Review by Editor : Approximating 3D bedrock deformation in an Antarctic ice-sheet model for sea-level projections; van Calcar et al.

We sincerely thank the editor for the constructive comments. Please find our response in blue.

L115 I suggest to remove “and it shows notable discrepancies compared to the 3D GIA model it was benchmarked against” as such a statement would require a more robust analysis of these discrepancies. I assume all models of reduced complexity will lead to notable discrepancies compared to 3D GIA over long timescales.
We have followed this suggestion.

L115-116 “so compute” -> “computing”
We adjusted this.

L169 “a quadratic local law” -> “a quadratic local parameterization”
We adjusted this.

L171 I suggest to change “The model does simulate marine ice sheet instabilities, but not marine ice cliff instabilities” to “The model does not include a marine ice cliff instability parameterization”. MICI involves an explicit parameterization while MISI usually emerges as a result of the flow law, bedrock geometry etc.
We adjusted this.

L443: Regarding your response to the reviewer comment “L.360: PISM and Kori use parameterizations for sub-shelf and grounding-line melting that yield similar sensitivities to those used here. I don’t think this explains the large sensitivity of IMAU-ICE that you observe.”
Your response highlights the impact of initialization, which, as you correctly state, would lead to different basal melt rates even for the same basal melt parameterization. However, your explanation covers the imprint of initialization not the melt parameterization itself. If you want to stick to this response then you would have to remove the part of the sentence stating : “the basal melt scheme, and the melt parametrization at the grounding line”.

Indeed, the main impact is the initialisation and we removed “the basal melt scheme, and the melt parametrization at the grounding line” from the sentence.

So far, your paragraph explaining the high SLE in your simulation is very general. There is no information on your initial model state expect that present bedrock topography is used, so the reader cannot evaluate this aspect of your work. Did you e.g. use inversion or a freely evolving spinup? I would suggest either explicitly stating that you did not investigate the reasons for the high model sensitivity (and that it is not a focus of this paper) or providing a more elaborate analysis in the supplements by providing a figure of the initial model state (e.g. thickness differences to present day observations, model drift). If you use an initial state from a previous publication, then it would be helpful to state this somewhere in the methods. If not it, I would suggest adding a short paragraph on the model initialization and a figure showing the present day initial state in the supplements.

We added text in lines 371-372: "We did not investigate the sensitivity of IMAU-ICE to the initialisation and ice sheet model parameters as it is not the focus of this study".

Irrespective of this, the explanation on the initial state of the ice sheet was indeed missing, we apologize for that. We used the initial state from a previous publication so we have added the following to the text in lines 148-151:

"The basal friction and sub-shelf ocean temperature are calibrated using an inversion procedure over a period of 10000 years to obtain ice sheet velocities in equilibrium with the present-day bedrock topography taken from Bedmachine version 3 (Morlighem et al., 2020). The calibration is discussed in detail in van Calcar et al. (2025). The present-day ice surface topography and grounding line position follow from this calibration."

L448-450: the forcing e.g. in the Amundsen Sea used in your study could be compared to the forcings used e.g. in ISMIP6. This way you could either remove the sentence "Other climate models provide significantly different thermal forcings in terms of magnitude and spatial distribution, which might not be captured by the two climate models used in this study" or explicitly state the quantitative differences in the thermal forcing and whether they lead to the high SLE contributions in your simulations. I suggest either quantifying the differences in your forcing (e.g. the thermal forcing in our simulations is substantially higher compared to ..) or remove the sentence.

We agree that this sentence could be removed and did so.

L 450-455 I appreciate the modifications to this sentence, but I do not think you can state "retreat significantly whereas the rest of the West Antarctic Ice Sheet is relatively stable". Figure 4 shows a continued ice loss until the end of your simulation, i.e. ongoing retreat. Chances are that the remaining parts of the WAIS in SSP1-2.6 will melt as well over time would the simulation(s) run longer. My point being, that stability cannot be inferred from your figures.

Thank you for clarifying your point. We agree with you and acknowledge the importance of mentioning the time scale. We therefore changed the sentence you are referring to in line 375 to: "… and a scenario where the Thwaites and Pine Island glaciers retreat significantly whereas the retreat of the rest of the West Antarctic Ice Sheet is relatively small.”. Additionally, we clarify your point by adding the following sentence at the end of the paragraph in lines 377-378: “Since there is ongoing ice mass loss at 2500, the West Antarctic Ice Sheet might collapse even in this scenario on longer time scales.”

L457 “Together, these simulations capture both rapidly retreating and relatively stable drainage basins across different Antarctic regions.” I think this sentence is valid for many simulations as there will be always relatively stable drainage basins at least for the forcings and timescales considered here. The relevant basins here are however the major marine drainage basins which are associated with MISI. Those are all retreating or collapsing in your model ensemble for both chosen forcings.

We see your point and removed this sentence since we already give a detailed explanation of the important differences between the scenarios and basins in the sentences before.

L626 “A widely used mantle viscosity for a 1D Earth model is 1021 Pa · s (hereafter referred to as 1D21) (Gomez et al., 2015; Konrad et al., 2015; Rodehacke et al., 2020; Golledge et al., 2019)”
I am not sure whether the references are fitting here. E.g. Konrad et al 2015 don’t use a single viscosity value, Golledge et al 2015 don’t mention the viscosity they use, but they indeed use 10^21 in previous publications. Gomez et al use a range of viscosities. So, strictly, none of the references show that 1D21 is used widely. Some model studies make an effort to treat this viscosity as a free tuning parameter. I would suggest either referencing a study showing that 1D21 is widely used or referencing studies which explicitly use 1D21.

Thank you for pointing this out. To clarify, we have changed the following sentences in lines 67-73 in the introduction:

“Some current existing ice sheet projections that are derived in conjunction with a 1D GIA model or bed deformation model with a viscoelastic half-space use a homogeneous upper mantle viscosity of 10^{21} Pa · s (Rodehacke et al., 2020; Golledge et al., 2015; Klose et al., 2024). However, using a relatively high viscosity value, or a relaxation time of 3000 years, does not affect sea-level rise projections significantly compared with excluding bedrock deformation entirely, and it overestimates sea-level rise by up to 20% by the year 2500 compared with projections that use GIA models that consider a lower 1D viscosity (Konrad et al., 2015; Gomez et al., 2015) or a 3D Earth structure, which we refer to as 3D GIA models (van Calcar et al., 2025).”

We removed the sentence you refer to from the results section to prevent receptiveness.

Fig 5: It is good that you try to be consistent with the color-code for the grounding line and the timeseries plots but maybe cmaps such as cm_crameri.broc or cmocean.cm.tarn allow for readability of both grounding line and cmap. However, as one reviewer pointed out, it could ease interpretation and readability if you would use a different colormap for bedrock response and ice thickness changes. Also, currently only thickness changes of grounded ice at the end of the respective simulation are shown while the majority of the bedrock response is due to the thickness change over all areas (i.e. including previously ice covered regions).

Thank you for the suggestions and we have now changed the colorscheme of all the figures showing changes in bedrock elevation. Figures S4 and S5 show the ice thickness and bedrock elevation difference in the year 2300.