Reviewer 2 (Robel) feedback

We are grateful to Dr Robel for reviewing the manuscript and providing thoughtful and constructive feedback. 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.

I do think much of the "discussion" section felt like just further description of results. In particular, I would suggest that section 4.1-4.4 be moved into section 3 since they are mainly a description of the results without much discussion or comparison to other studies. I also think these discussions of the results should probably be condensed by maybe 20-30% for the sake of readability.

The discussion section reading like more results, particularly section 4.1, is an issue that has also been highlighted by other reviewers. As such, we will edit the discussion so that new results are moved to the appropriate paper section. Moreover, on reflection and following reviewer feedback, we agree that editing the manuscript down would be improve readability. As suggested, we will edit down the discussion of the results in the section highlighted.

Perhaps my main concern with the science in this paper is how the control run is treated and discussed. I understand that it is standard ISMIP6 practice to subtract the control run from all results such that the resulting numbers represent "sensitivities" of the ice sheet to future emissions forcing. However, this procedure is then somewhat at odds with presenting the results as true projections of future sea level rise, as they are in this paper. While here in this paper and in other ISMIP6 publications the control is often discussed as representing model "drift", it does lump together many potential real sources of ice sheet change including the transient evolution of the initialized ice sheet state, which is out of equilibrium. The paper says as much around lines 220-221 where it says "Whilst subtracting the control can account for model drift, it may also in this instance be removing the sea level signal from ASE's long timescale to retreat initiated before 2015". I think this is quite important because the control run here simulates a non-trivial contribution to sea level rise (6 cm), comparable at first order to the forced changes simulated in the non-control simulations. Thus, when the paper says (e.g.,) that so-and-so simulation represents a "sea level fall", this isn't accurate. Rather, such simulations represents less sea level rise than in the control simulation, but the raw simulation is in fact projecting sea level rise (since even the most "sea level fall" is 5.3 cm, less than the SLR in the control). This can be quite confusing for a reader who is looking to this paper simply for sea level projections. My suggestion would thus be to revise the language throughout the text to discuss the projections as being relative to the sea level rise simulated in the control (i.e., not a "sea level fall" but "less sea level rise than in the control", and not "sea level rise" but "more sea level rise than in the control"). You say something like this for one part of the analysis (line 201), but it applies to all the analyses presented in this paper. Alternatively, you can just not subtract the control run in the plotted results

as presented, while still discussing the control run at length. I understand that this is a departure from ISMIP6, but given that we are already moving on to ISMIP7 as a community, this paper could point to a better way to think about considering the control run.

As noted by the reviewer, we follow the ISMIP6 convention of subtracting a ‘control’ run from our main projections. However, we are not consistent enough in presenting these results as ‘relative to control’, which can confuse the reader as pointed out by Dr Robel – e.g. presenting simulations as “sea level fall”, when in fact they show smaller sea level rise than in the control. As pointed out in both the reviewer feedback and line 220-221 of the manuscript, subtracting the ‘control’ does remove some dynamic sea level contribution not primarily driven by model forcing, that is none-the-less an important part of the future sea level contribution in our model. We therefore agree that it may be clearer to not subtract the ‘control’ simulation in our plotted results. However, for ease of comparison with other ISMIP6 publications, we will include results with control subtracted alongside those with the control not subtracted in table 2 (or supplementary).

L90: It would make more sense to say that you set the rate factor in effective viscosity purely based on temperature and then you also invert for damage given the A(T) field. (If I understand properly). Not sure if this is different than just inverting for A, but perhaps I don't understand.

We have made the following edit for clarity:
“Whilst BISICLES uses a depth integrated momentum balance equation, the rate factor A(T) in effective viscosity is based on 3D ice temperature. The inverted parameter phi corrects the vertically integrated effective viscosity in essentially the same way as a damage parameter D (phi = 1 – D), but will conflate the influence of errors in the ice temperature and thickness, as well as the form of the rate factor A(T) (Cornford et al. 2015)”.

We have also changed “…the ice damage coefficient are estimated…” to “…the effective viscosity coefficient phi…” on line 86.

Section 2.2: how is basal melt treated at/across the grounding line?

The sentence “Basal melting is only applied in cells whose centre is at floatation” will be added to the end of line 125.

L142: I'm quite confused about why basal melting is applied in this way for the control run? Is it time dependent? Does it vary as the model evolves or is it prescribed at the beginning and then held constant. I'm not sure how I understand the sense in which this is a control. Don't other ISMIP6 models just apply a constant-in-time basal melt forcing for the control?

The basal melt is time dependent in the sense that it adjusts to remove additional thickening in floating grid cells as this evolved through time (i.e. from advection and SMB). In projection simulations, melt anomalies are applied so that thinning corresponds to the melt anomaly as for BISICLES initMIP experiments. As suggested by other reviewers, we will provide more detail on the control.

Section 3.1: the paper mainly discussed how the forced simulations compare to other ISMIP6 models, but it would be useful to know how the control simulation compares to them as well

We will edit the text to mention results of the control simulations compared with other models participating in ISMIP6 (based on table B2 in Seroussi et al. 2020). However, this comparison will be added to section 4.5 'Comparison with other models'.

L160: is the reason for slow down at major ice shelves the lack of calving in this model?

L180: is it possible that the increase in floating area is causing an increase in buttressing. This is an artifact of models that fix the calving front, as discussed in Haseloff and Sergienko 2018, and may have considerable upstream effects on marine ice sheet stability

We will add discussion of how the fixed front calving may impact buttressing and grounding line dynamics, and slowdown in the control.

Figure 7: to me it would make more sense to have the AP panel with the same y-axis as the other panels to emphasize the very different scale of contribution, but can understand if the authors would prefer to keep it this way for legibility

Whilst we can see why this might be helpful for the reasons the reviewer highlights, as noted, at the same scale as the other plots the plot loses legibility. Moreover, positioned as it is on the lower row, we hope the difference in scale compared with the WAIS and EAIS plots will be more obvious to the reader. We will however emphasise the difference in scale on the figure caption.

L270: I am confused by this sentence since the choice of gamma_0 is independent of the choice of GCM. I can see how the result is dependent both on GCM and gamma_0, but not how one is dependent on the other. Perhaps more explanation is needed.

We have removed these lines following reviewer one comments.

L384: It is known that models with friction interpolated across the grounding line/zone are more sensitive and tend to have larger response to forcing than models with more conventional schemes (Tsai et al. 2015). It seems like that is probably playing a role here.

Edited line 385:
"…in BISICLES (Cornford et al., 2016). Previous studies have also highlighted that models using sub-grid interpolation schemes at the grounding line are more sensitive to forcing than conventional models (Tsai et al., 2015)."

L390: this is where it would be good to know how basal melt at the grounding line is treated in BISICLES, since this has a big influence on the model sensitivity as Seroussi and Morlighem showed.

edit line 390:
"core experiments for WAIS, which does not implement a sub-grid interpolation scheme for basal melting (Seroussi and Morlighem, 2018)."

L395: technically, the sliding scheme in BISICLES is closer to Tsai et al. 2015 than a purely Weertman sliding law, which may be a point of difference with ISSM.

This is a useful point, edited from 395:

"…than basal sliding law at comparable resolution (Cornford et al. 2020). Whilst BISICLES and ISSM have Weertman sliding over much of the domain, BISICLES uses a Tsai et al. (2015) type sliding law with Coulomb sliding close to the grounding line. This difference could contribute where higher sea level contributions are simulated in BISICLES. The mm-scale magnitude of this difference is comparable to that found in previous studies comparing Weertman-only and Tsai et al. (2015) type sliding laws (Nias et al., 2018, Bowan and Gudmundsson, 2024)"

Bowan paper https://tc.copernicus.org/articles/16/4291/2022/
Nias paper https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017GL076493

L475: I would hope that the statement "Data is available on request, and will be publicly available in due course" is merely a placeholder for the pre-print, since I'm not sure it is useful for a paper publishing important results contributing to widely used sea level projections. My suggestion would be to make these data available before the paper is published.

We are finalizing data for upload to Zenodo and will upload before submitting the final manuscript.