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.
- The uploaded track change document of the main manuscript is… not a track change document. This substantially complicates a second revision.
- The uploaded pdf of the supplements looks like a track change document, which shouldn't be the case.
- Many hyperlinks for figures and tables are missing.
- A random line break appears at l. 118. Double parenthesis at l. 41. Odd punctuation l. 180 and l. 300.
- 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.
- The caption of Tab. 1 and 2 of the supplements reads “RMSE [m]” although the values from the table look like they are 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”.

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.

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). 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. 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?
- 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.
- L. 255: Be consistent with your index notation (either with comma or without).
- L. 271: A field tau should either be tau_ij if discrete or tau(x, y) if continuous.
- 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.
- The research questions mentioned in the introduction are inconsistently cited in the conclusion.

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.
- 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)."

- L. 135: Does “ice shelf instabilities” refer to marine ice sheet instability? It would make much more sense in this sentence.
- 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.
- L. 202-204: You already say that above. Please remove redundancy.
- L. 242: “Earth properties are” → “viscous response is”
- 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?
- 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.
- 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.
- 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.