C_{d}), that would alter the rate of heat transfer, all studies of ice shelf melting that I am aware of use the Stanton number or just Gamma_{TS} as their tuneable parameter to match observed melt rates.

L142: Remove "of"

L148: Was there a specific reason you left out melting from geothermal heat flux? It seems like this would be a relatively simple addition to the model since you can assume it stays constant through 2100. I don't think this would be a reason to re-run the simulations, but I am just curious.
We justify this in the same line, but the reviewer is of course correct in that it would have been a very simple addition and perhaps should have been added for completeness. Unfortunately this would require re-running all our simulations with no strong justification.

L151: Check the formatting of your references here – I think they should be "Pollard et al. (2015) and DeConto et al. (2021)
We will carefully go through our references to make sure they are correctly formatted.

L158: General comment about units, make sure to add a space between the value and the unit (e.g., 1.5 m/yr rather than 1.5m/yr). I saw this come up in a couple of lines now. The only unit that should be directly against the value is the degree-symbol.
We will ensure to use correct spacing between units in the revised manuscript.

L174-175: Citation should not have parentheses

L200: What version of BedMachine do you use?
We used Bedmachine v3.4 (03-Jun-2022). We will clarify this in the revised manuscript.

L206: What specifically are these assumptions? Do you assume floating ice is at hydrostatic equilibrium? If so, please state this and other assumptions explicitly.

Yes we assume floating ice is at hydrostatic equilibrium and we will clarify this in the revised manuscript.

L235-237: This description of how ocean T/S is applied to the plume model is a bit vague. So within each of the three major ice shelf cavities (Pine Island, Thwaites, and Crosson/Dotson), you extract the thermocline depth and T/S above and below that depth. So you get 15 values (5 for each of the three ice shelves), but how is that T and S applied within the plume model? At all ice shelf depths above the thermocline, you apply the average T above the thermocline, and vice versa for below the thermocline? Is this done at every time-step? How does the location where you obtain these T/S/depth variables change as the ice shelf cavity geometry changes in time (i.e., do you always sample from the same locations, or does the location follow migration of the grounding line and ice front)?

Yes, we end up with 15 values for each point in time. Temperature (or salinity) below the thermocline is constant, then from the thermocline depth to z=0 temperature increases linearly to the surface temperature. The temperature, salinity and thermocline depth fields are sampled from the MITgcm model 10 times a year and at each ice sheet model time step we find the closest point in time in the corresponding MITgcm model to extract these 15 values. For all simulations, we define 3 basins for Pine Island, Thwaites and Dotson/Crosson, based on present day drainage basins, defined everywhere in the domain and extrapolated out into the ice shelf, and check which basin a node lies at any point in time to determine which MITgcm field we use for that location. We will expand on the description of this in the revised manuscript.

L415: It might be nice in this section to also mention the rate of sea level rise of the ASE, not just the final 2100 contribution. In figure-5, it looks like the rate of sea level contribution from RCP8.5 starts to decrease after 2070, do you know why that is?

The rate of sea level contribution for RCP8.5 (perhaps more clearly seen in Fig. 9) is relatively stable until approximately 2060, after which it declines until approximately 2100, before increasing again. We can add a reference to Fig. 9 in this section to point the reader to this information. Regarding what causes this reduction in the rate of sea level contribution, our interpretation is that this is caused by the increase in snow accumulation seen in RCP8.5, acting to counteract dynamic changes in the region and thus reducing the sea level contribution in this period. Once the surface mass balance stabilises (as we remove the trend from 2100 onwards) this effect is removed and continued changes in ice dynamics lead to further adjustment in the regional sea level rise contribution.

L435-455: I agree with the other reviewers that further discussion of these results would be really valuable! In particular, it is quite surprising that you see little grounding line retreat across both Pine Island and Thwaites Glaciers – some analysis on the ice shelf melt rates that were applied throughout these simulations would be very helpful. Perhaps you could show what the melt rates look like at a couple of different time-steps? You could also show a time-series of total integrated ice shelf basal melt across the main shelves in the domain.

Many model outputs were not saved due to the number of simulations involved. We are re-running a representative sample of simulations with increased outputs including melt rates and will add extended analysis including melt rates to the revised manuscript.

L451 In figure-6, do you know why Pine Island Ice Shelf ends up shaped like a triangle at both 2100 and 2250? I am very surprised to see that this same ice front shape holds throughout the whole simulation.

As mentioned in our reply to reviewer 1 and in the paper, we selected this calving law not necessarily because we believe it to be the most physically plausible, but because we wanted to be able to make some limited comparisons to results of the Pollard and DeConto modelling studies. We find this calving law tends to result in quite consistent calving front positions and in particular for Pine Island Glacier these seem to be relatively insensitive to grounding line position (although there are some examples of very different calving front positions that can be seen upon closer inspection of Fig. 6). In the case of Pine Island Glacier, the calving front seems to be generally anchored to sidewalls where the embayment noticeably widens, and presumably this is primarily driven by the dependence of basal and surface crevasse depth on ice divergence (Eqs B.1a and B.1b in Pollard et al. 2015).

L478: Remove “in the” once; you have it written twice consecutively in this line.

L481: This comparison to past results is a great starting point! I do think that you should go a step farther and try to deduce why your modeled sea level contributions are on the low end of other published results. Like I said before, I think it is worth looking at what your ice shelf basal melt rates look like in the Ua_fwd simulations and how they change in time (a figure of this would be valuable, maybe in an appendix or supplement). Another possible cause of limited mass loss could be the treatment of surface mass balance, which you could look into by quantifying how much snow fell over the simulation period and how much this offsets ice loss at the grounding line. Do the other projections you cited include atmospheric forcing as a positive degree day parameterization, or do they directly apply CMIP anomalies in SMB within their model without correction?

We are re-running a sample of simulations that will enable us to expand on the analysis, and will investigate these ideas and other excellent suggestions to add to the analysis for the revised manuscript.

L495: Is this statement about your melt rates true? I thought that the plume parameterization updates ice shelf melt based on changing ice shelf basal slopes and the depth of the grounding line? Maybe it would be more accurate to say that you cannot resolve 3-dimensional sub-ice shelf ocean circulation.

The statement specifically mentions ocean forcing, which in this case are derived from the un-coupled ocean model simulations, rather than the melt rates calculated by the plume model. The former are not updated in response to changing cavity geometry whereas the latter are, however this is certainly easy to misinterpret and we will reword the sentence to make our meaning clearer.

L505: Might be worth comparing to the new Science Advances paper by Prof. Morlighem (https://www.science.org/doi/10.1126/sciadv.ado7794). They do not find any evidence for MICI over the next 50 years, so this supports your findings well.

Thank you for the suggestion we will include this citation in the revised manuscript.

L525-530: A similar analysis of results for your 2100 simulations might be helpful in determining why mass loss is on the lower end of published projections.

We will include the same analysis for the final year of our 2100 simulations in the revised manuscript.

L533-535: Can you determine or at least speculate on why there is a decrease in RCP8.5 mass loss prior to 2100?

We prefer to avoid speculating and will wait until we have completed our extended analysis once our subset of simulations with additional outputs has finished.

L541: The sea level contribution is similar up until 2100, but not through 2250. Please specify this.

We will clarify this statement pertains to simulations up to 2100 only.

L560: Check the format of the citation, should have parentheses.

Figure Comments:

Figure 1: This is a very helpful figure and I like that you included the associated section and appendix numbers in each of the boxes. In the figure caption, can you please include the definition of acronyms (e.g., RNN, LSTM, and Del GMSL).

We will include definitions for these acronyms in the revised manuscript.

Figure 2/3: Should there be a color bar associated with this figure? Also, is the top row showing surface ice speed, or the change in surface ice speed (as would be consistent with a divergent colorbar)? This last comment applies for figure-3 as well – should this say change in surface ice speed? Lastly, for figure-3, why are there no changes shown over floating ice?

We will add a colorbar to these figures and correct any reference to ice velocity that should be surface ice speed. We mask out changes on floating ice shelves and do not include these in the Bayesian inference step since this would complicate our interpretation. As the calving front evolves in our simulations, changes in its location such that the extent of observational data and model data no longer match could create a very strong sensitivity to parameters affecting this process in comparison to other changes. In addition, the surface elevation change dataset only includes data on grounded ice, due to the processing and larger errors involved in calculating this field over floating ice shelves.

References:

Gudmundsson, G. H., Barnes, J. M., Goldberg, D. N., & Morlighem, M. (2023). Limited impact of Thwaites Ice Shelf on future ice loss from Antarctica. *Geophysical Research Letters*, 50, e2023GL102880. https://doi.org/10.1029/2023GL102880

Pollard, D., DeConto, R. M., and Alley, R. B. (2015) Potential Antarctic Ice Sheet retreat driven by hydrofracturing and ice cliff failure, Earth and Planetary Science Letters, 412, 112–121, https://doi.org/https://doi.org/10.1016/j.epsl.2014.12.035