We thank the authors for responding to our original review and we are overall happy with the revisions made to address these comments. We have minor comments that we believe will strengthen the manuscript.

We appreciate that the changes to the title make the scope of the paper more clear, and we appreciate the new articulation of the scope and limits of the proposed method. We believe the addition of a discussion on solid Earth models is a useful reference for ice sheet modelers aiming to better understand how variations in solid Earth structure translate into their estimates of bedrock deformation.

Given that this approach offers a simplification for ice sheet modelers, we believe it could be useful to add a sentence or two that explains any advantages to this proposed method compared to ELRA, a simplification for bedrock deformation often employed in ice sheet model. Could you enunciate in what ways it might be advantageous to adopt the proposed method? I can imagine that proposed method allows for better self consistency in comparing predictions using different Earth structures with full GIA models (as elastic/viscous parameters are not as to compare in ELRA to those used in GIA predictions using Maxwell rheology). Since the audience is intended to be ice sheet modelers, we feel like such a discussion would be appropriate and help modelers decide when to use this approach.

Line 69: in news limitations paragraph, please also include rotation as a process that is not included in this approach

line 135: the change to a curly G instead of a G for Green function is not an appropriate change in response to our comments as curly G is also a variable used in the sea level equation, denoting the geoid response to loading (not including delPhi/g). Instead this variable should be GF, to avoid confusion for those familiar with using the sea level equation.

Line 141: Lloyd et al. 2020 does not capture 3-D variability in the crust beneath Antarctica, additional references to papers that actually image crustal structure are needed, such as Chaput et al. (2014). Additionally, other studies that investigate solid Earth structure in Antarctica should be cited, including e.g., Chua & Levedev (2025), Hansen & Emry (2025), Lucas et al. (2020). This is not a comprehensive list of studies that image Earth structure in Antarctica, so add an “e.g.,” here.

Line 142: add reference to An et al. (2015) – “Temperature, lithosphere-asthenosphere boundary, and heat flux beneath the Antarctic Plate inferred from seismic velocities” along with the Wiens et al. (2023) and Brown & Fisher (2025) references.

Line 143: Add reference to Hazzard et al. (2023) – “Probabilistic Assessment of Antarctic Thermomechanical Structure: Impacts on Ice Sheet Stability” – which also characterized mantle viscosity using seismic constraints. Also reference Gomez et al. (2024) and Lucas et al. (2025), which both develop viscoelastic models for GIA modeling in Antarctica based on seismic imaging.

Line 159: Here, I think it needs to be specified that observational constraints on “lithospheric thickness and mantle viscosity from geodetic data” are strongest in …not “observational constraints on solid Earth structure”

line 162: I don’t feel it is appropriate to use bullet points for this section. Text should be written out in paragraph form. It is also not necessary to include information that is in the reference on love numbers. It feels like a lot of this information might be repeated from Adhikari & Caron (2024). Please reference the paper whenever that is the case for ease of readability!

Line 167: do you mean “Amery” by “Emery”?

Line 315: awkward phrasing, should be ability to, not ability in

Please include the figures shown for 2150 in the supplement. It seems like a strong point that your method accuracy is better for 2150 compared to the 2300 figures included in the main text. You could highlight that method accuracy degrades with larger ice loading changes, and therefore your predictions are more accurate in the nearer future.