We would like to thank both the reviewers and the editor for their valuable feedback. Please find our response in blue.

Editor report

Dear Dr. van Calcar,

we have now received two reviews and both reviewers in principle suggest publication providing additional contextualization of your results. The revised manuscript will not be sent out to reviewers again but only undergo review by the editor.

Apart from the reviewer comments on the discussion of the model results there remain stylistic and formatting issues (see both reviewer's comments).

Currently, your manuscript and abstract do not clearly state that your experiments only consider ice sheet model projections where MISI is already under way. Thus, generalization of your conclusions to other modes of ice sheet response (e.g. linear trend, equilibrium) remains difficult. I think it is important for the reader to understand this from the get-go (including the abstract).

We agree that this can be stated more explicit in the text. We add to the abstract in lines 23-24: “Using a rigid Earth model, this results in ~3-7.5 m of barystatic sea level rise with significant retreat in various basins due to marine ice sheet instability.”

In the beginning of the result section at line 432, we added: “Depending on the emission scenario and climate model, we project ~3-7.5 m of barystatic sea level rise with significant retreat in various basins due to marine ice sheet instability.”

We add the following in the conclusion starting in line 730: “If a low emission pathway or a more muted dynamical response, for example a situation in which MISI is weak or does not progress substantially, were to lead to only limited grounding line retreat compared with our simulations, the influence of solid Earth deformation in that region would likely be minor, and the choice of Earth structure would have little effect on the results. Hence, the preferred ELRA and 1D GIA models are also expected to remain applicable.

While you added paragraphs (l367 - 371 & 571 - 578) discussing MISI I do not see “a scenario with slower retreat without a collapse” (l573). Figure 4 shows the experiments with ongoing MISI and the supplements don’t include the no-collapse case either. Maybe a change in the wording alongside quantification of grounding line retreat paint a clearer picture compared to general terms such as "collapse" vs. "slower retreat".

We define a collapse as the loss of most grounded ice in west Antarctica, including Amundsen Sea Embayment, Ross Sea sector and Weddell Sea sector, following Bamber et al. (2009). In our simulations of the low emission scenario, retreat in WAIS is limited to Pine Island and Thwaites glaciers, while the remainder of WAIS is still intact by 2500. This can be seen in Fig. 5. We therefore state in lines 729-730 that the West Antarctic Ice Sheet (WAIS) does not collapse in scenario SSP1-2.6 in our simulations and refer to Fig. 5. Even though there is ongoing MISI, there is no collapse of WAIS. Figure 4 does show a significant sea level contribution for the low emission scenario. This contribution comes from significant retreat of Thwaites and Wilkes glaciers and not from a collapse of the WAIS.

We made multiple changes in the manuscript to be more explicit about this.

To be more explicit about what is meant by "collapse", we changed the sentence starting in line 444 and show in green what we added: "To estimate the uncertainty associated with the magnitude of retreat, we include a scenario where the West Antarctic Ice Sheet collapses, meaning that most of the grounded ice has been lost (SSP5-8.5), and a scenario where the Thwaites and Pine Island glaciers retreat significantly whereas the rest of the West Antarctic Ice Sheet is relatively stable (SSP1-2.6)."

We removed the word "collapse" from the results section in line 585 and change the sentence to: "Furthermore, the effect on grounding line retreat is small because the grounding line is already retreating rapidly and the negative feedback from bedrock uplift is not strong enough to slow the rate of retreat."

We removed "faster retreat" and "slower retreat" from the sentence starting in line 728 and only mention the collapse, now that it is more clearly defined.

Furthermore, we changed the sentence starting in line 493 to: "For SSP1-2.6-IPSL, significant ice mass loss in the West Antarctic Ice Sheet only occurs in the Amundsen Sea Embayment, where grounding line retreat in ELRA300 is 150 km greater than 3D-weaker by 2500 (Fig. 5)."

We add references to the figures in the conclusion section in lines 733-734: "We include a scenario with fast retreat leading to a collapse of the West Antarctic Ice Sheet in 2500 (Supplementary Fig. 3, 7 and 8), and a scenario with slower retreat which does not lead to collapse (Fig. 5)."

Sentences such as l575-577 "If the low-emission scenario were to produce less grounding-line retreat than in our current simulations, the region over which ice mass loss occurs cannot be very different from the low emission case" are unclear and not sufficient to support your statement "Hence, the preferred ELRA and 1D GIA models are also expected to remain applicable."

We agree that that sentence is unclear. Please see the response to your first comment where we changed these sentences to clarify this.

All figures in the supplement show ice sheet configurations with substantial grounding line retreat of Thwaites and PIG and Figure 4 does not seem to include a scenario without ongoing MISI judging from the sea level contributions displayed. Please add the SSP1-2.6 experiments without collapse to the timeseries figures (at least Fig. 4). So far Fig. 5 seems to be the only figure showing results from SSP1-2.6 forcing albeit with ongoing MISI in WAIS and Wilkes.

The SSP1-2.6 scenario is included in all the time series: Fig. 4 a and d (solid lines), Fig. 4c and f, Fig. 6c-d, Fig. 7c-d.

References:

Bamber *et al.* Reassessment of the Potential Sea-Level Rise from a Collapse of the West Antarctic Ice Sheet. *Science,* **324**, 901-903 (2009).

Report 1

The paper by van Calcar et al. discusses how simple assumptions made in the Earth model can be used for ice-sheet model projections for Antarctica. The paper is well written (for most sections), and the authors have taken into account the comments and questions raised by the two previous reviewers. The authors explain their choice of parameters in detail and also the limitations of their results in terms of the ice-sheet model assumptions.

I have only a few smaller, more technical, comments that would increase the readability.
1) The usage of a hyphen is not consistent in the manuscript.

We have changed this throughout the manuscript.
2) Line 63: Does that mean your GIA model does not solve the sea-level equation?

Thank you for pointing this out. We used two different sets of settings for the GIA model, which we refer to as configuration 1 and 2. We did state the use for configuration 2 of the GIA model but not for configuration 1 of the GIA model. We now indicate this specifically in the method section and add a reference where the effect of solving the sea-level equation on sea-level projections is discussed in detail. We change the sentence in lines 205-208 to: "The model includes material compressibility but it does not solve the sea-level equation, and it does not account for rotational feedback or the migration of coastlines because these have a relatively minor effect on sea-level change compared with the effect of changes in grounded ice thickness (Milne et al., 1999; van Calcar et al., 2025)."

3) Line 68: Does the statement in this line mean that an upper mantle viscosity of 10^21 Pa s relates to a relaxation time of 3000 years? If so, it would be good to mention the viscosity already in the paragraph before, around line 56.

Yes, one could state that and we add in green in line 58:

Typically, ELRA is used with a uniform relaxation time of 3000 years and a flexural rigidity of 10^{25} kg m^2 s^{-2}, which roughly correspond to a mantle viscosity of 10^{21} Pa·s and a lithospheric thickness of 100 km, respectively.

4) Lines 94-95: Do both GIA models not include the sea-level equation? Your description in lines 102/103 sound like the sea-level equation is included.

We are using only one GIA model but with two different configurations. The configuration referred to in lines 94-95 in the previous manuscript does not solve the sea-level equation. We explain this now in the method section, following comment 2 of this review. The other configuration used to derive the LVELRA maps does solve the sea-level equation, which is mentioned in line 298. Since solving the sea-level equation in the GIA model has a relatively small effect on the resulting barystatic sea-level projections (van Calcar et al., 2025), we prefer to describe the solving of the sea-level equation in different configurations in the method section. To improve clarity in the introduction, we did add "barystatic" in front of "sea-level rise" in the lines 120, 133 and 135 to be explicit that this is the barystatic sea-level rise resulting from the ice-sheet model and not regional sea level from the GIA model.

5) Maybe good to mention LVELRA in point 2 as well, because this is the type of model you are using there, right?

We add LVELRA in brackets to point 2.

6) Lines 203/204: This is a repetition. You wrote the same thing in lines 168-170.

Thank you for pointing this out. We have removed the repetition and added the viscosity of the lower mantle in line 216.

7) Line 270ff: I'm not sure that I understand this right. I thought you used a 3D GIA model to derive a relation between relaxation times and viscosities that can be used to find laterally varying relaxation times. Or is this paragraph about the lithospheric thickness only? Please make this clearer.

This paragraph is about the adjustment of the ELRA model to be able to include a laterally varying relaxation time (which is then later derived from the 3D GIA model in section 3). To make this clear, we change line 331 to: "To include laterally varying relaxation times (derived in section 3) in the ELRA model, we made the relaxation time in Eq.5 a function of the 2D grid coordinates, such that τ becomes $\tau(i,j)$."

8) Lines 277-280: This should come earlier, preferably in the Introduction.

We agree and move the whole paragraph to line 91 in the introduction.

9) Line 359: Which simulation now? Just ice-sheet model simulation or coupled with a 3D GIA model?

We add in lines 428-429 that this is the coupled model.

10) Section 4: Please mention in the beginning that your sub-sections focus on the comparison to the 3D coupled model. Sub-section head 4.1 gives the reader the impression that you talk about the results for just this model set-up, but you also discuss the difference to the 3D coupled model set-up.

We adjusted the first paragraph of section 4 to be more explicit about which model is used:

"Sea-level rise over the next 500 years is projected using two different climate models, each under a high and a low emissions scenario. Projections using the ice-sheet model coupled with simple Earth models that adopt a uniform relaxation time, a laterally variable relaxation time, and a 1D Earth structure are compared to the average sea-level rise obtained using the coupled ice-sheet – 3D GIA model (configuration 1) with 3D-weaker and 3D-stronger Earth structures. The average barystatic sea-level rise computed by the ice-sheet model coupled to the 3D GIA model using the two different 3D Earth structures is referred to as 3D-Average."

11) Section 4.1 is a bit un-structured. You shortly discuss the results for ELRA3000 but then move to ELRA300 and discuss these results in detail. Later on, you mention why you do this, because ELRA300 is the best approximation for the 3D coupled model set-up. This information needs to come much earlier. You did this much better in the sub-sections 4.2 and 4.3.

We have moved the paragraph about the optimal choice of relaxation time straight after mentioning the research question in the beginning of the section.

12) Figure 7: Why not adding the LVELRA best case here as well?

We agree that this would be more consistent and added LVELRA best case to the figure and adjusted the caption accordingly.

Report 2

General comments

The authors have satisfactorily included some comments and answered some questions that were made in the first round of review. This is appreciated, and I believe that no technical obstacle stands in the way of a publication. Nonetheless, I want to stress that some comments were not addressed, although the authors said they would do it in their answers to the first round of review. This is easy to fix, and I provide a list below. It is however symptomatic of a larger problem. The re-submitted manuscript has many flaws, including:

- Many papers cited in the main text are absent from the reference section.
  We have checked all the references and added missing references of Weerdesteijn et al. (2023), Nield et al. (2018) and Swierczek-Jereczek et al. (2024) to the reference list.
- The uploaded track change document of the main manuscript is… not a track change document. This substantially complicates a second revision.
  We regret to read that the reviewer could not find the documents with tracked changes. There were two documents including all tracked changes, one with tracked changed for the reviewers and one with tracked changes for the editor comment. Before the manuscript was send to the reviewers, we confirmed with the journal that the reviewers would have access to both documents.
- The uploaded pdf of the supplements looks like a track change document, which shouldn't be the case.
  We have uploaded the tracked changes in the supplementary materials because these were important changes regarding some comments of the reviewers. The final upload will not have tracked changes.
- Many hyperlinks for figures and tables are missing.
  We found no statement of the journal requiring or recommending hyperlinks for figures and tables so we did not apply hyperlinks in the manuscript.
- A random line break appears at l. 118. Double parenthesis at l. 41. Odd punctuation l. 180 and l. 300.
  We fixed these mistakes.
- The use of the hyphen is very inconsistent. For instance, "ice-sheet model" vs. "ice sheet model"; "sea level rise" vs. "sea-level rise". This was already pointed out in the last review round. It is not limited to these examples and should be checked thoroughly.
  We have changed this throughout the manuscript.
- The caption of Tab. 1 and 2 of the supplements reads "RMSE [m]" although the values from the table look like they are unitless.
  We confirm that the caption is correct. The unit is in meter and not unitless.

Reading the document with better care, from the beginning until the end, would easily prevent such oversights, which I believe are not appropriate for a second round of revision. As a reviewer who should mostly focus on the scientific aspect of the paper, it is very frustrating to be forced to comment the form so much.

From the scientific side, I think the authors do not highlight a quite important message that results from their work. ELRA with homogeneous relaxation timescale never fits the transient GIA response, since there is always a drift in the error over time. Of course some parameter choices do better than others, but I feel like the final recommendation should be: "do not use ELRA for AIS projections (even with tau = 300 years!), since (1) a single relaxation timescale just can't fit things correctly as soon as the retreat occurs both in the west and the east, and (2) using LVELRA is equally simple while greatly reducing the error drift".

We agree that LVELRA would be preferred over ELRA300 but we would also like to provide a recommendation for a uniform relaxation time for ice sheet modellers that do not want to include LVELRA.

We add the following to the conclusion section:

Line 667: "If the ELRA model with a uniform relaxation time were to be used, we …"

Line 687: "We stress that the relatively high root mean square error of ELRA with a uniform relaxation time can be significantly reduced by using LVELRA and 1D GIA models, which are the preferred models."

The authors don't mention, in the introduction, that the deformational responsehas an elastic and a viscous component. I believe this is misleading for the result interpretation because ELRA and LVELRA are tuned to fit the elastic + viscous displacement of the 3D GIA model, although their underlying relaxation equation is only adequate for the viscous response. This should be discussed more extensively, especially because the elastic response is important on such short projection horizons. As you mention in l. 501-504, this is the main reason why the 1D GIA model gives a better uplift pattern than LVELRA. This points to an obvious limitation of the present work: estimating tau while accounting for the elastic response (simple models exist for that) would give better results and should be mentioned as an outlook.

We agree that this could be stressed more clearly in the text. We add in line 61: "Furthermore, ELRA includes the flexural elastic response of the lithosphere, but it neglects the elastic part of the viscoelastic response."

Line 518: "Furthermore, the elastic response of the upper mantle is not taken into account in the ELRA model, which could lead to an underestimation of uplift compared to the viscoelastic mantle response in the 3D model."

We extended the description of the effect of flexural rigidity and add explicitly how elastic behaviour is taken into account, starting in line 525: "In the ELRA model, the elastic response of the lithosphere is computed using the flexural rigidity of the lithosphere. The lithospheric beneath the West Antarctic Ice Sheet can be as thin as tens of kilometers (Lloyd et al., 2019). We therefore test the impact of using a flexural rigidity of $1.92 \cdot 10^{24}$ $km \cdot m^2 /s^2$, which roughly corresponds to a lithospheric thickness of 60 km. The combination of a lower flexural rigidity and higher relaxation time yields a similar result to the combination of a higher flexural rigidity and somewhat lower relaxation time. Therefore, decreasing the lithospheric thickness does not improve the fit of ELRA to the 3D Average (Supplementary Fig. 6)."

I wish the best of luck to the authors for the last modifications.

Jan Swierczek-Jereczek

Unaddressed comments of the previous round of revision

- L. 90: You said you would mention that Bueler et al. (2007) and Swierczek-Jereczek et al. (2024) capture the dependence on the load wavelength… but you don't do so explicitly. The same applies for the evolving and heterogeneous sea level (that is coupled to deformation) in the case of Coulon et al. (2021) and Swierczek-Jereczek et al. (2024).
  We acknowledged the advantages of the model presented in Swierczek-Jereczek et al. (2024) over ELRA in the text in line 112. We have added in green the description of the model: "A computationally efficient Earth model based on fast Fourier transforms has been developed that approximates lateral variations in mantle viscosity and lithospheric thickness in the Earth structure and takes into account the effect of a spatially and time varying sea level on deformation (Swierczek-Jereczek et al., 2024)."

  We have added that the dependence on the wavelength of the load is one of the advantages of FastIsostasy over ELRA and added in line 114: "Coulon et al. (2021) coupled ELRA with a gravitationally consistent geoid calculation so compute near-field relative sea-level changes. Furthermore, ELRA uses a single relaxation time and is therefore independent of load wavelength, but the framework could in principle be extended to a scale-dependent formulation where the relaxation time becomes a function of wavenumber."
  Furthermore, I don't think you can consider that the errors over a full glacial cycle, presented in Swierczek-Jereczek et al. (2024), are "notable" since (1) the maximal error is lower than with a 1D GIA model and (2) you are not making any comparison to FastIsostasy in the present paper.
  Concerning point (1), Fastisostasy has lower errors than a 1D GIA model for part of the glacial cycle, but for other parts of the cycle, the 1D GIA model has lower

erros. Our manuscript states in lines 114-115 that the error of FastIsostasy is notable compared to the 3D GIA model, which is still valid. Concerning point (2), we explicitly state that the error relates to modelling a glacial cycle rather than projections (line 113) because FastIsostasy have not been used in projections yet.

As I stated in the previous review round, you have such an easy justification for your work, which does not need to invoke the specificities of other GIA models: "going from ELRA to LVELRA is extremely simple in terms of code adaptation". Why not just focus on this appealing aspect of your work to motivate it?

The reviewer stated in the first round that the description of existing models with lateral variability needs more nuance and context and we agree that it is a valuable addition to discuss efforts to overcome computational infeasibility. We therefore implemented the suggestions in round 1 of the reviewer themselves to not only focus on ELRA and LVELRA in the introduction, but to discuss also other models (Swierczek-Jereczek et al., 2024; Nield et al., 2018; Book et al., 2022; Weerdesteijn et al., 2023). We still highlight the simple adaptation of LVELRA in multiple places in the manuscript, for example in the conclusion section in line 703.

- L. 199: You don't specify that the layers refer to layers where the solid-Earth parameters can vary along the depth, and that this is not the vertical resolution.

  We mention in the text: "A high resolution area is defined over Antarctica with a horizontal and vertical grid resolution of 30 km wide and deep between the surface and 670 km depth."

- L. 255: Be consistent with your index notation (either with comma or without). We added a comma.
- L. 271: A field tau should either be tau_ij if discrete or tau(x, y) if continuous. Since tau is derived from a spatially varying viscosity, we chose tau(x,y). However, for this context mentioning the 2D grid we switched to tau_ij.
- You don't include the problematic use of a very low viscosity in your 1D GIA model, for instance when representing the global response to ocean load. This is already stated in lines 724-726: "Furthermore, using the suggested upper mantle viscosity would lead to an overestimation of the response to changes in global ocean loading and to changes in ice loading in East Antarctica over millennial timescales."
- The research questions mentioned in the introduction are inconsistently cited in the conclusion. We updated all research questions to the correct version.

Specific comments

- L. 65-66, l. 484-485, l. 491-492: Golledge, Rodehacke, Klose and Kachuck use the Lingle-Clark-Bueler approach, which uses a single viscosity as parameter but is not a 1D GIA model. However, the sentences are confusing and should be rephrased.
  Thank you for pointing this out. We have now separated these two types of models as "1D GIA model" and "bed deformation model with a viscoelastic half-space using a homogeneous upper mantle viscosity" in lines 75 and 605-614 (corresponding to the parts of the text previously in lines L. 65-66, l. 484-485, l. 491-492).
- L. 132: "Basal melt at the ice shelf is computed using the local Favier quadratic method and the surface mass balance is computed using a temperature and radiation parametrization (Favier et al., 2019; Berends et al., 2022)." → "Sub-shelf melting follows a quadratic local law (Favier et al., 2019) and the surface mass balance is computed using a temperature and radiation parametrization (Berends et al., 2022)."
  We switched the location of the reference and changed the name of the basal sliding law.
- L. 135: Does "ice shelf instabilities" refer to marine ice sheet instability? It would make much more sense in this sentence.
  We changed "shelf" to "sheet".
- L. 142: "Besides the stabilising effect of bedrock deformation on ice-sheet evolution, there is also a sea surface height component, and together these comprise the sea-level feedback. The loss of gravitation from the ice sheet causes a local sea-level drop…". This reads quite bad (e.g. there is no "loss of gravitation" but rather a reduced gravitational pull), please rephrase.
  Rephrased according to your suggestion.
- L. 202-204: You already say that above. Please remove redundancy.
  We have removed the duplicated information.
- L. 242: "Earth properties are" → "viscous response is"
  We changed it to "bedrock response" since the full bedrock response is simplified to the ELRA approximation and not just the viscous response.
- L. 302: Isn't the uplift rate a direct output of your model, which computes it more accurately than what you do based on the displacement curve?
  The displacement curve follows directly from the uplift rate output by the model. To make this more explicit, we change the sentence in lines 353-354 to: "For each simulation, the resulting displacement over time for each surface load/Earth model combination is computed, yielding a displacement curve."

- 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.
  Different initialisations yield distinct ice thickness fields, grounding-line configurations, and stress regimes. Basal melt and grounding-line parameterisations depend on local ice thickness, shelf geometry, and basal traction. Different initialisations place the model in different parts of this parameter space, so the same perturbation (e.g., a given melt or grounding-line parametrisation) can produce different dynamical outcomes. Because basal-melt and grounding-line parameterisations operate on these background states, identical parameter choices between models can induce different perturbations to melt, buttressing and grounding-line migration.
- L. 427: I would mention earlier that ELRA300 minimizes the RMSE. This way, the reader understands much better why you spend so many lines on describing this specific parameter realization.
  We have moved the paragraph about the optimal choice of relaxation time to straight after mentioning the research question in the beginning of the section.
- Fig. 5 (and all other figures with the same format): the figure would be much more legible if you would use a different colour bar for displacement and ice thickness anomaly.
  We understand the reviewer's point and would prefer different colormaps for ice thickness and bedrock elevation as well. However, we also wanted to keep the grounding line in the same color on both the ice thickness and bedrock elevation maps, and we could not find a second colormap in which the grounding lines are distinguishable and colorblind proof. The colors of the grounding line are now also consistent with the colors of the time series (3D stronger is pink, 3D weaker is orange and ELRA300 is green). We therefore chose to use the same colormap.