Reviewer 1 (Dr Zhang) feedback

We thank Dr Zhang for his helpful comments on the first paper draft. Where minor suggested edits have been proposed, these are incorporated into the revised paper and we do not include them below. We respond to more substantial comments in the following. Reviewer comments are bold and italicised for clarity, followed by our response.

L32-33: I think it is probably better to say something differently between EAIS and WAIS if the SMB change pattern is different, which is in consistent with the experiment design of this paper.

We will edit these lines to reflect that increased warming increasing snowfall is a more important phenomena for the EAIS.

L58: put the sentence "BISCILES ISMIP6 ..." in a different paragraph and elaborate the reason you choose BISCILES for this study.

As suggested, we will include a paragraph break and elaborate our reasons for choosing BISICLES:

BISICLES ISMIP6 experiments were included in the synthesis and sensitivity tests of Edwards et al. (2021). We chose BISICLES to complement the original ISMIP6 ensemble experiments because of it's use of the L1L2 flow approximation, making it well suited to simulating marine ice sheets, and adaptive mesh refinement. This allows BISICLES to capture grounding line dynamics at high resolution, whilst maintaining computational efficiency"

L85-86: Regarding "m=1/3 and Coulomb friction coefficient", I think you still need put some basic important equations here, like several equations describing the L1L2 approximations, and then you can properly get those model parameters settled somewhere.

As suggested, we will include a more detailed set of model equations, if not in the main text then in the supplementary section.

L91: Regarding "the calving front is fixed", but there are also several experiments that you calve all ice shelves away, correct?

No, collapse experiments only remove shelf area where 10-year average melt exceeds 725 mm/a in CCSM4 – i.e. over limited regions of the shelf. However, we will amend the main text to make this clearer.

Table 2: Looking at the Collapse On experiments here, it reminds me ABUMIP. Have you compare your results with that of ABUMIP? If not, I suggest doing some

analysis. Also, the numbers at the "Sea level contribution" column are not exactly the same as in your following figures (e.g., figure 11), please check.

Whilst the collapse on experiments remove *some regions* of the ice shelves instantaneously, this is not quite comparable to ABUMIP. ABUMIP removed *all* ice shelves immediately, whereas our collapse on experiments remove regions of shelf ice according to the mask calculated from CCSM4 2m air temperature, calculated using the equation from Trusel et al. 2015 (ref). We will edit the paper to make this clearer to the reader.

L268-270: I don't think this discussion is necessary here, as SLR is directly contributed by VAF and there is very complex relationship between basal melt of ice shelf and SLR.

As suggested, we have removed these sentences.

Figure 10: For SMB, why are all ice shelves are missing? In addition, the spatial pattern here is not clear. Maybe you should try another way to plot them - maybe log scale?

We have amended the figures to make the shelf edge contours clearer and changed the colour scale for improved clarity.

L305-312: I think you should say something about the two major ice shelves in WAIS, Filchner Ronne and Ross ice shelf. For example, does the basal melt of Filchner Ronne ice shelf increase nearly proportionally to that of Ross ice shelf?

We will include discussion of how basal melt co-evolves in time for the two major WAIS ice shelves.

L325-330: So can we get a conclusion that the buttressing of ice shelf can contribute a 20-30 mm SLR?

We cannot say this as we do not look at full shelf removal. However, we could make a more caveated statement that 'Based on the temperature-melt relationship proposed in Trusel et al. (2015), and a conservative interpretation of the limit of stability for ice shelves, under CCSM4 temperatures, ice shelf collapse can contribute to 20-30 mm SLR to 2100'.

Section 4.5: This is probably the major concern of this study. The analysis and figures here seems to be a bit overlapping with previous studies like Seroussi et al. (2020). I doubt if it is necessary to compare BISICLES with all other different types of models. Maybe you can just compare it with other higher order models, which I

think makes more sense. Then it might be possible that you can put all comparisons in a single figure, instead of showing them similarly in 3 figures (14-16).

We have merged the figures into a single figure showing only those experiments mentioned in the main text. Whilst it could make more sense to only compare with other higher order models, other aspects of model set up (e.g. resolution, initialisation, treatment of basal sliding, numerical error) can have a large impact, so we think it may be good to keep the comparison with other models in the figures. As highlighted, some of the comparison does overlap with previous studies. We will therefore edit down the paragraph beginning line 398.

Figure 13: Please put the thermal forcing curves in a separate plot, which will make a clearer and nicer figure.

We will edit the figure so that the thermal forcing curves are in a separate plot.

Section 4.7: It is not clear to me if you have done the hindcast experiment. Please clarify.

We agree that we have not sufficiently detailed whether we did hindcast (historical) experiments. Other reviewers have also asked for more clarity around the initialisation process and relaxation run. We will clarify the approach of initialising the model with 2007-2010 velocity observations, completing a relaxation run, and the simulating from 2010.