

Answer to EGU sphere-2026-2640-RC2

We thank Dr. Sasgen for his careful and constructive reviews. The comments are well-targeted and addressing them will clearly improve the manuscript. Below, the reviewer's comments are reproduced in blue and our answers follow in black.

The manuscript "Fast approximation of Antarctica's GIA response to future ice melt with 3D Earth structure" (egusphere-2026-2640) presents coupled ice sheet/GIA simulations for the next five centuries. Its aim is to evaluate the performance of a regional 2D GIA representation compared to a full 3D GIA model solution, particularly in the context of the stark lateral differences in Earth structure present in Antarctica. The model setup consists of the 3D model Seakon (used as reference) and FastIsostasy, a computationally far less expensive approximation, both coupled to the thermomechanical ice sheet / ice shelf model PSUICE3D. The simulations follow ISMIP6-type experiments. Earth model parameters for Antarctica are derived from a published regional seismic velocity solution, converted to viscosity using two different established methods to construct a weak and a strong Earth structure model. The approximation's performance is evaluated against Seakon using several metrics, including ice thickness, grounding line position, vertical displacement, and sea-surface elevation, at various stages of the future evolution. The authors claim that FastIsostasy approximates the GIA response sufficiently well, and better than even simpler realizations, opening a pathway for improved ice sheet projections in ISMIP7-like experiments.

The manuscript is clear, well-written, and pleasant to read. The figures are of high quality, even though they do not always present the results in the best way (see comments below). The study is novel and timely, in the sense that the implementation of GIA responses in ice sheet models is still far from common. The study is carefully designed to answer the targeted questions; the models are state-of-the-art, and the nesting of the Antarctic Earth structure within the global models is nicely done. On a personal note, I think the improvement of FastIsostasy over the ELRA models is not as large as expected. And I think the authors could highlight the advantages of FastIsostasy more clearly (e.g., sea-level representation). However, I would like to see an improvement to Figures 2 and 3 (see major comments), and would also like the authors to consider my suggestions for the other figures. Finally, the authors should be explicitly mentioned that evaluation is specific to this time frame, range of viscosities and not the least the ice sheet model / glacial history.

I consider these changes minor, since they require no new computations or additional checks. After these changes are made, I think the paper is acceptable for publication in The Cryosphere.

Best regards,

Ingo Sasgen

Major comments

[L255ff] Figures 2 and 3. It is recognized that this paper focusses on the difference between regional 2D versus 3D coupled simulations. However, without a map of the absolute changes in ice thickness, or a relative metric, the figures become far less useful; the reader can't judge whether the 2D approximation is justified, given the magnitude of load changes. Second, it is impossible to judge whether 2D really fails for the MICI scenario or whether it is just a large load change overall. Also here,

a relative metric could help for the intercomparison. Helpful would also be to zoom in on the Amundsen Sea Embayment and the Wilkes Land coastal sector.

We currently show absolute changes in ice thickness from Seakon simulations in Fig. S4-S7, but we agree with the reviewer that adding rows presenting absolute ice thickness changes in Figs. 2 & 3 will help readers better understand panels a-f. Thus, we will add two additional rows to both Figs. 2 & 3 to present absolute changes in ice thickness for both the Strong and Weak Earth models.

Also, do you really need the Zwally basins, in particular without labeling...?

We will add labels to notable basins (e.g., Thwaites, Pine Island, Wilkes) in Fig. 2a to make the basins more useful to the reader.

Further, there is a performance degradation for FastIso and weak Earth structures & MICI concerning the grounding line position (Fig. 4). This is invisible in Fig. 2; so I think it is safe to remove all grounding line positions from Fig. 2 and 3. Also, is this difference significant; if not, the conclusion would be that the ELRA approximations would be passable as well... The very critical perspective one might take on the results is that, for these projections, FastIso does not significantly outperform ELRA500.

Compared to the Seakon simulations under the RCP8.5-MICI scenario, the simulation with FastIsostasy and weak Earth structure does perform worse than the simulation with FastIsostasy and strong Earth structure. However, the simulations with FastIsostasy and weak Earth structure under RCP8.5-MICI (solid red line in Fig. 4) still predict mean ice thicknesses and grounded ice area more closely to the Seakon simulations compared to the ELRA500 and ELRA3000 simulations (dotted red line in Fig. 4). Nonetheless, as the reviewer notes, FastIsostasy does not outperform ELRA500 for these projections as much as one may expect. Thus, we will add text to further highlight the fact that ELRA500 is not a bad choice for such simulations, just not the optimal choice.

We prefer to leave the grounding lines in Figs. 2-3 because they provide insight into the extent of the ice sheet at each time step.

[L285ff] Figures 5 and 6. It would be helpful to show relative sea-level as well. After all, this is the relevant quantity for the grounding line dynamics. And it would nicely show how vertical displacement controls sea-level fall and acts as a stabilization for the weak Earth model. Thus it also shows that, although FastIso may fail to represent the sea surface well, it represents the relative sea-level well. Finally, it would also be great to show rates as well, particularly for displacement, in order to put them in context with uplift rates at GNSS stations, possibly selecting one or two locations.

Relative sea level change is defined as the difference between sea surface elevation change and vertical bedrock change, both of which are shown in Figs. 5-6. Thus, showing both variables in Figs. 5-6 allows readers to better understand the individual contributions of sea surface elevation change and vertical bedrock change to relative sea level change. We include plots of RSL in Fig. S11 and S13, and we will refer to the supplementary figures more in the main text.

Given the focus on comparing the performance of FastIsostasy and Seakon for future predictions of AIS change, it is outside the scope of this manuscript to compare predicted vertical displacement rates with modern uplift rates observed at GNSS stations, but we will provide a repository of modeling results so that these can be calculated as needed.

[L310ff] The introduction mentions the stabilizing effect of rapid rebound on ice-dynamic retreat. Yet, the barystatic curve for the strong Earth model appears to be lower in 2500 than the one for the weak

Earth model. Please comment on this. I am pretty sure this is simply due to the limited projection period (one problem of the study design in general that the interesting effects like stabilization and tipping will only appear on longer time scales). In addition, FastIso seems to systematically perform worse for the low-viscosity cases (e.g. Figure 4, grounding line; Figure 6, local sea surface height) – can you explain why? A final comment: the improvement with FastIso may be considered not that significant compared to a tuned ELRA model. Considering all the loose ends in ice sheet modelling and the uncertainties in the forcing, I am afraid that modelers will stick with ELRA even if this means that sea level cannot be represented properly. However, making your code available will certainly help to improve coupled runs.

The barystatic contributions for simulations with the weak and strong Earth models are comparable in 2500 (see Fig. 7.a-b, both give about 16 m of sea level change) because both simulations show similar retreat of the AIS, with complete WAIS collapse and substantial retreat in the marine-based sectors of East Antarctica. Rapid rebound has a transient stabilizing effect on ice-dynamic retreat. Under the high-emissions RCP8.5-MICR scenario, ice retreat is faster than bedrock uplift in simulations with both the weak and strong Earth models, leading to comparable barystatic contributions by 2500. A more extensive discussion of how ice retreat outpaces bedrock uplift can be found in Gomez et al. (2024); therefore, to keep this manuscript focused on the FastIsostasy versus Seakon benchmark, we would prefer not to expand upon this point further in the main text.

The reviewer is right to point out that FastIsostasy systematically performs worse for the low viscosity case. When looking solely at the GIA response in relative terms, the picture becomes more nuanced. Compare, for instance, the displacement at year 2500 for the point sampled in the ASE (light orange curves Figs. 5.c and 6.c): $120/150 = 0.8$ for the strong Earth, and $405/475 = 0.85$ for the weak one, thus both yielding similar values. However, since the weak Earth yields larger absolute values, this impacts the ice dynamics more significantly. We will add a few sentences to the manuscript to discuss this further and thank the reviewer for drawing our attention to this point.

Minor and Technical Comments

[L3] GIA control much weaker than ocean forcing or bedrock parameters. Maybe consider writing „important“.

We will rephrase to say “important control” rather than “strong control”.

[L6] “include realistic spatial variations in solid Earth structure”. And gravitational and rotational variations?

Here we are highlighting that spatial variations in solid Earth structure must be captured, necessitating the use of 3-D GIA models. We do not think gravitational and rotational variations should be mentioned in the abstract, since they are not the primary focus of our study. However, we mention both of them in the remainder of the paper.

[L8] Actually, when checking I was surprised to see that the majority of ISMIP6 models use a rigid bed approximation; nonetheless, you should introduce the more advanced approximations like ELRA you use later as intermediate step between a rigid Earth and a 1D GIA model.

At l. 8 we mention “simple GIA models”, which includes ELRA. We would like to avoid introducing technical terminology in the abstract and therefore prefer to keep the explicit introduction of ELRA, along with ELVA at l. 35-39, which is very early in the manuscript nonetheless.

[L13] simpler – “overly simplified” is subjective and is part of the results of this paper.

We will replace “overly simplified” with “simpler”.

[L19] I think it would be fair to include some climatic drivers here, which exert the dominant control and are responsible for a large part of the uncertainty.

We will revise this sentence to state that “AIS evolution will depend on a variety of climatic, oceanic, and atmospheric factors, in addition to bedrock elevation changes beneath the ice and sea level change at the grounding line...”.

[L25] Across → underneath; also, please mention values for this statement already here: 10^{17} Pa·s to 10^{22} Pa·s?

We will switch across the underneath and mention values for viscosity.

[L26] Absolute values would be more helpful.

We will state absolute values.

[L32] Please add: <https://doi.org/10.1038/s43247-026-03738-y>

Thanks for the recommendation. We will add reference to Sasgen et al. (2026).

[L44] Maybe add: „although computationally less expensive“, or similar.

In L43, we specify “...at a low computational cost..”

[L49] models → modelling

Will switch models to modelling.

[L56] and radially? You explicitly mentioned this before. Also please very briefly describe how the approximation works.

FastIsostasy lumps mantle layers into a single layer, resulting in a single viscosity for the entire mantle. In the intro to the Methods, we want to focus on highlighting the fact that FastIsostasy captures lateral-variations in Earth structure, which other simple GIA models fail to do. We discuss the 2-D Earth models used in FastIsostasy more extensively in section 2.3.3.

[L60] It might be useful to mention that you use the PennState ice sheet model already here, to highlight that that you are really coupling two numerical models.

We will specify “iteratively coupled PSUICE3D dynamic ice sheet - GIA simulations” here.

[L73] Please mention that you start from a quasi-equilibrium state of the GIA model in 1950, and that the deformational response from earlier retreat is neglected (if this is the case).

We will state that GIA response from ice mass changes prior to 1950 are neglected.

[L73] Changes in bedrock → vertical deformation and sea surface elevation – the quantities you actually show in the figures.

“Changes in bedrock elevation” and “vertical bedrock displacement” are commonly used interchangeably.

[L83] I know the effect won't be large – but can you mention how you deal with Greenland Ice Sheet mass loss, or give a justification for neglecting it?

We will specify that we neglect Greenland Ice Sheet mass loss because its contributions to sea levels around Antarctica will be small compared to GIA effects associated with local ice loss (Hay et al., 2017; Lucas et al., 2026).

[L109] border à domain margin?

We will change “border” to “domain margin”.

[L116] What grid spacing are you choosing for the FastIso runs? 10 km, as for the reference 3D GIA model and PSUICE3D?

Thanks for catching the missing information. FastIsostasy runs on 10 km horizontal resolution, which we will clearly state in the revised version of the manuscript.

[L129ff] It is a bit awkward to state the elastic lithosphere thickness explicitly and then fall back to seismic velocity anomalies for the upper mantle structure. Please resolve.

We do not think that this is an awkward statement, since the lithosphere thickness is also inferred via the seismic velocity anomalies.

[L132] Could you please state the extent of these “localized regions”? Are these small patches of super-low viscosity within the rift system of West Antarctica? I think this is important for the GNSS interpretation.

The goal of this manuscript is not focused on GNSS interpretation. Consistent with Lucas et al. (2025), localized regions of low-viscosity upper mantle are found within regions of the West Antarctic Rift System.

[L136ff] Are these average upper-mantle viscosities? Could there even be a lower-viscosity asthenosphere layer?

As specified, these upper mantle viscosities are inferred from GNSS observations.

[L143] time → times

Will change “time” to “times”.

[L144] ... fascinating and a nightmare from a modelling standpoint ... (comment)

Yes! We agree.

[L149] Please add: Konrad et al., 2015

Will add reference to Konrad et al. (2015).

[L150-156] Please shorten to 30-50%. A bit generic and repetitive.

We will shorten this block of text.

[L156] Please mention already here that you consider these models as plausible upper and lower bounds.

These Earth models should not necessarily be considered plausible upper and lower bounds, they are instead, weaker and stronger possible Earth models. We will add additional details to section 2.3 detailing why these Earth models cannot be considered end-members.

[L157] I think it would be very helpful to make the 3D Earth structure models available, and add them to the data sets of plausible viscosity models for Antarctica.

The 3-D Earth structure models are available upon request. Furthermore, the 3-D viscosity inference based on GLAD-M25 (Lei et al., 2020) is already available from Lloyd et al. (2024), and the viscosity inferences from ANT-20 are available from Hazzard et al. (2024).

[L164] GLAD-M25 → global seismic shear-wave speed model GLAD-M25

We will make this change.

[Table 1] Please state, for completeness, what happens below 800 km.

We will add a row to the table to specify what happens below 800 km, where we just adopt the viscosity inference based on GLAD-M25 from Lloyd et al. (2024).

[L206] I think the approach of performing the lumping based on minimizing deviations from the 3D reference is a practical one; using the minimum viscosity within 150 km of the LAB is somewhat physically justified. However, it was based on the specific weak and strong Earth models. Do you think this is transferable to other viscosity profiles and simulations?

We do think that this approach will be transferable to other viscosity profiles and simulations and plan to test this approach more extensively in future work.

The purpose of using two very distinct Earth models aims at this: the lumping yields good results in both cases although the upper mantle viscosities show very large discrepancies between the weak and the strong case. We will make this more explicit in the discussion.

[Figure 1] Would it be possible to compress the color scale at high values for lithosphere thickness, to allow a finer color grading for the thinner values? It would be interesting to see more detail for West Antarctica and along the continental margin.

We agree and will simply add more colours to the colour bar (uniformly, for the sake of simplicity).

[L215] Do any of these simulations map onto ISMIP6 experiments? It would be impactful to place the results within the wider modelling community's efforts.

We are not sure what the reviewer means by “map onto ISMIP6 experiments”. The forcing used here corresponds to that of ISMIP6, thus providing comparable setups. We stress that our forcing from year 2300 until 2500 is held constant at the 2300 value and therefore represents a simple extension in time of the forcing, outside of the ISMIP6 scope.

[L216] A bit difficult to understand, maybe: “...producing a spatially large and complex GIA response that is more difficult to approximate by a 2-D regional GIA model; an extreme case which specifically tests the performance of FastIsostasy versus Seakon.” You could also give some more details on why FastIsostasy may fail in the MICI case (forebulge, domain boundary, rotation, etc.).

We will make this sentence more clear by taking the rewording suggestion.

[L233] Please add information on the initialization of the solid Earth models.

All GIA models assume zero imbalance between ice load and solid Earth at the start of the simulation in the year 1950. Of course, this is a simplification, which omits the ongoing GIA response from the last deglaciation and Holocene, and we will state this in the revised version of the manuscript.

[L236] Add: Antarctic → future Antarctic

We will make this wording change.

[L237] Predictions → evolution.

We will make this wording change.

[L239] Predicted → represented.

We will make this wording change.

[L242] “Average differences in ice thickness” Please explain how this metric is calculated.

We will specify that average differences in ice thickness are computed by calculating the differences in grounded ice thickness across the continent between the FastIsostasy and Seakon simulations and then averaging.

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