We thank the reviewer for their constructive and thoughtful comments, which helped us to improve the manuscript. We have provided our response to reviewer's comments, leaving the original comments in black text and our response in blue text.

We start by noting that both reviewers suggested that the length of the paper may detract from the findings. To reduce the length of the paper, we have moved the extended description covering the process used to construct the regional-scale viscosity model and the comparison between ICE-125 and ICE-25 ice forcings to the Supplementary Material.

Reviewer 2:

General Comments:

The submitted manuscript investigates the impact of regional-scale (50-100 km) lateral variations in mantle viscosity on GIA model predictions in Antarctica. Understanding heterogeneity in mantle viscosity is important for interpreting geophysical data and modeling ice sheet-solid Earth feedbacks, and has critical implications for the future of the Antarctic Ice Sheet.

The authors employ two previously published regional tomography models for their work. They stitch these models into continental- and global-scale topography models and convert the velocity anomaly to a viscosity anomaly. Their final models show similar features to previously published work, however, they highlight shorter wavelength variability, particularly in the Amundsen Sea.

Model predictions of solid Earth deformation, gravitational potential change, and relative sea-level change with the regional viscosity structure show significant differences from the continental and 1D models. The authors highlight the importance of these findings by comparing their deformation estimates with GPS data from across West Antarctica. While they do not find that the regional model improves the overall fit to the data, they show convincingly that these differences are significant and warrant further exploration.

This work presents a specific and important scientific problem and investigates it with sound methods. I find the work to be robust and believe it will make a solid contribution to our understanding of GIA in Antarctica. That said, I have outlined some issues below that should be addressed.

Thank you for the summary of our study and findings.

Major Points:

In isolation, the parameter choices for the 1D viscosity model are well justified, as is the justification for the 1-D reference profile from which 3D anomalies are calculated. However, I find it confusing to use two different 1D profiles in the same study. It would be much easier to interpret the differences between the 1D and CONT/REG_P/REG_S models if those 3D models

used the same reference 1D case. As written, it is unclear to what degree differences between 1D and 3D are due to the actual anomalies in the 3D model or due to the difference in the mean viscosity value (approximately an order of magnitude).
Adopting the same viscosity profile for the 1-D reference profile as used in the 1D_WAIS viscosity model (previously named "1D") would result in unrealistically low viscosity values in regions of low viscosity within the 3-D models. Additionally, we would not be able to capture near-average and high upper mantle viscosities found in other regions of West Antarctica by adopting a lower-viscosity 1-D reference profile. For these reasons, we argue that it is better to retain the current 1-D reference profile used in the 3-D models.

The adopted 1D_WAIS viscosity model we use facilitates the comparison of model predictions from this study with previous work aimed at understanding solid Earth – ice sheet feedbacks locally in the Thwaites and Pine Island glacier regions (e.g., Kachuck et al., 2020; Book et al., 2022). We have added text to Section 2.1 to further explain the rationale behind adopting the 1D_WAIS profile, including "..we adopt one 1-D (i.e., radially varying) Earth model representative of the structure of low viscosity zones in West Antarctica inferred in the literature.." and "The adopted 1D_WAIS model allows for more direct comparison with recent studies on solid Earth – ice sheet feedbacks in the ASE, which use 1-D Earth models with upper mantle viscosities in the 10^{18}-10^{19} Pa s range (e.g., Kachuck et al., 2020; Book et al., 2022)."

I think the issue of vertical smearing in body wave tomography identified in Lucas et al. (2020) is overlooked. The authors explain that the checkerboard tests determine the amplitude recovery values, but do not explain how this is related to vertical smearing of the velocity anomaly. This limits the vertical resolution of their REG_X models and certainly has an impact on the GIA results they obtain so it should be discussed. It might also be noted that their vertical resolution is quite different to the ANT-20 model.
We do acknowledge that "vertical resolution is limited in the Lucas et al. (2020) regional seismic models; however, resolution tests indicate that the imaged velocity anomalies primarily originate from mantle structure between the Moho and ~250 km depth." In Section 2.1.2. Additionally, we note that vertical smoothing in ANT-20 is fixed at ~45 km for all depths in Section 2.1.1. Per the suggestion to expand upon vertical smearing in the body wave tomography, we have added in an additional sentence to Section 2.1.2 stating: "Given that resolution tests show >150 km of vertical smearing in the Lucas et al. (2020) regional models, ANT-20 likely provides superior resolution of vertical variability in upper mantle structure with vertical smoothing fixed at ~45 km."

It is unclear to me how the regional models are inserted into the ANT-20 model. The relative travel time models should only provide velocity perturbations, while the ANT-20 adjoint model provides absolute velocities. It seems that to insert the regional model would require making some correction based on the ANT-20 mean over the same spatial domain. Could the authors please explain their methods and reasoning here?
We have added additional details to explain how the regional models are inserted into the ANT-20 model in Section S1 of the Supplement.

The added text reads: “The relative travel-time tomography approach adopted by Lucas et al. (2020) provides velocity anomalies relative to an unknown background mean rather than absolute velocities. In contrast, mantle velocity anomalies in ANT-20 are reported relative to the 1-D Earth model STW105 (Kustowski et al., 2008). Consequently, a 0% velocity anomaly in the Lucas et al. (2020) regional seismic models does not correspond to a 0% velocity anomaly in the ANT-20 model. To ensure consistency amongst the regional-scale viscosity models and the CONT viscosity model, we use the maximum and minimum viscosity bounds from the CONT viscosity model as a guide for constructing the regional viscosity models, ensuring upper mantle viscosities in the regional models remain within these viscosity bounds for central West Antarctica.”

The investigation of the 125 versus 25 year ice forcings is very thorough, however, since this paper is focused on the spatial pattern of solid Earth deformation rather than the ice reconstructions, I think including a lengthy discussion of both of these in the main text is unnecessary and detracts from the most important findings of the study. The main takeaway from Figure 4 is that there is more deformation in the region of load change in the longer loading scenario, which is not surprising and does not add much additional information in terms of how regional- versus continental-scale viscosity models behave. I suggest moving this figure to the supplement and focusing mainly on the 125 yr history in the main text.
We agree with you that the assessment of GIA predictions using 125- versus 25-year ice histories detracts from the focus of the paper and have moved the text and figure comparing results using 125-year versus 25-year ice histories to the supplement (now S1 Comparison of GIA model predictions for simulations adopting ICE-125 versus ICE-25). Fig S7 in the supplement originally showed the difference in model predictions in simulations using ICE-125 versus ICE-25 for the REG_S, CONT, and 1D viscosity models, so we have combined the plots from Fig. 4 with those in Fig. S7 for simplicity.

I suggest moving the discussion of specific features in the Earth models (section 2.1.3) to the results section. These features are a product of the conversion from velocity to viscosity and thus belong in the results. I think this will also improve the readability to have these features highlighted closer to where they are discussed in detail at the end of the paper. A sentence in the beginning of the results section (lines 379-380) actually indicates that this was the intention, but for some reason it was placed in the methods.
We agree that moving the section on “Regional upper mantle viscosity features” to the results section improves the readability of the paper. Section 2.1.3 is now included in the results section as Section 3.1, with some minor edits.

Ideally, the authors would address the issue of future projections of GIA by running a fully coupled simulation with a dynamic ice model. This would be the only way to fully understand the impact that their REG_X viscosity models might have on groundline dynamics and GIA. Without coupled simulations, it is hard to interpret their results since these ice-loading models (ICE-FUT) are based on different viscosity structures. At a minimum, it would be helpful to plot the groundline evolution (as calculated by the floatation criterion in the Seakon) in Figure 6 for different models to assess the potential impact this viscosity structure might have on ice stability.

We agree that coupled simulations with a dynamic ice model would be the best way to understand the impact of incorporating regional upper mantle structure on grounding line dynamics and, motivated by the results of this investigation, we foresee pursuing such an investigation in the future. Such simulations are highly computationally expensive and as illustrated in Gomez et al. (2024) with the continental viscosity model, the strength and nature of the feedback is sensitive to the climate forcing. We thus feel that a thorough exploration merits its own study.

As the flotation criterion in Seakon does not accurately capture the feedbacks between GIA and ice sheet dynamics, we feel that it would be misleading to show and challenging to interpret grounding line positions calculated using the floatation criterion for each viscosity model. Thus, we only show grounding line positions predicted using the ICE-FUT model in Figs. 4-5.

Comparing predicted and observed crustal rates is a nice way to highlight the importance of regional viscosity models. However, it is unclear to what degree either model performs better/worse than the 1D or CONT models in matching observed vertical rates overall. For each model, I would suggest reporting an average residual between predicted and observed rates (for the vertical rates at least). This would provide context for the authors' argument that regional models are necessary to accurately interpret the data (lines 687-690).

To make the performance of each viscosity model more clear, we have added text to Section 4.1 in which we report the average residuals between the observed and predicted vertical crustal rates for the 1D, CONT, REG_P, and REG_S models.

The added text reads: "Across all GPS sites, the average residual between observed vertical crustal rates (corrected using Gomez et al. (2018) model predictions) and model predictions is 9.1 mm/year for simulations adopting the 1D_WAIS viscosity model. In comparison, the average residuals for simulations using the CONT, REG_P, and REG_S models are 6.1 mm/year, 6.7 mm/year, and 7.2 mm/year, respectively."

In keeping with the central theme of the paper to investigate the impact of shorter wavelength features, I think it would be useful to have a paragraph in the discussion about whether the resolution of the adopted models are good enough. Should the GIA community strive for even higher resolution? What resolution is unnecessarily high? Is there evidence to suggest low/high viscosity zones may exist that these new models do not capture? I think the authors have valuable insight to contribute and could strengthen the overall impact of the paper by addressing these questions.

We have expanded Section 4.2 to discuss how continuing to improve constraints on various low viscosity mantle features across West Antarctica will help to improve the accuracy of GIA predictions and likely reduce data-model misfits. More specifically, we expand upon the discussion of how constraining the geometry of localized low viscosity mantle features, like that found beneath the Byrd Subglacial Basin, will help improve GIA predictions:

"For instance, as discussed in Section 3.4, the simulation with REG_P predicts ~10 m lower sea level along the eastern portion of the TG grounding line in 2300 due to the presence of low viscosity upper mantle material beneath the Byrd Subglacial Basin (Feature C; Fig. 4b, h), a graben that likely underwent Neogene extension (e.g., LeMasurier et al., 2008; Granot et al., 2010; Lucas et al., 2020). The influence of accounting for such a localized upper mantle feature on GIA predictions underscores the need for improved geophysical constraints on the spatial distribution and geometry of similar low viscosity mantle features across West Antarctica. In particular, refining constraints on Earth structure in other areas that may have experienced localized Neogene extension – such as the Pine Island Rift (beneath Pine Island Glacier) and Bentley Subglacial Trench (adjacent to Byrd Subglacial Basin) – as well as various Cenozoic volcanic provinces will improve the accuracy of GIA predictions and reduce data-model misfits."

Minor Points:

Line 80: Could you provide approximate length scales of 'local' and 'regional'? Reviewer 1 also commented on this, and we have added in approximate length scale for regional-scale imaging and removed the reference to 'local' scale imaging for clarity. We originally mentioned local-scale imaging in reference to the Lucas et al. (2021) study, which images uppermost mantle structure near the grounding lines of Thwaites and Pine Island glaciers; however, this could be considered regional-scale imaging. Therefore, we have simplified this by removing the reference to local-scale imaging.

Line 87: Similarly it would be nice to define clearly what is exactly meant by regional and how much it differs from continental. We have further specified the scale of regional-scale imaging in the introduction and note that regional-scale imaging has revealed heterogeneity at the glacial-basin scale: "glacial-basin scale investigations of GIA have remained elusive due to limited seismic resolution. However, benefiting from improved seismic station coverage in West Antarctica, recent regional-scale (~400 - 1000 km length-scale) seismic imaging has revealed notable heterogeneity in upper mantle seismic velocities within the TG and PIG glacial drainage basins…"

Line 88: It would be useful to define 'relative sea level' here or somewhere in the introduction or at the beginning of the results section. This term can be confusing especially in studies like this where simulations are run both from the past to present and from present into the future. We now define relative sea level in the last paragraph of Section 1 – "…we evaluate the impact of regional-scale variability in upper mantle viscosity on predictions of changes in relative sea level (i.e. the height of the sea surface equipotential relative to the solid surface)".

Line 194: Looks like something happened to part of a sentence here. You are correct, and we have added on the rest of the sentence. It was meant to say "how to correct for underestimated seismic velocity anomaly amplitudes in the Lucas et al. (2020) models."

Line 405: I would suggest changing "higher relative sea level" to "less relative sea level fall". It may be confusing to some who are less familiar with GIA and thinking about 'relative' sea level to interpret this sentence. Saying “less sea level fall” more directly gets to the point that over the length of the simulation there is less change in RSL in the 1D model. (On a very technical level, the statement that RSL is ‘higher’ at present is also confusing since the final prediction of the Seakon (or any) GIA code in a historical/paleo simulation is that RSL=0.) We have changed the wording from “higher relative sea level” to “less sea level fall”. We agree with you that this phrasing will reduce confusion for readers who are less familiar with GIA.

I might also change ‘lower magnitude vertical crystal rates’ to ‘lower magnitude modern-day vertical crustal rates’ if that is what is plotted. We have changed the wording to “lower magnitude modern-day vertical crustal rates”.

Line 416: Could you label the PSK region in Figure 3? The PSK region is labeled in Fig. 3a, so we have changed it to reference Fig. 3a instead of Fig. 1d.

Figure 3: The label on the left for plots (a) (b) and (c), should more accurately be ‘change in relative sea level’ or ‘relative sea level at 125 ya’. We have changed the label on the left for plots (a), (b), (c) to “relative sea level change”. We have also updated several plot labels in the supplement from “relative sea level” to “relative sea level change”.

Line 419-420: Following the comment about line 405, I would change the language to “less change in relative sea level”. Changed the wording to “less change in relative sea level”.

Figure 4: It would be useful to readers to state which model is subtracted from the other in the figure caption. (Same with Figure S4). We have added to the second sentence of the caption that we do “(REG_P minus CONT predictions)” and “(REG_S minus CONT predictions)” To further clarify for readers. We have also added “(1D predictions minus CONT predictions)” to the caption of Fig. S4.

Figure 5: I would add in the caption a line to aid the reader in interpreting the REG_P - CONT and REG_S - CONT plots. Something like: “positive values indicate overall less sea level fall in the regional model during the labeled time period”. We have added “In the REG_P – CONT and REG_S – CONT plots, positive values indicate overall less sea level fall in simulations adopting the regional model during the labeled time period, while negative values indicate greater overall sea level fall.” to the caption of Fig. 4 (old Fig. 5). Additionally, we have added the line “In (b-c), positive values correspond to less sea level fall predicted in simulations adopting the regional viscosity models compared to those adopting CONT, while negative values correspond to greater sea level fall.” to the caption of Fig. 3 to aid reader interpretation.

Line 543-544: Here, I think it makes sense to say ‘higher relative sea level is predicted’ since RSL can vary between different models in the future. But I would clarify this in the sentence and also add a point about what this means for overall sea level change to aid the reader. I would correct this by changing:

“Compared to simulation with CONT, higher relative sea level (+1.31 m compared to CONT) is predicted in the central PIG basin with the REG_P model”

To something like:

“In the central PIG basin, the REG_P predicts overall less sea level fall from 1950 to 2050 compared to CONT, resulting in 1.31 m higher relative sea level in 2050”
Thank you for the suggestion on how to make the wording in this sentence clearer. We have updated the sentence based on your suggestion.

Line 543-548: I found this paragraph confusing. I would suggest revising to get at the really intriguing differences between the REG_P at 2050 (which predicts less sea level fall overall) and the REG_S (which has a northern region with more sea-level fall and a southern region near Thwaites with less). We have done significant revisions to this paragraph to make it less confusing. It now reads:

“In the central PIG basin, the REG_P simulation predicts less overall sea level fall from 1950 to 2050 compared to the CONT simulation, which ultimately produces 1.31 m higher relative sea level in 2050 in the REG_P simulation (Fig. 4a-b). Unlike the REG_P simulation, greater overall sea level fall (-0.49 m) is predicted from 1950 to 2050 in the northern PIG basin in the simulation adopting REG_S versus CONT (Fig. 4a, c). These discrepancies in relative sea level predictions in the PIG basin can be attributed to differences in the REG_P and REG_S viscosity models. More specifically, the presence of low-viscosity Feature B in REG_S, which is not as prominent in REG_P, is what produces greater overall sea level fall in the REG_S simulation.”

Figure 6: In panels A and B, could you also plot the viscosity anomalies along the profile for REG_P and REG_S? We find it difficult to effectively visualize the bedrock elevation profiles and changes in bedrock elevation if the viscosity anomalies are also plotted in Fig. 5 (previously Fig. 6); therefore, we have left the figure as is.

Figure 7: The symbols in the A and B are hard to read. I would suggest offsetting the symbols horizontally by a small amount and adding dashing lines to separate each station. REG_P (ICE-25) could also be in the supplement. We have taken your suggestion to offset the symbols in the figure Fig. 6 (old Fig. 7) and have added dashed lines to separate each station. We have removed predictions from the REG_P ICE-25 simulation and moved them to a new figure in the supplement (Fig. S8). Fig. S8 includes predictions from simulations adopting the ICE-25 and ICE-125 ice models with the REG_P viscosity model for comparison.

Line 720: Could you say in which direction it would alter it? More or less? We further clarify that “accounting for regional-scale viscosity structure could *reduce* the amount of uplift at the TG grounding line by up to 20% (or up to 20 m), which would *negatively* impact the strength of the sea level feedback in the region.”