Dear Dr. van Calcar, Dear Authors,

Thank you very much for your replies to the reviewer comments. I would now like to ask you to implement the envisaged changes and submit a revised manuscript which will undergo another round of review, hopefully by the same reviewers. I have added some additional remarks below on some of the reviewer comments which might help in drafting the revised manuscript.

Thank you very much and I am looking forward to receiving your revised manuscript.

Best regards,

Johannes Sutter

One comment on your reply to R1:

b. The large uplift discrepancy of 250 m that you mention at l. 369 has almost no impact on the result. Explaining the little impact of this difference on SL contribution by invoking the fact that it arises relatively late in the simulation is a partially albeit not fully satisfactory explanation to me. I believe the (potentially too) high nonlinearities that are internal to the ice sheet model contribute to it.
Indeed, it is not only the fact that the uplift arises relatively late, but also the fact that the ice sheet is in a state of collapse. Whether this is due to larger sensitivity (non-linearity) or and of the other differences with published estimates ("differentinitialization method, basal melt approximation, and melt parametrization at the grounding line) is difficult to say, but we will extend the description as follows: "However, this uplift occurs mainly in the last 100 years. Furthermore, the effect on grounding line retreat is small because the grounding line is already retreating rapidly and the West Antarctic ice sheet in our simulation is in a phase of collapse. For another ice sheet simulation with different melt approximation or melt parametrization, the sensitivity to a similar uplift might be relatively larger."

I am not sure whether you are addressing the reviewer's comment with the addition: For another ice sheet simulation with different melt approximation or melt parametrization, the sensitivity to a similar uplift might be relatively larger." As the main question here is the non-linear response (ongoing MISI). Variations in melt rate approximations would probably not affect retreat much in case of an unforced self-sustained grl-retreat. R2 has a similar comment mentioning the similarities between grl positions in ASE. A model setup which is close to collapse but hasn't crossed a threshold yet might show a bigger imprint of bedrock response parameterizations.

Regarding the comment of R1:

5. I know very little about IPSL but CESM is known as a quite high-sensitivity model. I think this is a rather good thing since it leads to ice-sheet configurations that show large differences to present-day, thus exploring more diverse AIS configurations. I think this

could be highlighted in a discussion section, especially since it supports the overall message of the paper. I would very much appreciate more information about the GCM outputs in the supplemental material: e.g. their present-day bias compared to RACMO, MAR or ERA5, and their warming pattern (atmospheric and oceanic) by 2300.
We will add more information the manuscript about the GCMs. What is most relevant for the Antarctic simulations is the ocean warming. The average ocean temperature anomaly is colder for CESM than IPSL in the high emission scenario, but warmer in the low emission scenario (Extended Data Fig. 7 in van Calcar et al., 2024). In the supplemental materials of this manuscript, we will include a figure showing the average ocean temperature anomaly by 2300.

I would encourage you to also add a plot of SMB changes as CESM5-8.5 suggests extreme surface melt starting around 2150 CE and ocean temperature increase is also quite large.

Additionally (R1):

The authors are showing the ice volume that was lost (including ice grounded below sea level) and NOT the actual contribution to barystatic sea-level. If this is the case, this might of course partly resolve the doubts I raise in 2. and 3.
The ice volume that we show does not include ice grounded below sea level. The explanation corresponds to option a: The 3 to 4 meters sea level rise not only includes a large retreat of the Amundsen Sea Embayment, but also significant retreat in the Wilkes basin and some retreat of the Ross Ice Shelf. Furthermore, there is thinning of the WAIS. This is shown in Fig. 5 in the manuscript and will be discussed in the results.

It seems that in the projections the ice sheet begins to lose mass from the onset which would indicate an imprint of the model initialization on projected sea level. I am aware that this is not the main focus of this manuscript but for context I would invite you to elaborate on the model initialization and any persisting drift which might affect the results. I agree with the reviewer that the substantial SLE contributions under the SSP1-2.6 scenario merit discussion including a plot of the GCM forcing over time (ocean temp, smb, sat). This pertains also to R2's comment to extend the discussion on scenario uncertainties and how these could affect your suggested parametric range.

As per the comment by R1:

Just as for the lateral resolution, I wish to see at least some convergence/refinement arguments to fully approve a paper that recommends parameters to the community.

I would encourage you to consider this point and provide a meaningful convergence test for a selected experiment (i.e. SSP5-8.5).

In your response to the first comment of R2 you mention:

This effect is thus not related to differences between East and West Antarctica, but to the retreat within one basin in which ice loss is expected to occur in the near future regardless of which emission scenario is used …

Grl retreat in the Amundsen Sea sector is highly uncertain and the question whether we have crossed a threshold to MISI already is unresolved. Depending on the model, the initialization and the boundary condition you can either surmise stability or instability. I agree with the two reviewers that changing parameters depending on time scales might not be the ideal input for ice sheet modelers but providing a parameter range and associated uncertainties would be a very valuable contribution.

34. Fig. 5 & Supp. Figs. 2,4,5: It would be helpful to see some comments regarding the strikingly similar grounding line contours across all GIA models. Also, it would be helpful to show these plots for year 2300 where there will be actual differences in both ice thickness and grounding line extent in West Antarctica. We will explain in the text that the grounding line is similar for most of Antarctica because most of the AIS is stable over time. GIA only has an effect where there is mass loss, which is, for example, mainly in the Amundsen Sea Embayment in the case of the IPSL climate model. Supplemental Figure 3 shows the difference in terms of grounding line position, ice thickness and bedrock elevation between the ELRA300 and the two 3D Earth structures for the year 2300 when using IPSL. We will add the same figure but for the CESM climate model.

I think what the reviewer alludes to here is that the grounding line positions in the different cases are remarkably close to each other even for the retreating Pine Island and Thwaites glacier region. My suspicion is, that this is due to the scenario at hand in which MISI is already under way and the different modes of bedrock response cannot modulate the pace of retreat considerably. The impact of different bedrock response parameterisations is probably more pronounced in model setups close to the tipping point in which one considerably delays retreat or even prevents it and the other favours it due to a delayed bedrock rebound for example.

In response to comment of R2:

24. L241: how is "the Earth" different from "the mantle" in this context? Does it make sense to say "the region of the mantle….depends on the sensitivity of the Earth…"? The current sentence sounds like the mantle is sensitive to its own viscosity, which is confusing. To clarify, we will change this sentence to: "However, the actual region in the mantle that dominates the Earth's response depends on the sensitivity of the ice load to the viscosity in the sub-surface, which depends on the viscosity profile itself.

I think your modified sentence could cause confusion as it is unclear what you mean by "*depends on the sensitivity of the ice load*", do you mean "*depends on the sensitivity to ice load changes*"? Apologies if I misunderstand something here.