We thank Reviewer 1 for their constructive and thoughtful comments, which helped us to improve the manuscript. We have provided our response to the reviewer's comments, leaving the reviewer's original comments in black text and our response in blue text.

**Reviewer 1:**
In this study the authors investigate lateral heterogeneity in upper mantle viscosity below West Antarctica. They compare three different viscosity profiles based on three different seismic models, two that are able to distinguish higher resolution features. They use the viscosity profiles to investigate the effect on GIA in the region using three ice histories: extended modern, modern and future. The results are compared to GPS vertical and horizontal rates. The main findings are that there is up to 20% difference in sea-level change near the grounding line of Thwaites and Pine Island glaciers over the next 300 years. Showing that including higher resolution 3D variations in upper mantle viscosity is important for these crucial and highly dynamic areas.

**General Comment:**
Overall, the paper is interesting, and the science is solid. However, my main concern is that the impact is lost in the sheer length of the paper, with already a lot of figures in the supplementary material. My comments are geared towards simplifying this paper to allow the science to be more impactful.

Thank you for the summary of our study and findings. To shorten the paper and make the science more impactful, we have moved the text and figure detailing the investigation of the 125-year (ICE-125) versus 25-year (ICE-25) ice forcing to the Supplementary Material. We now primarily report on results for simulations adopting ICE-125 in the main text and focus on discussing how different degrees of upper mantle heterogeneity impact GIA model predictions. We have also moved the extended description of the process used to construct the regional-scale viscosity models (REG_P and REG_S) to the Supplementary Material. The text has also been edited and simplified in numerous places in response to specific suggestions and questions below.

**Specific Comments:**

- **Scales**: Abstract line ~18 and introduction. It becomes a bit confusing with the mention of scales here. You mention continental, regional, local scale, and then >150km, 50-100km, and 50-200km separately. It would be useful to clearly define the spatial scale in km of each of the continental, regional and local scale – perhaps lines on 78 and 80. Furthermore, "local scale" is then not mentioned in the results – they are either CONT or REG. Is the regional scale actually representative of regional and local combined? Please clarify and simplify. Line 86, again clarify the scale in km. We have added in an approximate length-scale for regional-scale seismic imaging to Section 1: "…recent regional-scale (~400 - 1000 km length-scale) seismic imaging…". 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 also be considered regional-scale imaging.

Therefore, we have simplified this by removing the reference to local-scale imaging here and throughout the manuscript.

- **Section 2.1**: I think this section is too long. We have shortened this section by moving the description of the process used to construct the regional-scale viscosity models (REG_P and REG_S) to the Supplementary Material. Additionally, the section describing "Regional upper mantle viscosity features" has been moved to the Results section (now Section 3.1).

    - 1D model – This is introduced as if there will be lots of comparisons between the 1D and 3D, but I can only see it in Figure 6 of the main paper. This is fine, and I'm more interested in the differences between the 3D models anyway, but perhaps state on line 132 that most 1D results are in supp info. First, we have changed the name of the "1D" model to "1D_WAIS". We have added that "As this study primarily focuses on GIA predictions using 3-D viscosity models, results from simulations with 1D_WAIS are largely presented in the Supplementary Material (Fig. S4-S7)."

    - Line 162: why is this 1D viscosity profile different to that mentioned earlier? The 1-D reference viscosity profile that we describe here is distinct from the 1D_WAIS viscosity model (previously just "1D") described earlier. As described in the preceding text, this is a reference profile on which "Lateral mantle viscosity variations are superimposed" for the 3-D viscosity models.
      It is a global average? If so, I would suggest to delete the end of the sentence "consistent with previous GIA modelling…. In west Antarctica" as this makes it sound like previous modelling shows UM viscosity in West Ant to be 5x10^20. You are correct, this reference viscosity profile is representative of a global average. We have removed the portion of the sentence referring to West Antarctica for clarity.

    - Line 175 to 181: Suggestion - I think you can simplify this to: "…47degS (Fig 1a). [delete the next bit] Global viscosity variations are estimated from GLAD because it offers improved structure compared to S326ANI that was used in Gomez 2024" We have simplified the sentence based on your suggestion.

    - Paragraphs starting line 217 and 234: I think most of this detail should be moved to supp info, particularly (all of?) the second paragraph. We have kept key details about the method followed to merge the regional- and continental-scale models in the main text and moved much of the extended detail on steps taken to merge the models and scale to Section S1 of the Supplement.

    - I'm interested to know how you join the regional models into the ANT20 model – in terms of the edge effects. Do you have to do smoothing where these two datasets are joined together? Or are they referenced to the same background

model so its not an issue? Perhaps include in the supp information with (d). We have included additional details clarifying the methods used to merge the models in Section S1. We adopt a ~75 km smoothing band along the edges of the regional and continental models to ensure a gradual transition between the model. These two models are not referenced to the same background model because the regional seismic models provide anomalies relative to an unknown background model mean.

The added text reads: "When inserting the regional seismic models into ANT-20, a ~75 km smoothing band is adopted along the edges of the regional and continental models to ensure a gradual transition between the models (Fig. 1). 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."

- Line 294: "error" feels a bit harsh. Change to limitations? Maxwell is still valid! Could you return to this in the discussion – any idea what might the impact be if the experiments were repeated with a transient viscosity? Good point – we changed "error" to "limitation" here.

  Because the manuscript is already long, we do not explore the impact of adopting transient viscosity. To return to the idea that transient or non-linear rheology may need to be considered, we have added a sentence to the discussion noting that "…discrepancies between predicted and observed crustal rates in the ASE may indicate that mantle deformation is better represented by transient or non-linear rheology (e.g., Lau et al., 2021; Blank et al., 2021; Ivins et al., 2022) rather than the adopted Maxwell viscoelastic rheology."

- **Section 2.2:**
  - Just on terminology here – modern/extended modern ICE-25/ICE-125. Later on ICE-125 is also referred to as "modern ice mass change" e.g. section 3.2 title. Either change "modern" to "extended modern" where referring to ICE-125, or state that there are two version of modern in this section, and then be clear which modern you're talking about. Thank you for catching the inconsistency in our terminology. As we have moved the discussion comparing results in

simulations using ICE-25 and ICE-125 to the supplement, we have updated the text to refer to ICE-125 as the modern ice loading model.

- Figure 2 caption – some of this description is repeating what is in the main text on how the ice histories are constructed. Not needed in the caption. Panel b – over what area or drainage basic is this calculated? We have removed the description of how ice histories are constructed from the caption. We also specify that cumulative mass is calculated for the entire Antarctic Ice Sheet.

- **Section 4.1**: comparison to horizontal rates. I question the inclusion of comparing to horizontal GPS rates (line 656 to 685; Figure 7b and c). As the authors state, the horizontal GPS are not corrected for any kind of background GIA rate, and so already the comparison between the model and GPS observed rates in 7b is not going to match. I think this whole section could be removed or put into supplementary information. GIA studies in Antarctica (and elsewhere) often overlook the comparison between predicted and observed horizontal crustal rates. However, horizontal crustal rates undoubtedly provide valuable insight into both Earth structure and ice history. To underscore the importance of improving data-model misfits for horizontal crustal rates, we prefer to leave the discussion of horizontal crustal rates in the main text. While we acknowledge that our original manuscript was too long, we feel that the other edits we have made to address this will suffice.

**Minor Comments:**

Line 51: Change to "Accurate modelled predictions" since empirical estimates of GIA do not rely on Earth properties. Added in modeled to "accurate predictions".

Line 111: remove "the" > according to Wan. We think that it reads better with the "the" before Wan.

Line 175: put the transition zone depth in brackets here to help the reader, and also for the Moho on line 216. Replaced with 670-km depth for clarity. We cannot reference a single depth for the Moho here because it is variable.

Line 194: something has happened with the end of this sentence – "how to correct". 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."

Figure S1: the scale of 200km looks like a label, a bit confusing as I thought I was looking at two slices at 200km depth. We have changed the scale to show 400 km instead of 200 km to reduce confusion.

Line 388: modern or extended modern? As we have moved discussion of simulations adopting ICE-25 versus ICE-125 to the supplement, we are now referring to simulations with ICE-125 as modern throughout.

Line 416: Super picky – but can you change the reference to Fig 3a where PSK is also labelled? To save a lot of going backwards and forwards to different figures. Yes - we have changed the reference to Fig. 3a instead.

Lines 541-551 and the next paragraph: I'm not sure I follow here. Does "higher relative sea level" mean "less sea level fall"? So the CONT model predicts more sea level fall than the REG models? Might be worth just clarifying here and relating to the colours on the graph. Yes, higher relative sea level does mean less sea level fall. We have changed several references to "higher relative level" to "less sea level fall" to make the two paragraphs read more clearly.

Figure 6: Is this figure really needed? Yes, we think that this figure (now Fig. 5) is useful for visualizing differences in grounding line elevation amongst the simulations in a different way than what is shown in Fig. 4. We have added some further references to this figure in the text so that it is now better utilized to explain our findings.

Figure 7: The symbols are hard to see because they are so small. In fact, the statement on line ~650 "introducing regional variations in viscosity reduces differences between predicted and observed crustal rates at a number of sites" is really hard to see in this figure. It looks like the opposite is true. I wonder if REG_P ICE-25 should be removed, REG_S colour could be changed to group it in with REG_P, leaving CONT and 1D in blue/green. We have updated Fig. 6 (previously Fig. 7) to offset the symbols from each other horizontally for each GPS station. We have removed predictions from the REG_P ICE-25 simulation in Fig. 6 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. It would not make sense to make the symbols from REG_P and REG_S the same color as they are predictions from two different simulations.

Section 4.1: Is there also an elastic correction made to the GPS rates for SMB? No, an elastic correction is not made to the GPS rates for SMB. We have added "An elastic correction for surface mass balance is not applied to the GPS crustal motion rates. " to Section 4.1.

Line 716: What is this "O" in O(100m)? Please define. The italicized O can be used to denote "on the order of". To reduce confusion, we have removed the *O* and replaced it with "on the order of".

Line 720: alter the amount of uplift by 20% impacting the strength of the feedback…. Which way does this go – does it increase or decrease the strength of the feedback, i.e., is it stabilising or destabilising/less stabilising by including regional scale 3D variations in viscosity? 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."

Code availability: It's great to see that this code is on its way to being made publicly available.