Response to Volker Klemann

The authors propose in this manuscript a new method to consider the viscoelastic response of a viscoelastic earth to surface loading. The method is designed to consider the solid-earth feedback mechanism in coupled dynamic ice sheet models. In order to reach an efficient set up, they made some simplifications with respect to the usual solution of the glacial isostatic adjustment problem, here called GRD.

The main advantage I see in the consideration of pre-calculated Green's functions. They suggest an efficient algorithm for the convolution, although I would argue that such an algorithm should be state of the art. Furthermore, the main aspects of the algorithm only appear in the Appendices. Only after reading it becomes clear that this algorithm is not sufficient to analyse the SE-IS feedbacks for longer time scales, when efficiency breaks down and also the accuracy is reduced. Furthermore, the sea level equation is not considered regarding mass conservation, ocean loading and also the rotational feedback are not considered in this approximation.

Whereas the authors rise the impression that the GRD response is considered, at the end their approach turns out to solve the sea level equation in its simplest form (Spada et al., 2019), i.e., without mass conservation, ocean loading and rotational feedback, providing the local relative sea level change due to radial displacement and gravity change from the ice-mass change only. They should set this premise at the beginning of the study and motivate it from the specific application to solid earth--ice sheet model coupling
on decadal to centennial time scales.

We thank Volker for his time and constructive review. While he acknowledges the usefulness of the proposed method, he raised some issues about its limitations, especially for its application to long-term simulations. We apologize for not making this clearer, but the main purpose of this manuscript is to facilitate ice sheet models to capture *leading-order* solid Earth feedback on the *centennial* timescale with *relative* ease.

- With *centennial-scale* simulations (the timescale of interest for IPCC and partner consortia such as CMIP and ISMIP), we show that a simple convolution of ice load and Green's functions yields (conservatively speaking) more than 90% of the self-consistent sea level/solid Earth feedback signal (see Figures 3-5).
- As ice load alone induces a large portion of the feedback signal, the ocean load effect and rotational feedback together play a secondary role (at least on centennial timescales). Consequently, ice sheet modelers -- not so familiar with glacial isostatic adjustment (GIA) processes but nonetheless interested in capturing the leading-order feedback signal -- may forgo solving a sea level equation, thereby simplifying the coupling strategy.
- Of course, the simplified coupling strategy comes at a cost (despite its ability to capture the feedback signal *sufficiently accurately*) -- the cost of not being able to (1) capture self-consistent sea level/solid Earth feedback, (2) satisfy ice/ocean mass conservation, and (3) account for the effects of far-field ice melting or coastline migrations. We assert that all these effects play a secondary role in the centennial-scale simulations.

We have conveyed these points by clearly articulating the scope and limitations of the proposed method in the introductory paragraphs [lines 44-65], abstract [lines 6-10], and conclusions [lines 274-283].

Additionally, we have revised the title to enhance transparency further [underlined words are new additions]: "Enable ice sheet models to capture centennial-scale solid Earth feedback with relative ease and sufficient accuracy."

In particular, they show that these simplifications are sufficient for modelling the future evolution of the West Antarctic ice sheet during the coming centuries giving an error of up to a few %. I see this approach valid for the applications described, but I question its help when working over longer time scales, like a glacial cycle, when interpreting geodetic observables, or when considering lateral variations in earth structure consistently. In so far, I see this model as a small step towards a more realistic coupling of the solid earth to ice sheet dynamics, but not sufficient in order to achieve a physically consistent solution.

As noted earlier, the proposed method is designed for centennial-scale simulations. So, it is not recommended for long-timescale simulations (e.g., glacial cycles), over which the effects of ocean loads, far-field ice melting, migrating coastlines, and rotational effects all come into play [TCD lines 185-190]. In such applications, solving the sea-level equation is the only option, and no simplified method can do justice [TCD lines 197-199]. We agree that that this point should be stated up front in the manuscript. We have done so in the revised manuscript [lines 57-65; 274-283].

Like any 1-D glacial isostatic adjustment (GIA) models, the proposed method, which is based on radially stratified Earth [TCD line 79], cannot capture the 3D Earth effect. (Kindly see our response to the second reviewer on page 9 of this rebuttal.) We have made this point clearer upfront in the manuscript [lines 62-64]. Thank you for pointing it out.

Your last statement correctly sums up the paper's sentiment -- encourage ice sheet modelers to capture leading solid Earth feedback without much hassle [lines 289-294].

My summary is minor, as the presentation of the results can be improved. Furthermore I recommend to transfer this manuscript to GMD, which I see more on the side of discussing software developments (the reason why I set novelty to Fair). In particular for TC, I do not see a major impact, also due to the fact that the applicability of this method was discussed only for the future evolution of West Antarctica. Otherwise, the setup is ok, and my further recommendations remain valid also after a possible transfer.

While there are some merits to sending this manuscript to GMD (in fact, we also contemplated about that before targeting TC), we argue that TC is still a better choice, as the method is specifically tailored for Cryosphere scientists who may not be as familiar with solid Earth/sea level processes but still would like to capture leading-order feedback mechanisms for centennial-scale simulations (e.g., some ISMIP participants). This point has been articulated in the introductory paragraphs [lines 45-48] and conclusions [lines 289-294].

Further recommendations:

1. L. 7: I would skip 'virtually additional software development'. My interpretation is, that only an offline coupling is necessary, where only the interface has to be set. But such approaches were already applied in a number of coupled models like PISM (Albrecht et al., 2024), and MPI-ESM (Mikolajewicz et al., 2025 https://doi.org/10.5194/cp-21-719-2025). Only to list those in which I was involved.

Thank you for bringing these references to our attention. The quoted phrase does not appear in the revised abstract anymore.

2. In the Introduction I miss a more structured discussion to which time and spatial scale the method is designed for.

Please see our introductory response. We have dedicated a couple of paragraphs in the Introduction to clarify the scope and limitations of the proposed method [lines 44-65]. Additionally, the revised title more transparently reflects the method's scope and utility.

3. L. 22ff: Regarding the solid earth response, I miss the discussion of ELRA and Lingle and Clark, which are both considered for instance in PISM, with more realistic viscoelastic displacements but no gravity change.

We have now explicitly acknowledged these important papers [lines 23-26].

4. L. 28: I miss here the reference of Konrad et al., (2015, http://dx.doi.org/10.1016/j.epsl.2015.10.008).

Indeed, we missed this important reference in the TCD draft. We now cite it on line 30.

5. L. 36: Konrad et al. (2016, https://dx.do.org/10.2312/polfor.2016.005), compared ELRA against a global GIA model representing the solid earth response in a coupled system.

Unfortunately, we were unable to locate this article. We will happily acknowledge it in the next iteration if deemed critical.

6. L. 38: Coming back to my 1. suggestion.

The quoted statement does not appear in the manuscript anymore.

7. I don't see the requirement of adapting a GIA module in an existing code. Regarding VILMA it was done for CLIMBER-X (Willeit et al. 2024, https://doi.org/10.5194/cp-20-597-2024) but for PISM and MPI-ESM it was done off line, by only exchanging the RSL and Ice thickness.

Noted. See our response to 1 (page 3 of this rebuttal).

8. L. 55: Also, I would reserve an own equation for the definition of R.

Agreed. We now introduce a new equation [Equation 2, line 76]: R = S – B

9. L. 74: p was not introduced.

p represents the number of computational grids (or elements) having q vertices (or nodes). It is introduced on TCD line 62. In the revised manuscript, it appears on lines 83 and 115.

10. L 82ff: The authors should specify the sampling and set up of the viscosity structures. Furthermore, they state that the time range extend from seasonal to hundreds of years. So, why do they not consider anelastic effects in their Green's functions in this study?

We have now specified the viscosity structures and spatiotemporal sampling of Love numbers and Green's functions in detail [line 121-140]. Thank you for this comment – much appreciated!

Because the Adhikari and Caron (2024) dataset was computed for Maxwell rheology, the mention of a "seasonal" timescale merely implies that the method can capture elastic feedback signal. To avoid any confusion, we now stick with the "decadal to centennial" timescale throughout the manuscript, as we expect meaningful ice/Earth coupling at these timescales (rather than the seasonal timescale). Thanks for pointing it out.

11. L. 99ff: The discussion regarding the number of integrations comes a bit late, and results in a further problem which weakens the statement of 'ease' a bit. Can the author give realistic integration times? That would help the reader to estimate which further approximations have to be made.

The integration time is proportional to the mesh size, coupling interval, and simulation period. We will happily report example computational times for the self-consistent method that solves the sea level equation and for the proposed simplified method.

Conceptually speaking, for any mesh having x elements for the ice sheet domain and y elements for the global sea-level domain comprising the ice sheet, convolution takes longer for the self-consistent method because y > x, and, unlike for the ice load, the ocean load needs to be determined recursively (typically 5-7 recursions) via as many convolutions. That said, we believe "ease" in this context is not only about computational cost but also about forgoing the sea-level equation altogether if the goal is to capture leading-order feedback on the centennial timescale. Nonetheless, we have replaced "ease" with "relative ease", relative to solving the sea level equation [e.g., line 7 and the title].

12. L. 105: I would expect 'validate' instead of 'scrutinize' the proposed method.

Done. See line 202.

13. L. 113ff: I wonder why the authors do not consider the floating condition here. And is the rotational feedback only neglected in this experiment or everywhere. This is only clarified in the last paragraph of this study.

We do consider floating conditions when deriving ice loads. It is just that we ignore the ocean load. We have made it clearer in the revised text [lines 210-211].

14. L. 121: I would not translate 'minor differences' into 'high accuracy', but into 'sufficient accuracy'.

Agreed. We have revised the text accordingly [e.g., line 219]. As noted earlier, we propose to tweak the title, swapping "ease and accuracy" with "relative ease and sufficient accuracy."

15. L. 123: the neglect of ocean load is only insignificant when considering small changes in sea level.

Yes. This is indeed the case for shorter-timescale simulations. Now clarified on line xx.

16. Figure 3: I miss the error in R as this is the main quantity when coupling is considered.

R, by definition, is the difference between S and B [Equation 2, line 76]. Therefore, showing additional panels for R to an already busy figure will only add redundancy. We appreciate the comment, though.

17. L144: Although it becomes clear from the reference, a short note what PICOP means would help.

We have added a sentence to introduce the PICOP model better [lines 247-248].

18. L 147: A transient rheology is considered in the fully coupled setup. Does this mean this is also considered for the Green's functions applied for the simpler model, and so is also considered in the provided set of Green's functions, or alternatively additional transient effects are not necessary for the coupling.

Yes, we used the transient-rheology Green's functions (based on the same EBM parameter set as used in Houriez et al., 2025) for the simple method as well. The reason is that we chose to compare the proposed simplified method against the in-house state-of-the-art simulations of the coupled ice sheet/solid Earth/sea level system. We have added a Supplementary Figure [S3 on page 28] showing the Love numbers and Green's functions for the EBM model, including a comparison with corresponding Maxwell model.

19. L 153: They state that for the year 2350 AD a cumulated error bias only reaches 2%, for their experiment which is rather small. But may it not affect the timing of tipping points. Also, what about local effects, which are not represented in discussing the GMSL.

Our analysis suggests that the grounding line (GL) locations predicted by the two methods (i.e., self-consistent vs. our simplified solutions) are virtually on top of each other for a 350-year WAIS simulation (see Figure 5a). That said, we are aware that even a km-off prediction of GL near bedrock ridges may impact the timing of unpinning and subsequent GL retreat by a few years to decades (e.g., Larour et al., 2019; Houreiz et al., 2025). Consequently, as the reviewer noted, local effects may be larger than the reported GMSL bias. A comprehensive analysis along these lines is beyond the scope of this paper. We have cautioned readers in the revised text about the possibility of larger local biases [lines 259-262].

20. In Figure 5: I miss a comparison with LC or ELRA which are further coupled set ups.

Since the main goal of the paper is to present a simplified yet sufficiently accurate method for capturing the solid Earth feedback signal, we focused on comparing its performance against the self-consistent solutions of the sea level equation. Comparing our method against many methods that are out there -- although useful -- may not necessarily serve the main purpose of the paper. We appreciate the comment, nonetheless.

21. L 152ff: For the process I agree, that the effect is small. What about the impact on far-field aspects? Do we need a different model approach then? So, my impression is that the rotational potential is not considered and so, the degree 21 pattern in the far-field sea level is not modelled correctly. Furthermore, the ocean loading is not considered. Which results in an additional difference.

We have already addressed this issue (see our introductory response). The proposed method, by design, is not intended to capture relatively minor feedback signals (on centennial timescales) from ocean loads, far-field ice melting, migrating coastlines, and rotational effects. We acknowledged this on TCD lines 185-190. We have moved the text to the introductory paragraphs and further revised it [lines 44-65].

22. L 163ff: I read this between the lines: This approach does not allow a straightforward representation of a self-consistent GRD. Additional effort would be necessary to reach this, but for the considered case of analyzing centennial coupling and near field processes it is sufficient.

Agreed. We believe we have already addressed this issue (see our introductory response).

23. L. 166ff: Here it becomes more clearly, where the method is applicable. Nevertheless, I wonder regarding 'accurately' and 'high accurate'. That the method holds only when coupling on decadal to centennial, I agree. At L. 83 you state also 'seasonal', so be consistent.

Thank you. As noted earlier, we now use “sufficient accuracy” (rather than high accuracy) and “decadal to centennial” timescales (rather than seasonal scale) in the revised text.

24. L. 170: You capture the GRD by a number of simplifications, which you should summarize here in view of what terms do not have to be considered in the intended scenarios.

We have stated in the revised text [lines 278-280] that ocean loads (driven by local or far-field ice melting or by migrating coastlines) and rotational feedback can be neglected if the goal is to simplify the coupling strategy while capturing the leading feedback signal on the centennial timescale. We have also noted that, in applications where these processes are critical, one must solve the self-consistent sea level equation [lines 280-283].

25. L. 176: Why does the neglection of a number of processes result in a strategic advantage. Migration of grounding lines, and bedrock ridges, are rather important in the discussion of grounding line evolution, especially when reaching spatial resolutions of the ice sheet models.

We believe there is an important trade-off between a simplified (yet sufficiently accurate) method and a more comprehensive (but resource-intensive) method. The former approach encourages end users -- ice sheet modelers with a standalone system and limited resources and knowledge of solid Earth processes -- to account for a reasonably accurate ice/Earth feedback signal. In the absence of such approaches, end users may have to either invest in developing their own sea-level solver, collaborate with GIA modelers, or, in the worst case, abandon the coupled simulation altogether. Additionally, a simpler, less resource-intensive method enables end users to consider shorter coupling intervals, yielding more realistic ice-sheet evolution and projections, or run a larger ensemble of simulations, facilitating more robust uncertainty quantification. We have expanded on this in the revised manuscript [lines 287-294].

26. L. 178: Love numbers of radially symmetric viscoelastic earth models are also accessible with public domain software like TABOO. Can such sets be implemented in your setup?

Absolutely. The proposed method can handle Love numbers computed by any method. We have stated it explicitly, citing TABOO [lines 295-297]. Thank you for the comment.

27. L 182: Is the extension to more comprehensive linear rheologies necessary regarding the number of simplifications you apply and also the statement on L. 154, that the largest uncertainties are in the ice-sheet models themselves?

As noted earlier, the main meat of the paper concerns a simplified yet sufficiently accurate method for capturing solid Earth feedback. Although the method fails to account for effects of ocean loading and rotational feedback, it does not discriminate between linear rheological models (e.g., Maxwell, Burgers, EBM). We have clarified it in the revised manuscript [lines 295-305]. The quoted statement (TCD line 182) refers to enhancing the Green's function library with refined Earth structures and additional rheological models, which, in itself, is not problematic, whether more comprehensive rheology is adopted or not.

28. L.185ff: This final statement appears rather late to my understanding. It reads rather like a premise for the development of the code. So, I would expect this motivation and also the disclaimer at the beginning of the manuscript. This would make the discussion of the study much more transparent.

Agreed. As noted earlier (see page 1 of this rebuttal), we have moved up this text to the introductory paragraphs [lines 45-46, 57-65] and conclusions [lines 280-283].

Response to the reviewers of the second report.

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:

Thank you for a positive evaluation of our manuscript. The paper was written with the sole purpose of facilitating standalone ice sheet models to capture *leading-order* solid Earth feedback on the *centennial* timescale with *relative* ease. Kindly see our introductory response to the first reviewer (page 1 of this document) for explanation.

Any simplified methods come with limitations. The proposed method cannot capture self-consistency in GRD (gravity, rotation, deformation) processes, nor does it satisfy mass conservation in ice and ocean domains. For centennial-scale simulations of a realistic ice sheet, as shown in Figures 3-5 for Antarctica, these processes appear to be less important, as the overall effect of ocean load is much smaller than the direct effect of ice load. Consequently, the effect of farfield ice melting or coastline migration -- which primarily affects the ice sheet via a modulation of the ocean load -- is also expected to be of second-order importance on the timescale of interest. We acknowledge that long-term simulations (e.g., glacial cycles) require capturing all these features, for which there is no alternative to solving the sea-level equation.

We have conveyed these points by clearly articulating the scope and limitations of the proposed method in the introductory paragraphs [lines 44-65], abstract [lines 6-10], and conclusions [lines 274-283]. Additionally, we have revised the title to enhance transparency further [underlined words are new additions]: "Enable ice sheet models to capture centennial-scale solid Earth feedback with relative ease and sufficient accuracy."

Like any 1-D GIA models, the proposed method -- rooted in Green's function representation of the radially stratified solid Earth response -- has inherent deficiencies in capturing the effect of lateral mantle heterogeneity. The Antarctic mantle exhibits significant lateral heterogeneity (see Ivins et al., 2023, for example). And its impact on solid Earth/sea level feedback to ice sheet dynamics is expected to be much larger than that of ocean load. Gomez et al. (2024) show significant 3D effect on sea level projections, although the appropriate choice of a regionally averaged one-dimensional structure may partially mitigate this deficiency [lines 62-64].

[Additional thought: It is important to note that uncertainty in ice sheet processes, MICI vs MISI, leads to far greater discrepancy in sea level projection than the contribution from solid Earth feedback, let alone that of lateral mantle heterogeneity (see Figure 3 of the Gomez paper). In other words, given the large uncertainty in ice sheet models, capturing leading-order solid Earth feedback should be of primary focus than getting the solution right by accounting for additional 3D effects. This does not mean discouraging users from using the 3D Earth model that accounts for self-consistent GRD solutions; every effort should be made in this regard assuming expertise and resources are available.]

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.

We agree that we were a little naive in choosing solid Earth parameters and justifying these. We always thought that the main meat of the paper was the proposed method. Then the purpose of Figures 2 & 4 would be to demonstrate the method's performance across diverse scenarios, rather than to provide accurate computations of Antarctic solid-Earth response signals. Following the reviewer's comment, we conducted a thorough literature review to derive multiple parameter-sets applicable to various regions of Antarctica and Greenland [Table 1 in the revised manuscript on page 7]. And we now have a whole new section [Section 3], "Solid Earth Models," dedicated to this topic. Accordingly, we revise Figures 2 & 4: WAIS-earth (representing Amundsen Sea Embayment structure) has 60 km lithosphere thickness and 5*10^18 Pa s upper mantle viscosity, and EAIS-earth (representing the coasts of East Antarctica) has 120 km thick lithosphere and 5*10^20 Pa s mantle viscosity.

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.

As the main goal of the proposed method is to enable coupled ice/Earth simulations, it is indeed important to guide end users (i.e., ice sheet modelers) in selecting appropriate Earth structures for their applications (e.g., Antarctic basins or Greenland). We also note that, in addition to its applications to modern ice sheets, the method can be applied to centennial-scale simulations of paleo ice sheets or used to deepen process understanding through synthetic experiments.

A new section, "Solid Earth Models" [Section 3], has now been added to provide a more detailed discussion of the topic. This section includes a table [Table 1 on page 7] presenting recommended values and plausible ranges for lithosphere thickness and upper mantle viscosity in the Greenland and Antarctic regions. In our view, this addition has substantially improved the quality of the manuscript. We appreciate your insightful comments, which prompted this addition.

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.

We believe that this comment has been fully addressed above. Kindly see our responses to the first reviewer (page 1 of this rebuttal) and to your introductory comment (pages 8-9 of this document). Briefly, we have provided a clear description of the scope and limitations of the proposed method in the introductory paragraphs [lines 44-65], abstract [lines 6-10], and conclusions [lines 274-283]. We have also tweaked the manuscript's title to enhance transparency from the get-go.

Line by line comments:

- Abstract**:** Need to specify somewhere in the abstract that the proposed approach assumes 1-D / laterally homogeneous Earth structure.

  Agreed. We have made necessary changes [line 8]

- Line 64: the dashes in this line are slightly confusing, may be good to phrase this without the dashes.

  Not sure we fully registered the intent of the comment. Happy to resolve it in the next iteration if deemed critical.

- 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.

We follow the community convention, largely based on Gregory et al. (2019) . Following this reference, we have made a clear distinction/connection between mean sea level and the geoid [lines 74-78, 108-111]. We avoid using GMSL; we rather use barystatic sea level [e.g., line 227]. We believe the noted confusion has been resolved.

- 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

  We use G for geoid [line 108] and $\mathcal{G}$ for Green's functions [e.g., Equation 5, line 101]. These notations appear visually distinct. As already noted, a clear distinction is made between mean sea level, S, and the geoid, G, following the Gregory definition [lines 108-111].

- 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

  After careful consideration of this suggestion, we have determined that a dedicated table of symbols is not necessary. Most mathematical notation is restricted to a single dedicated section [Section 2], "Proposed Method," and does not appear frequently elsewhere in the manuscript. Nonetheless, should the reviewer consider that the inclusion of such a table would enhance clarity, we remain willing to incorporate it in the next iteration.

- Line 108: I think of Kendall et al. 2005 as a standard reference for the sea-level equation, might consider including

  Agreed. We have now included this citation [line 205].

- 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 Antarc5ca." *The Cryosphere* 16.6 (2022): 2203-2223.

  We have now cited these articles appropriately [e.g., line 285]. We appreciate your bringing them to our attention.

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.

  This has been stated explicitly in the revised caption [penultimate sentence].

- Figure 2: why is unit in meter per kilogram?

  Figure 2 shows the solid Earth's response to a point load, which is dimensionalized by a factor introduced on TCD line 215 [now line 330] (Appendix A). The noted units imply meters of VLM or geoid change induced by a kilogram of point mass.

- 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.

  As noted above, we have now included a new section, "Solid Earth Models," in the revised manuscript. In this section, we have provided a detailed description of the two models considered as examples [lines 176-182] and our recommended Earth parameters for several regions in Antarctica and Greenland [Table 1 on page 7]. We have also modified the figure caption accordingly [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.

  We have combined panels b and c to plot a single RSL map [now panel b], showcasing the global ocean load driven by ice mass change shown in panel a. We have updated the caption and the main text accordingly.

- 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.

  We have added Supplementary Figure S2 to illustrate the geoid fields for the two solid Earth models. In contrast to the VLM field, the EAIS-earth model yields larger geoid amplitudes by 2300, primarily due to its reduced compensating response to the geoid signal directly induced by ice mass change [lines 234-242]. This observation also motivated a revision of Figure 2b to explicitly separate the direct and induced geoid contributions. We believe these additions/modifications have improved the clarity and overall quality of the manuscript. We thank the reviewer for this helpful suggestion.

- 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.

  Thank you for this suggestion. After examining the 2100 results (see below), we did not identify any additional insights beyond those already conveyed by the 2300 results presented in the manuscript. We therefore chose not to include them. That said, if the reviewers still think these figures would add value, we would be happy to include them as Supplementary Figures in the next iteration.