Adhikari et al. provide a method for rapidly calculating the gravitational and deformational solid Earth response (glacial isostatic adjustment) to ice sheet loading/unloading that is applicable for use near-field of ice sheets on decadal to centennial timescales. The approach outlined in this paper will be useful for ice sheet modelers who are interested in including accurate gravitational and deformational solid Earth feedbacks with low computational cost. This is a beautifully written paper that clearly articulates the method proposed, which is a simplification of a global gravitationally self-consistent sea level model.

Essentially, the authors make the argument that in places like Antarctica, the gravitational and deformational response to the ice load is the dominant control on the solid Earth response, and it is reasonable to ignore far-field ice sheet changes, ocean loads, or true polar wander. If this is the case, then calculating the gravitational and deformational response simply requires a single convolution calculation (convolving precomputed Green's functions with a given load history). They show that viscoelastic calculations using this approach differ by only a few percent compared to global self-consistent sea level models, and this small difference is dominated by rotation. Such a simplified approach can easily be adopted by ice sheet modelers to include gravitational and deformational feedbacks in dynamic ice sheet simulations and provides a computationally effective method for high resolution simulations. The authors have published a previous paper that provides a large suite of precomputed Green's functions for different Earth models, so ice sheet modelers have a range of 1D Earth structures to select from, and may even be able to better represent uncertainty on Earth structure by performing simulations with many different 1D Earth models.

There are limits to the proposed approach: it is not appropriate for thinking about global feedbacks or long-term stability of ice sheets in response to glacial isostatic adjustment. We would guess that on the millennial timescale (modeling for more than 1000 years), it will be important to include global ice sheets (global sea level change and far field effects), ocean loads, and rotation in order to match the global self-consistent sea level model. This approach also ignores 3D Earth structure heterogeneity, an important point to mention for a paper focused on Antarctica, where lateral heterogeneities can significantly alter glacial isostatic adjustment predictions (e.g. Lucas et al, Cryosphere, 2025)

Below we list a few suggestions for strengthening the manuscript:

1) More specific context for readers on WAIS-earth structure:

We recommend providing additional information in the manuscript about regional variability in West Antarctic Earth structure. While the authors state in the paper that these two Earth models represent laterally averaged Earth structure in West and East Antarctica, respectively, it would be more accurate to say the Earth model selected to represent WAIS specifically represents coastal West Antarctica, specifically the Amundsen Sea Embayment sector, and not laterally averaged Earth structure across the entire WAIS. The authors justify their choice of both WAIS-earth and EAIS-earth models by referencing the Lloyd et al. (2020) and Ivins et al. (2023b) studies in Lines 84-84; however, neither of these studies suggest an average lithospheric thickness of 50 km across West Antarctica. Lloyd et al. (2020) does not quantitatively discuss lithospheric thickness in Antarctica. Instead, we suggest referring to the lithospheric thickness estimates from Wiens et al. (2023) and Brown & Fischer et al. (2025). These studies find 60-100 km lithosphere across West Antarctica, suggesting the choice of a 50 km lithosphere as a laterally-averaged value is not consistent with seismic constraints for West Antarctica.

We are happy with the choice to include the Earth model selected to compare against the EAIS-earth model (we don't see a need to change all of the figures or results), as long as the authors specify that this Earth model represents an end member rather than an average value for the WAIS, and that this earth structure is likely representative of specific regions (e.g. Amundsen Sea Embayment). We suggest citing studies that justify this Earth model for these specific regions in coastal West Antarctica, since such an Earth model (50 km lithosphere and 10^19 Pa s upper mantle) is certainly not appropriate for places like Weddell Sea or Ross Sea sectors. Explicitly explaining the choice of Earth model and the applicable region will be important for an audience of ice sheet modelers who need to decide which earth model is appropriate for their location of study.

Because significant ice sheet retreat is projected in the Weddell and Ross sectors over the next ~3 centuries, it will be useful to explain to readers that these are regions where there is lithosphere >50 km and upper mantle viscosities >10^19 Pa s. Accurately capturing the solid Earth and sea level response to ice mass loss in these sectors will therefore require an Earth model different from that adopted here.

Given these comments, we believe the manuscript would be strengthened by adding a paragraph that discusses the best choice of Earth models to use for different parts of Antarctica. Such additional text will be especially useful for a paper geared towards the ice sheet modeling community, as readers will want to know that there is not a single Earth models they should use to represent West Antarctica. These users should be aware that such a thin lithosphere and low-viscosity upper mantle represents one of the Earth structure end members in West Antarctica. We think this would broaden adoption of the proposed method to explain more clearly how ice sheet modelers can select from this large suite of 1D Earth models.

2) More discussion of the appropriate use of this methodology:

It would be helpful for the authors to include more discussion about the appropriate use (and limitations) of the proposed methods in the discussion/conclusions section. For example, even though the authors present two different Earth models for Antarctica, the proposed method ultimately requires a single 1-D Earth structure, which in not realistic across all of Antarctica. The authors need to mention this point to better situate their proposed method with respect to other methods. You might include reference to other efforts aiming to approximate bedrock deformation assuming laterally heterogeneity (i.e., van Calcar et al., in review: "Approximating ice sheet – bedrock interactions in Antarctic ice sheet projections"). It will also be useful to explicitly state the appropriate timescales and questions that this method targets. For example, adding a sentence or two that describes the potential uses of this methodology and also notes its limitations (multi millennial timescales etc.). While there is a sentence in the text that mentions long-term stability and global feedbacks, it should be explicitly stated somewhere what timescale limitations there might be using this method. Explain when it will be important to include rotation, ocean loads, and far-field effects. Describing the appropriate uses of this methodology will also be useful to emphasize in the abstract and introduction - and if the authors really wanted to look in detail they might provide information in the supplement showing at what timescale there is no longer a good fit between this methodology and that of a globally self-consistent model.

Line by line comments:

- Abstract**:** Need to specify somewhere in the abstract that the proposed approach assumes 1-D / laterally homogeneous Earth structure.
- Line 64: the dashes in this line are slightly confusing, may be good to phrase this without the dashes.
- Line 81: can you define mean sea level here rather than referencing a paper. MSL is referring to see surface height, is that correct? It is a little confusing to have GMSL and MSL in the same paper, it might be less confusing to use a different acronym.
- Similarly S is a confusing symbol for the geoid when G is usually used in papers on the sea level equation, we suggest changing Gx to GFx and making G the geoid variable, this would be simpler for the sea level community to follow given what past variable names have been used for
- Rather than explaining the variables in the text, it would be useful to include a table with all the variable names as an easy reference for the reader
- Line 108: I think of Kendall et al. 2005 as a standard reference for the sea-level equation, might consider including
- Line 171: Consider adding additional references that explore coupling GIA and grounding line predictions at high resolution: Kodama, et al. "Impact of glacial isostatic adjustment on zones of potential grounding line stability in the Ross Sea Embayment (Antarctica) since the Last Glacial Maximum." *EGUsphere* 2024 (2024): 1-25., Wan, Jeannette Xiu Wen, et al. "Resolving glacial isostatic adjustment (GIA) in response to modern and future ice loss at marine grounding lines in West Antarctica." *The Cryosphere* 16.6 (2022): 2203-2223.

Figures

- Figure 1c: In the caption, it would be useful to specify that this figure represents an ice load that is shrinking through time to make the figure easier for the reader to interpret.
- Figure 2: why is unit in meter per kilogram?
- Given the importance of the WAIS-earth and EAIS-earth models to the entire manuscript, please move (or at least repeat) the description of the WAIS-earth and EAIS-earth models to the main text instead of putting this description in the caption of Figure 2.
- Figure 3c: We suggest showing a global map instead of just the northern hemisphere. It is somewhat confusing to only see a portion of the northern hemisphere. A global map would help the readers better understand what is happening in Antarctica.
- Figure 4: It would be informative to include cumulative mean sea level changes in Figure 4, similar to how figures of mean sea level are included alongside vertical land motion in Figure 3. Plots of mean sea level in Figure 4 would be relevant to the points made in the discussion section, comparing the self-consistent model to the proposed method.
- Method accuracy for viscoelastic Earth models is only compared for 2300 in Fig. 4. It would be useful to ice sheet modelers to see other time steps comparing method accuracy, i.e., 2150. Such a figure could be included in the supplement perhaps.