

## Reviewer 3 feedback

We are grateful to Reviewer 3 for their detailed and constructive comments, and helpful suggestions. 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.

***1. I found the both the Introduction and Discussion sections to be unnecessarily long. There are also some instances of duplicated text. I think the Introduction would benefit from being shortened and more concise in places.***

See response to comments below.

***2. I think the paper would benefit by presenting more detail on the initial ice sheet model state and comparing this to observations and the other model states included in the ISMIP6 experiments. It would be good for the Control simulation section of the Results to include more detail on this, and perhaps an additional figure (see Specific comments).***

See responses below.

***3. I have several comments with regards to the Figures. 1) I think the number of figures in the manuscript is a little excessive and I wonder if all of them are absolutely necessary for the main messages of the paper. I think some could be moved to a Supplementary Information document at the least. 2) In many cases the Figure captions are not as detailed as they need to be and the individual panels on almost all figures should be labelled. For both points I've included suggestions in the specific comments below.***

See responses below.

***4. The Discussion as mentioned above is very long and in places presents a stream of new results, and often without any reference to Figures/Tables where the results can be found making it difficult to follow. I think it would be good if each section of the Discussion is considered whether it belongs there or in the Results. At the very least I think Section 4.1 should be moved to the Results. In general it would be good to consider what detail is/is not necessary, and I've made some suggestions in the specific comments. Also the Discussion mostly compares the results to those of Edwards et al., 2021 and Seroussi et al., 2020, and could benefit from referencing additional papers to support your findings.***

See responses below.

***5. The Limitations section could benefit from adding several more. For example how might your results have been different if you had used a different basal melt***

***parameterisation? Also it would be good to mention the choice of sliding law, and how this is a potential source of uncertainty that has not been explored in these experiments. Finally, it would be good to discuss some unaccounted for processes in your experiments, e.g. iceberg calving (fixed ice front), and the SMB-elevation feedback/evolving topography during the simulations (that you do mention on Line 128).***

As suggested, we will expand the limitations section to include those suggested.

***6. I think the Conclusions would benefit from being revised, making sure they are a brief summary of the paper and not introducing any new information, and towards the end providing summary sentences on the key finding(s) of the paper and the wider importance.***

We will revise the conclusions as suggested.

***Line 6: could include examples of the uncertain ice sheet processes you are exploring***

Edited ‘...more comprehensively explore uncertain ice sheet processes.’ To ‘...more comprehensively sample uncertainties in future climate, ice shelf sensitivity to ocean melting, and their interactions.’

***Line 8-10: I think this sentence would benefit from being rephrased for clarity***

Edited:

“The BISICLES experiments presented here show the important interplay between surface mass balance forcing and ocean driven melt, with high warming, high accumulation forcing conditions leading to mass gain (negative sea level contribution) under low sensitivity to ocean driven melt.”

To:

“We present BISICLES experiments showing the important interplay between surface mass balance processes and ocean driven melt in determining Antarctic sea level contribution. Under higher warming scenarios, high accumulation offsets more ocean driven mass loss, when sensitivity to ocean driven melt is low.”

***Line 12: “increases sea level contribution by 25 mm” relative to what? I think the whole range of sea level contributions from your experiments should be stated in the abstract.***

Edited to “...increases sea level contribution by 25mm, relative to the no collapse experiments, for both....”

Sentence added on line 12:

“...accumulation. Overall, we simulate a sea level range across our experiments from X mm to X mm.”

***Line 16: Does this mean “dominated the total sea level change of 20 cm” or ice sheets contributed to 20 cm?***

Mountain glaciers (41% of total) and thermosteric sea level rise (38% of total) together dominated sea level rise from 1901 to 2018 (Fox-Kemper et al., 2021). Will rephrase sentence beginning line 15 to clarify this:

“...global mean sea level (GMSL), behind thermosteric changes and mountain glaciers (Palerme et al., 2017; Horwath et al., 2022) – which together accounted for 79% of the 20 cm of sea level rise between 1901 and 2018 (Fox-Kemper et al., 2021).

***Line 36: “future” Antarctic contribution to sea level? Also “represent a greater range of interactions and dynamic processes” reads a bit awkwardly, could it be rephrased, or examples of these “interactions” added?***

We didn't specify “future” as ice sheet modelling advances have improved characterisation of palaeo sea levels too. However, given the context of the paper, we will add “future” to remove any ambiguity. Suggested edit for improved readability:

Rephrased:

“To better project Antarctic contribution to sea level, ice sheet models have been developed to represent a greater range of interactions and dynamic processes, at higher resolution than ever before, over the past few decades (Pattyn et al., 2017)”

To:

“Ice sheet models are the primary tool for projecting future Antarctic sea level contribution. Over the past few decades, models have developed to represent a greater range of ice sheet processes and climate-ice sheet interactions, at higher resolution than ever before (Pattyn et al., 2017)”

***Line 44-47: These sentences duplicate the information in the previous paragraph (lines 39-43), I suggest combining and removing the second paragraph of the two.***

As suggested, will merge paragraphs and remove duplicated information i.e.

“...from the Greenland and Antarctic ice sheets (Nowicki et al., 2016). With a common set of experiments run by different modelling groups, it allows for improved quantification of uncertainty in sea level projections due to choice of ice sheet model.”

***Line 49: I think the estimate of sea level contribution from the ISMIP6 experiments could come earlier/in the first paragraph of the introduction.***

We think it is useful to have some explanation of what ISMIP6 is before giving the results, but we could give the ISMIP6 sea level contribution range in a sentence at the end of line 47.

***Line 60: See later comments and in the general comments section. How does your initial model state here compare to the one included in the initMIP experiments?***

We use the same model initial state as that used in initMIP, we will edit the text to make this clear.

***Line 79: So the model set-up in this paper was exactly the same as the one presented in the initMIP experiments? that is not totally clear from the text. Does that include the choice of basal sliding law, grid resolution, etc? Despite this, I think more space could be given here and in Section 2.3 on how this initial state compares to that of the other models and to observations.***

Yes, we will edit the text to make this clear. As suggested, we will add more detail about the initial state.

***Line 82: A figure of the model mesh would be good in a Supplementary Information document***

The model mesh is adaptive and is simulation dependent i.e. simulations have different meshes, and the mesh evolves through time. However, we are happy to include an example model mesh in the supplementary material.

***Line 85: Perhaps state why this sliding was chosen.***

Add sentence on line 86:

‘...coefficient of 0.5. This sliding law accommodates regions of hard beds and slow flow through the Weertman law, and regions of faster flow on deformable beds through the Coulomb law, as well as a smooth transition between the two (equation n., supplementary section n.). Basal traction coefficients...’

***Line 91: How long was this relaxation run? and how does the ice sheet state deviate from the one arrived at after the inversion? Some figures on the change in ice thickness/speed during this relaxation and the comparison to observations at the end of the relaxation/start of the control simulations would be good.***

The relaxation simulation is approximately ten years long. As suggested, we will include figures and text detailing the relaxation simulation, and how it compares to observations in the supplementary material.

***Line 100: The first few sentences of this section duplicate the information already presented in the Introduction. I suggest removing either here or from the introduction (lines 63-66).***

We will remove the duplicate sentences.

***Line 105: Is this the same selection of CMIP6 models included in Payne et al. 2021, if so state this.***

Payne et al. 2021 explore 4 CMIP6 models including CNRM-CM6-1. We chose this model because it had a low emissions scenario available. We will edit the text to make this clearer.

***Line 125: Why did you not use PIGL5th? State this in the text.***

Edited to:

‘...are sampled in the simulations presented here (Table 2). With limited time and computational resources, we did not use PIGL<sub>5th</sub>, prioritising instead higher gamma<sub>0</sub> simulations to bound the ice sheet sensitivity to ice shelf basal melting.’

***Line 127: Why did you choose to use the surface mass balance from Arthern et al., 2006 rather than RACMO?***

Edited to:

‘mass balance from Arthern et al. (2006), following previous BISICLES studies (Cornford et al. 2016) and BISICLES initMIP experiments (Seroussi et al. 2019).’

***Figure 2: The units between the spatial plots and line graph in Figure 1 are the same. It would be good to do that for the SMB anomalies in Figure 2 as well.***

We will edit the figure accordingly.

***Line 141-142: This sentence is almost identical to the one on line 127-128, remove one of them.***

We will edit the text to remove the duplicate sentences.

***Line 142: As mentioned above, it would be good to have more details on your initial model state, the relaxation run, and the subsequent control simulation. It would be useful to include a figure of the surface mass balance and basal melt rates used during the control simulation.***

As suggested, we will include figures and text detailing the relaxation simulation and initial model state. We will also include a figure of the control surface mass balance and basal melt rates for the control simulation in the supplementary information.

***Line 151: None of the place names in this paragraph are labelled on a figure. I guess the assumption is people will know where they are, but it would be nice to label the main ice shelves.***

***Line 158: It's almost impossible to see the grounding line retreat on Fig. 3b, I suggest making the map much larger or even creating an inset(s) for the key regions talked about in this paragraph.***

As suggested, we will edit the figure for clarity and label the main locations mentioned in the text.

***Line 187: Be consistent with use of negative numbers for mass gain/sea level fall. Improve the readability of the following sentence.***

Following reviewer 2 feedback, we will revise all presented sea level contribution results so that the control is not subtracted, but will ensure consistency in revised results.

***Line 194-197: Am I correct in thinking you didn't merge these basins and this was done in Jourdain et al., 2020? If so, just state that you use the basins from Jourdain et al., 2020 and remove the detail here.***

This is correct, we will edit the text accordingly.

***Line 200: It would be nice to see the perturbation experiments alongside the control simulation (without subtracting it). Perhaps a figure in a supplementary information document.***

Following reviewer 2 feedback, we will edit the manuscript to present main results without subtracting the control simulation. For completeness, and to aid comparison with the main ISMIP6 results, we will also present the results with control subtracted in the results table 2 (or supplementary).

***Line 206: Make it clear when you say "Filchner-Ronne has a large area..." you are talking about the catchment not just the ice shelf?***

Edited to "Filchner-Ronne drainage basin has a large area..."

***Line 210: The important compensating effect of accumulation on ocean-driven mass loss has been noted for the Filchner-Ronne region by a number of previous studies, some of which might be worth citing here e.g. Cornford et al. 2015 and Wright et al., 2014.***

We agree that it would be useful to include these references, and will edit the text accordingly.

***Section 3.4: Throughout this section there are almost no references to any figures where the results can be found.***

We will edit the text to add figure references where relevant.

**Figure 5: This figure caption is incomplete. State what each panel shows and add the CMIP forcing to the caption as well.. Also add panel labels to the figure.**

Figure caption edited to:

“Subplots show the grounded area for NorESM1-M (a.), CCSM4 (b.), MIROC-ESM-CHEM (c.) and CNRM-CM6 (d.) experiments from 2015 to 2100”

**Line 224: Could state that an increase in VAF in the Filchner-Ronne and Ross regions in the control simulations is consistent with the present-day trend in VAF and some references e.g. Rignot et al., 2019.**

Edited to: “...VAF throughout the control simulation – broadly consistent with 1979-2019 VAF trend in these regions (Rignot et al., 2019).”

**Line 225: Given that this section of the results is quite short it could just be combined with the basin discussions in the previous section. Figure 9 could probably be removed or moved to a supplement.**

As suggested, we will merge this with the previous section, and move Figure 9 to the supplementary text.

**Line 222: Be careful here, this sentence is quite confusing, you are saying that it is “equivocal” that there is potential for MISI in the ASE, but that there is not evidence that for MISI happening yet. I would rephrase this to improve the clarity.**

Rephrased:

“Evidence for marine ice sheet instability in the ASE is equivocal ... incontrovertible evidence of MISI (Fox-Kemper et al., 2021).”

To:

“It is not clear that marine ice sheet instability has been initiated in the ASE, with the IPCC AR6 stating that observed flow regimes in the ASE are compatible with but not incontrovertible evidence of MISI (Fox-Kemper et al., 2021).”

**Figure 7: Needs panel labels and increase the font size in the legend.**

Figure will be edited as suggested.

**Section 4.1. The entirety of this section reads as a stream of new results and reference to new figures. I think this would be better moved to the results. I also don't know how useful the spatial discussion is, and was wondering if Figures 10/11 would be better in a supplement and replaced with a figure that shows integrated ice sheet/regional surface mass balance and dynamic ice discharge instead.**

Following feedback here and from other reviewers, this section will be moved to results. As suggested, we will replace the figures 10 and 11 with a single figure showing regional (EAIS, WAIS and Peninsula) integrated surface mass balance and discharge.

**Figure 8: The y-axis label is “SLC” but throughout the manuscript you use “SLE” I would be consistent with one or the other. Also I can see the reason for a consistent y-axis scale, but it means in some basins it is impossible to see any change through time, so I recommend modifying the y-axis scales individually.**

Axis labels and scales will be changed as suggested.

**Figure 9: I am not sure about showing the SLC lines on top of the spatial plots, they are difficult to see and in several cases they obscure the text. Also the colourbar for the thinning is quite saturated. Could you just show the inland/grounded ice thinning only instead?**

We will edit the plots to only show grounded ice thickness change as suggested, and improve legibility of the line plots and text.

**Line 278: Not sure this should be the start of a new paragraph. Also, is the same true for other unconstrained ice shelves, e.g. Thwaites?**

Yes, sector 9 (ASE) sea level contribution for CNRM relative to control is lower under SSP5.85 than SSP1.26. Will add reference to figure 8.

**Line 279: I did not find a clear statement as to why you did not also use the PIGL sensitivities for these CMIP6 model runs?**

See response to earlier comment on this.

**Figure 10: The caption is incomplete. Refer to experiments in Table 2 (as done in Fig. 9). See earlier suggestions to replace this figure with integrated ice sheet/region wide SMB.**

As suggested, this figure will be replaced with a figure showing regional (EAIS, WAIS and Peninsula) integrated surface mass balance and grounding line discharge.

**Line 296: This section also reads as new results. If it remains as a Discussion section I suggest adding references to more papers that have shown similar results. E.g. for statements about compensating effects of increased precipitation with warming in East Antarctica mass for example Jordan et al., 2023- <https://doi.org/10.1038/s41467-023-37553-2> and Stokes et al. 2022 and references therein <https://doi.org/10.1038/s41586-022-04946-0>.**

As suggested here and by other reviewers, we will move results into the results section. We will include references suggested in edited discussion section.



***Line 305-312: This paragraph again is a list of new results without any reference to any figures/tables where these results can be found. Same is true for the following paragraph.***

As suggested here and by other reviewers, we will move results into the results section, and include figure references where needed.

***Line 356: Given that this equation is not used in this manuscript it feels a bit out of place in the Discussion, I would suggest removing it and just referring to the equation number in that paper instead.***

Lipscomb et al. 2021 do not give the equation separately in their 2021 paper, they state the equation from Jourdain et al. 2020 and write their modification in the text. However, we will remove the equation as suggested, and reference Lipscomb et al. 2021.

***Line 414: There are several references to Sections of Edwards et al., 2021. I think in most cases it would be better to briefly summarise the findings in that paper in your manuscript so the reader doesn't have to frequently go back and forth.***

We will edit the text accordingly.

***Figures 14, 15, 16: The text describing the details of each simulation overlaps large parts of the figure/results, I suggest removing and just directing the reader to Table 2, where the details of the experiments are.***

Following reviewer 1 feedback, we will replace these figures with one showing the experiments mentioned in the main text.

***Line 421: It is my understanding that future work is underway to extend simulations to 2300? is that worth mentioning here?***

Edited sentence 'To confirm this, extending ... worthwhile extension on this work'. To 'Work is ongoing to extend these simulations to 2300, and will shed valuable light on mass loss under high basal melt sensitivity beyond 2100'

***Line 427: Can you state why you omitted the PIGL5 values?***

See earlier comment response.

