

Answer to EGU sphere-2026-2640-RC1

We thank Dr. King 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 authors present an analysis of the FastIsostasy code against a fully featured 3D GIA code, Seakon, in the context of an iteratively-coupled ice sheet/GIA model. Such a code would provide practical benefit to sea level projection realism and is a very worthy topic with potential for significant downstream impact. The work finds that these two models are in pretty close agreement, happily so, and certainly much better agreement than with ELRA style models. The work is well suited to TC and will be of wide interest to those working with ice sheet models, especially given the speed of the FastIsostasy model and its potential for coupling to ice sheet models. The paper is clearly written and makes good use of figures.

I don't have any major concerns, although I think there are things that do need revising.

Section 2.3.2 outlines the weak earth model which reaches to just below 10^{19} Pa s in regions where the geodetic data have suggested more like 3×10^{18} . I think this section and 2.3.1 should summarise the range of upper mantle viscosities and here or in the discussion note that the model doesn't go as low as the obs may suggest and what issue that may cause. This is important for the ASE, especially since most of the ice loss will occur there this century. That is, the testing in this paper doesn't cover the viscosities most relevant to the near future. Out of interest, is this a technical restriction in Seakon or FastIsostasy or both that prevented going to lower viscosities?

We thank the reviewer for raising this point, which we believe stems from Fig. 1 and which we should have made clearer in the text.

Fig. 1 shows the *effective 2-D* viscosity used by FastIsostasy, not the 3-D fields. The 3-D weak Earth model used by Seakon does reach values of about 10^{18} Pa s in the Amundsen Sea Embayment (ASE), consistent with the geodetic inferences cited by the reviewer; this is visible in Fig. S1 but is never stated in the main text. We will add explicit statements of the viscosity ranges to Sects. 2.3.1 and 2.3.2.

The reviewer is nonetheless correct that the *effective 2-D* viscosity field used by FastIsostasy does not reach as low as the observations may suggest. This is not a consequence of the lumping method of Appendix A, which selects the minimum viscosity over 150 km below the LAB and would preserve the low ASE viscosity values, but of the two corrections applied afterwards (Appendix C of Swierczek-Jereczek et al., 2024). This step is insufficiently described in the present manuscript and we will add the corresponding reference and a short explanation to Sect. 2.3.3.

The consequence is that Seakon predicts a somewhat faster uplift than FastIsostasy over the ASE, as expected from the above (Fig. 6.c, orange curve). The reduction of a 3-D problem to a 2-D one, and the associated viscosity lumping, is indeed the main source of discrepancy between the two models. We stress, however, that this remains modest on the timescales the reviewer is concerned with: in 2150 the mean ice thickness difference between the FastIsostasy and Seakon coupled simulations is 9 m under RCP8.5-MICR and only 0.7 m and 0.5 m under RCP8.5-MISI and RCP2.6-MISI, respectively. We will make this point explicitly in the discussion.

Finally, to answer the reviewer's question: low viscosities are not a technical restriction of either model. They imply smaller time steps and, therefore, longer computation times, but no loss of accuracy or stability. The values used here follow from the two Earth models adopted, not from a numerical limitation.

Section 3.1 . The focus in the text is mean differences, but I think $\max|\Delta|$ needs reporting also. These are much larger. Figure 4 could have 2 further panels to show maximum differences. This applies to all subsequent discussions. I presume the means are spatial means computed over the common grounded ice sheet? Further, the regions where the differences are largest is also worth mentioning. They are all at the grounding line. This difference may affect different types of ice sheet models in different ways., notably those that resolve grounding line regions differently.

We agree that maximum absolute differences should be reported alongside the means, and we will add a panel to Fig. 4 showing $\max|\Delta|$ for ice thickness. We note that we already report both metrics for RSL in Sect. 3.2 (mean and maximal absolute errors of about 4 m and 22 m for the strong Earth model), and we will report both consistently for vertical displacement and sea surface height as well.

The reviewer's presumption is correct: the reported values are spatial means computed over the extent of grounded ice. We will state this explicitly in Sect. 3.1 rather than leaving it implicit.

We will also add a sentence noting that the largest differences are systematically located near the grounding line, where ice thickness changes are largest and where bedrock and sea-surface changes feed back most directly on the flotation criterion. As the reviewer points out, this has implications beyond our setup: ice-sheet models differ substantially in their grounding-line treatment (horizontal resolution, sub-grid grounding-line parameterisations, flux conditions), so the same GIA discrepancy may propagate differently depending on the host model. We will make this caveat explicit.

The quite bold statement on L284 that more complex GIA models than FastIsostasy probably needs the caveat that it applies to the class of ice sheet model used here and within the range of mantle viscosities tested.

We agree and will nuance the statement. The claim is supported by the present setup but is not a multimodel assessment, and we will restrict it accordingly, e.g.: "For the class of ice-sheet model and the range of upper mantle viscosities considered here, improving the representation of the GIA response does not appear to require GIA models more complex than FastIsostasy but rather improved constraints on solid Earth properties."

We note that the range of sub-lithospheric upper mantle viscosities spanned by the strong and weak Earth models is nonetheless broad (Fig. S1), and that the two models were chosen to represent a wide range of plausible mantle viscosities inferred from geodetic observations and seismic imaging in Antarctica.

As a separate note, I wonder if the bedrock topography offshore, which controls warm water pathways, is also little different between the two approaches. This isn't relevant to the ice-GIA coupling, but it is for the ocean-ice-GIA system. That may be something to comment on in discussion if it is trivial to compute.

We agree that this would be an interesting aspect to quantify, since GIA has been shown to modulate the access of warm circumpolar deepwater via ocean gateways (Kreuzer et al., 2026). The subshelf melt does not depend on GIA in the present setting, but this dependency should be increasingly included in ice-sheet models.

L58 benchmarks are referred to lacks an explicit reference to Swierczek-Jereczek et al., 2024 and I think it should be cited again in this sentence to avoid confusion. I think some quantification of "performed well in benchmarks" would be appropriate for the introduction.

The reviewer is right: we will include both a reference to Swierczek-Jereczek et al. (2024), and a few illustrative numbers to support the statement.

More minor remarks:

L148 it may not be necessary to modify the example of Wilkes Subglacial Basin, but I note the paper by Hansen and Emry suggests there could be lower-viscosity mantle in this region. <https://www.nature.com/articles/s43247-025-02140-4>

We thank the reviewer for raising this point. Hansen and Emry (2025) do indeed report slow vertically polarized shear wave speeds beneath portions of Wilkes Subglacial Basin and along some areas of the Wilkes Land coast, extending inland beneath the Aurora Subglacial Basin. However, there are some points of concern regarding this tomographic model, including these features. Thus, we recommend caution when considering GIA implications regarding this structure.

The most relevant aspect here is that the Hansen and Emry (2025) model shows large variations in velocity over short spatial distances that are dominated by elongated elliptical features resembling surface wave sensitivity kernels (*their supplementary figure 6*). These features radiate from seismic stations in East Antarctica and their associated anomalies are truncated by other crossing paths. For example, the slow wave speeds extending from the Wilkes Land coast, near station CASY, continue inland beneath the Aurora subglacial basin and transition to fast wave speeds when reaching paths radiating grid-north from station DRV (*their supplementary figure 1*).

Other tomographic models ANT-20 (Lloyd et al., 2020), ANT-41 (Zhou et al., 2026), AP2024 (Chua & Lebedev, 2025), and others (e.g., Shen et al. 2018; An et al., 2015) do not reveal similar anomalies beneath the Wilkes Subglacial Basin or such a strong asthenospheric anomaly extending inland from the coast beneath the Aurora Subglacial Basin. Most importantly this is true for ANT-41 (Zhou et al., 2026), which uses and fits single and double difference travel times from earthquakes and travel times from ambient noise surface waves. The latter include the same station pairs used in Hansen and Emry (2025). This model, following on ANT-20 (Lloyd et al., 2020), continues to image some of the fastest shear wave speeds beneath Wilkes Subglacial Basin and continues to show that the lithosphere at the coast near station CASY is thinner (~100 km) and that this thinner lithosphere (< 200 km) extends inland partially beneath the Aurora Subglacial Basin (accompanying model comparison figure). It does not image strong slow asthenospheric wave speeds, consistent with other models (e.g., Chua & Lebedev, 2025, An et al., 2015).

From our view the imaged features presented in Hansen and Emry (2025) reflect the limited seismic coverage provided by ambient noise surface waves across East Antarctica and do not faithfully capture the underlying mantle structure. We do not believe there is compelling evidence to warrant

consideration of a thermal asthenospheric anomaly in either location of Wilkes Land that would significantly reduce upper mantle viscosities. Instead, the thinner lithosphere along the coast, already captured by our models, allows for larger and shorter wavelength deformation in comparison to the rest of the East Antarctic shield. While transient deformational processes succeeding rapid ice mass loss may enhance the solid Earth response (e.g., Lau et al., 2021).

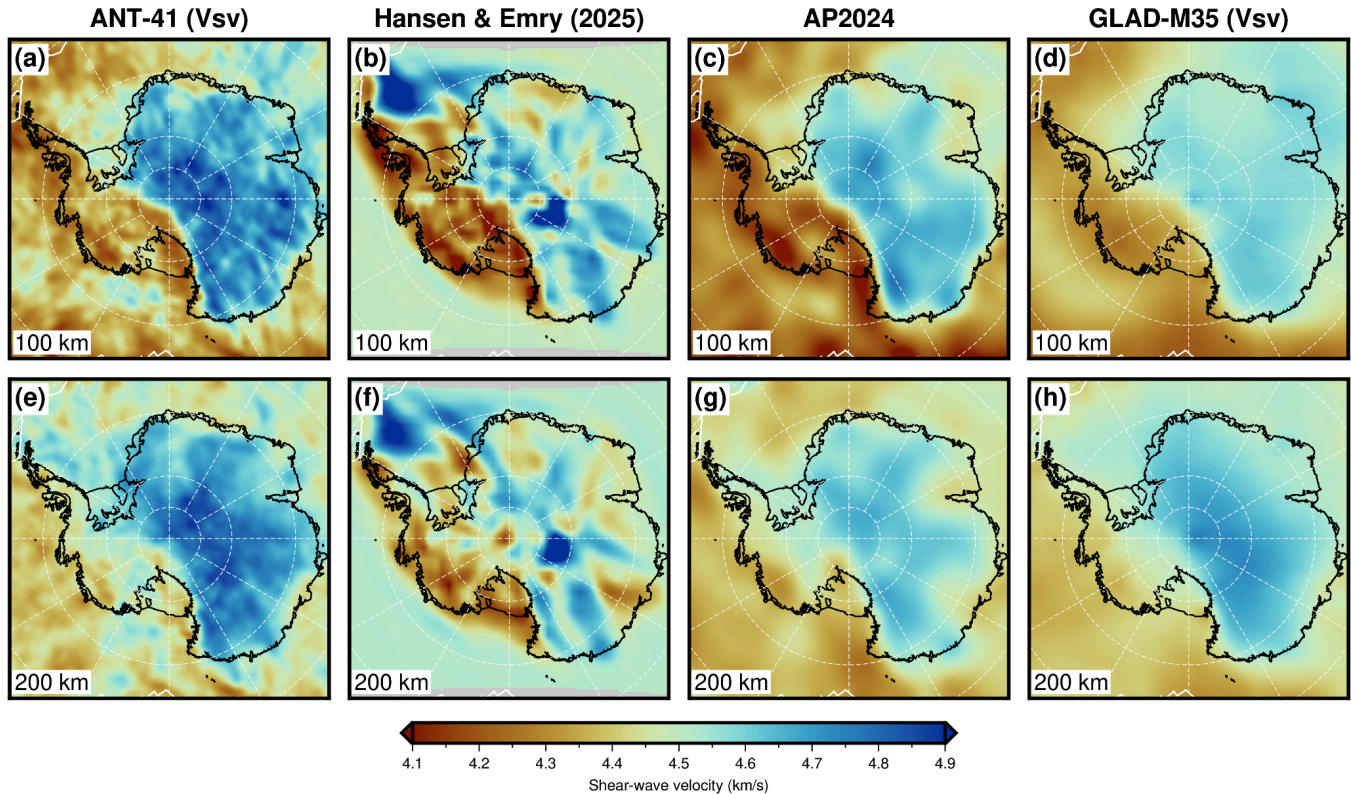


Figure 1. A comparison between the full Hansen and Emry (2025) model with ANT-41 (Zhou et al., 2026), AP2024 (Chua & Lebedev, 2025), and GLAD-M35 (Cui et al., 2024). Hansen and Emry (2025) structure across East Antarctica suggest large ranges in wave speeds and elongated structure along station-pair paths, inconsistent from other models.

L212 re RCP8.5 - it is appropriate to say that this is no longer regarded a realistic pathway due to climate mitigation and hence is an extreme upper bound for comparing the models.
<https://gmd.copernicus.org/articles/19/2627/2026/>

We will state before presenting the results that RCP8.5 is no longer regarded as a plausible pathway and is used here as an extreme upper bound, citing the reference kindly provided by the reviewer. We will also make clear that this is deliberate: the resulting GIA response is the most difficult to reproduce with a regional 2-D model and therefore the most demanding test of FastIsostasy. We also note that while RCP8.5 climate forcing is considered unrealistically high, ice-sheet projections may at the same time be systematically underpredicting the ice sheet response to a given climate forcing due to poorly captured ice-sheet processes and Earth systems feedbacks, and this level of ice sheet contribution to sea level rise is not unprecedented in the paleo sea level record either (e.g. Bradley & Hewitt, 2024; Aschwanden et al., 2021; Dutton et al., 2015). So the scale of ice sheet retreat projected under RCP8.5 climate forcing that our simulations capture is still possible. We will also include this clarification in the revised manuscript.

L244 references Fig 2a, but I think 2c,f is meant.

The reference is indeed incorrect. The values quoted (21 m and 32 m) are read from Fig. 4.a; we will correct the reference and additionally point to Fig. 2.c and 2.f for the corresponding spatial patterns. We thank the reviewer for spotting this.

the discussion of Figure 7 would be improved by referencing the individual panels of the figure in the text.

We agree and will reference the individual panels throughout the discussion of Fig. 7.

Conclusions. Perhaps some comment could be made about the coupling interface and how this may or may not suit other ice models.

We are happy to comment on the nature of the coupling interface. The coupling interface was designed to be flexible and compatible with any ice-sheet model via compilation to binaries. Coupling the source code directly can be done if the ice-sheet model is written in Julia or Fortran, and only requires an online interpolation method if it uses a different grid compared to FastIsostasy.

References

- Kreuzer et al. (2026). Antarctic Meltwater-Stratification Feedback Is Less Pronounced Under High Climate Forcing. *Geophysical Research Letters*.
- Bradley & Hewitt (2024). Tipping point in ice-sheet grounding-zone melting due to ocean water intrusion. *Nature Geoscience*.
- Aschwanden et al. (2021). Brief Communication: A roadmap towards credible projections of ice sheet contribution to sea level. *The Cryosphere*.
- Dutton et al. (2015). Sea-level rise due to polar ice-sheet mass loss during past warm periods. *Science*.