General comments

Capturing gravitational, deformational and rotational (GRD) effects of the solid Earth in response to ice-sheet evolution has been known to be important for ice-sheet model simulations for the paleo and future timescales. Of these, bedrock response (i.e., deformational effects) play the most dominant role on ice-sheet evolution. Bedrock deformation can be computed with different models of complexities and computational expense, and coupled ice sheet – 3D GIA models remain to be the state-of-the-art.

However, not many ice-sheet models have been coupled to bedrock deformation that incorporate 3D (radially and laterally varying) Earth structure. Many other models incorporate more simplified bedrock models, such as ELRA, LVELRA, or 1D (radially varying Earth structure) GIA models. Thus, it could be beneficial to provide suitable parameter values to those ice sheet models that incorporate simpler and cheaper bedrock models that would best resemble ice-sheet contribution to sea level projected by state-of-the-art coupled ice sheet – 3D GIA models.

In their manuscript "Approximating ice sheet – bedrock interaction in Antarctic ice sheet projections", van Calcar et al. attempts to provide suitable solid-Earth related parameters to the structurally different type of bedrock deformation models – ELRA, LVELRA, 1D GIA model. The authors simulate Antarctic Ice Sheet for 500 years into future with a coupled Antarctic Ice Sheet – 3D GIA model as a benchmark for the other three bedrock models, using two different climate forcing from two different climate models. ELRA models incorporate parameters such as flexural rigidity and a relaxation time and GIA models incorporate parameters such as lithosphere thickness and mantle viscosity, and the authors first empirically derive 2D maps of relaxation times informed by the 3D Earth structure. They then provide parameters for ELRA, LVELRA and 1D GIA models that ice-sheet modelers can use in their projection of Antarctic Ice Sheet for the time period.

This manuscript undoubtedly contains a great amount of work with creativity. It also has in mind a meaningful contribution to the field of future ice-sheet modeling – to provide constrained parameter values to the ice-sheet modelers who do not have access to the most sophisticated, state-of-the-art 3D GIA model. This contribution can also potentially be very useful for the next round of Ice Sheet Model Intercomparison community effort, ISMIP7. Therefore, I think the topic of the work deserves eventual publication in The Cryosphere. However, before the acceptance of this manuscript, I recommend major revision in text and analysis with regards to few points outlined below.

We would like the thank the reviewer for the valuable feedback and the recognition of the importance of our manuscript to the field of ice-sheet modeling.

1. The main issue I have with the current form of the manuscript is the limited exploration and discussion regarding the sensitivity of the derived parameter values to different climate forcing from different climate models. It is known that even for the same high-

emission scenario (e.g., RCP8.5 & SSP5-8.5), different climate models provide drastically different thermal forcing and surface mass balance, resulting in a wide range of uncertainties in Antarctic Ice Sheet mass loss and region of mass loss (e.g., Seroussi et al., 2024). To this argument, I suspect the reason why the authors derive ELRA relaxation time of 300 years for pre-2300 and 500 years for post-2300 has to do with change in the location of ice mass loss (West vs. East Antarctica). Thus, I am uncertain how applicable these results would be when ice-sheet models apply climate forcings from models other then CESM and IPSL used in this study. I'm not necessarily asking for a wide range of ensemble simulations, but there at least needs to be extensive discussions around this topic, which might potentially require some more simulation/analysis addressing this point.

We agree that there are more climate models available and used in the study by Serrousi et al. 2024, but our goal is to focus on a variety of GIA related aspects in the context of climate change. For this reason we have used two climate models which are widely varying in there patterns. They include warming in key regions such as the Amundsen Sea, the Ross Sea, and the Weddell Sea, covering the primary areas of ice loss, while differing significantly in warming intensity. Thus, our choice for these two models ensures that we capture different forcing magnitudes, spatial warming patterns, and long-term projections. We acknowledge that different climate models provide significantly different thermal forcings in terms of both magnitude and spatial distribution. which might not be captured by these two models. Therefore, we will extend the discussion of possible effects of unknown climate forcing to highlight this issue, but again repeat that our aim is not to come to the best possible projection, but to focus on the importance of GIA in the context of warming.

Regarding the changing relaxation times, we note that this is primarily driven by the evolving location of ice mass loss over time that will occur regardless of the climate models. For example the grounding line retreat in the Amundsen Sea Embayment initially occurs over regions with relatively low mantle viscosity. As retreat progresses further inland, the viscosity increases, leading to a change in bedrock response characteristics. This effect is thus not related to differences between East and West Antarctica, but to the retreat within one basin in which ice loss is expected to occur in the near future regardless of which emission scenario is used. Furthermore, the high emission scenario covers a larger area of ice mass loss, triggering deformation deeper in the mantle where the viscosity is different from the viscosity at shallower depths. This explains the change in relaxation time with timescales for the high-emission scenario which also holds for a different climate forcing used as long as comparable ice thickness changes are induced. This rationale is discussed in lines 311-314 and 321-323 of the manuscript, but we will clarify it further and include it in the conclusions.

2. The authors create a bound of benchmark sea-level rise values based on the 1) difference between 3D-Average and 3D-weaker and 3D-stronger GIA model results and seems to then hand-wavily pick lines that sit between these bounds for the longer period. I am not sure if this is a good metric and a way of choosing recommended

values. For instance, how is relaxation time 200 years in Fig. 4c not a good choice compared to relaxation time 300 in Fig. 4c when the difference between 3D-Average and relaxation time 200 years is as small as the difference between 3D-Average and 3D-weaker? A way around could be to provide a range of recommended values rather than a single value for each bedrock model for different time periods.

We acknowledge that the criteria for selecting the best-fitting relaxation time are not sufficiently clear. For each ELRA simulation, we will quantify deviations from the average response of the two 3D models across all scenarios and climate models to determine the best fit. We will clarify this methodology. We also agree that a range of recommended values is better. We find that a range of 275 to 375 years is suitable and will change the text to recommend 300 years as the optimal choice considering both scenarios and climate models.

3. Section 3: How are the choice of four regions in West Antarctica made, and can this choice be well-justified? Given that ice in North Antarctic Peninsula and Ross Sea Embayment seem particularly insensitive to bedrock response, wouldn't it be more accurate to use just ASE or ASE and WSE to derive the 2D map for LVELRA where most grounded ice mass loss occurs in realistic simulations? Also how is East Antarctica treated where the idealized unloading test is not performed?

The aim was to have a robust relationship between relaxation time and viscosity that is valid for any viscosity map without a priori constraints on where ice loss is exactly taking place. Therefore we aimed to include as much variation in size and location of the ice load and underlying viscosity as possible while balancing computational constraints. Although the four regions did not include a region in East Antarctica, the range in viscosities and relaxation times is very large and includes plausible viscosities in East Antarctica in locations where mass loss might occur. Having an extra loading scenario would add a few points to figure 2b but likely not change the linear relation between viscosity and time scale significantly. This is especially so because we observe reduced sensitivity to viscosity variations at higher values that are more likely in East Antarctica (Fig. 2b).

Can you improve your 2D ELRA map in Victoria Land and Mertz basins in East Antarctica so the difference between the 3D GIA results and 2D ELRA improve?

In those basins, the 3D GIA model results in greater uplift than the 2D ELRA model. The 2D ELRA map might therefore be improved for Victoria Land and Mertz basins by decreasing the relaxation time in those basin. The goal of this study was to provide one relationship between viscosity and relaxation time that works best for the whole of Antarctica, but studying region specific relaxation time alterations would be very interesting. We will add this to the discussion.

Also, how is the 500 m-thickness for ideal loading chosen? Please provide some discussion around how your results would be sensitive to the choice of this ice thickness.

Regarding the choice of 500 m ice thickness for the idealized unloading test, this thickness was selected to approximate stress changes comparable to those expected in realistic ice loss scenarios. The wavelength of the ice load is more influential than its thickness, as per normal mode theory, so we prioritized including sensitivity to loading wavelength by varying the size of loading areas. We will further clarify why wavelength of the load is more important than magnitude and the choice for 500 m in the manuscript.

4. The manuscript will benefit significantly from more extensive literature review, giving credits to those deserve, and being more appropriate/consistent in the citation scheme. Detailed comments are main in the following section.

We appreciate the reviewer's recommendations and will incorporate the suggested references where appropriate to ensure a comprehensive and representative citation scheme.

5. The manuscript could benefit from more careful proofreading for the next round of submission.

We acknowledge the need for careful proofreading and will implement de reviewers' detailed textual comments and ensure that the revised manuscript is polished and free of inconsistencies.

I realize these comments may seem a lot to address, but I hope they are helpful in improving the manuscript and getting it published in the journal. I am wishing the best to the authors.

We would like to thank the reviewer for providing valuable conceptual and detailed feedback.

Specific comments: addressing individual scientific questions/issues.

1. L25-27: The authors attribute the differences across different bedrock models only to the spatial variations in viscosity. But some of the differences arise due to structural differences in theory in each bedrock model. This needs to be clarified in the motivation and discussion. We will clarify this in the introduction and the discussion.

2. The "negative" or "stabilizing" feedback on ice mass loss has first been introduced in the context of "sea level", which includes both bedrock uplift and sea surface height drop. I would recommend the authors to introduce "sea-level feedback" as a general concept, and then explicitly say that they choose to omit the sea surface height component in their investigation; I would like to confirm this, by the way - is it actually true that you only feed the bedrock changes back onto the ice model in the case of using 1D and 3D GIA models, not sea-level change?. If so, you should refer to previous studies that explored the separate impact of deformational and gravitational effects on ice evolution to mention that deformational effects are the dominant portion of sea-level feedback. (e.g. Gomez et

al. 2015, Han et al. 2021, Coulon et al. 2021). We will introduce the term "sea-level feedback" and confirm that only bedrock changes are returned to the ice sheet model. We have performed a sensitivity test for the effect of sea-level changes in van Calcar et al. (preprint 2024) and will repeat the conclusion in this manuscript. Furthermore, we will refer to the suggested references.

3. Provide in detail how sea-level change is calculated. Does it correct for changes grounded ice thickness (or volume) due to bedrock deformation captured in different bedrock models? We will include a detailed description of the sea level change calculation which involves computing the volume above floatation and division by the ocean area.

4. Some statements are introduced as if they are general "facts" when they are not. For example, in Abstract Line 11, there is a sentence as follows: "... accounting for the impact of bed deformation on ice dynamics can reduce predictions of future sea level rise by up to 40% in comparison with scenarios that assume a rigid Earth." This is not an established fact, but rather depend on strength of climate forcing, sensitivity of the ice-sheet model to climate forcing and bedrock topography change. Therefore, instead of citing a number from a specific study, being general would be more helpful. We agree that the impact of the GIA feedback is highly variable. We stated the reduction can be up to 40%, implying a range. We will add to the text that the impact is highly variable depending on forcing and Earth structure, but we think that it is useful to keep the upper limit to emphasize the significance GIA might have in projections.

5. L14: "Because modelling the response for a varying viscosity is complex, sea level projections often exclude the Earth's response,..." This is a very convenient and even a naive way of explaining why ice-sheet models that project the future have been using fixed bedrock, let alone spatially varying viscosity. Until relatively recently, bedrock deformation (or GIA) used to be considered not very important for the "short", centennial time scale ice-sheet evolution, which future projections focus on. This very same comment applies to L49 as well. We agree and we will emphasize that GIA had until recently not yet been shown to be an important feedback on the time scale of hundreds of years, and therefore was not implemented with a 3D Earth structure.

6. L15: ... or apply a globally constant relaxation time or viscosity." It may not seem clear to many people what the differences you mean here by using the words "constant relaxation time" or "viscosity". You can make this clearer by specifically referring to the use of simplified bedrock adjustment models such as ELRA or 1D GIA models. We understand the confusion and will move the mention of ELRA and 1D GIA models in lines 19-20 to line 15.

7. L24: "... that deviates less than 40cm from the average of the 3D GIA models… can be further reduced to 10 cm". The specific numbers 40cm & 10cm do not mean much. It would be helpful to express the errors in a relative number, such as percentage. We will

provide the percentages throughout the whole document.

8. L34: "... requiring thousands of simulations to produce robust projections of potential sea level rise over the coming centuries." Seroussi et al. (2020) goes up until 2100 only. Seroussi et al. (2024) goes up until 2300. The ice model ensemble simulations in Seroussi et al. 2020 & 2024 are on the order of hundreds, not thousands. Maybe just say "ensemble simulations". We will change "thousands of simulations" to "ensemble simulations", and we will refer to the 2024 paper.

9. L34-35: I would recommend citing only the seminal papers that first showed stabilizing sea-level feedback (e.g., Gomez et al., 2010; 2012). Otherwise, you are missing references that also confirm the effects in addition to the ones being cited. We will refer only to the seminal papers for the stabilizing feedback.

10. L47-48: "However, it is currently unfeasible to include a realistic Earth structure in a large ensemble of sea level projections due to the long computation time involved (van Calcar et al., 2023)": This statement could be potentially out of context and misleading. The previous work the first author they cite here simulates the Antarctic Ice Sheet over the last glacial cycle, which is for hundred thousands of years. Here we are talking about the next few hundred years of projections, which would be feasible. Also, it could be appropriate to acknowledge other studies that showed ways to overcome computational infeasibility for long simulations, e.g., de Boer et al., 2014, Han et al., 2022, which are for 1D GIA models and thus could be debatable whether they are "realistic" models or not. Also, there is a study that emulates and thus provides large ensemble for 3D GIA model (Love et al., 2024). We will change the word "realistic" to "3D" in the manuscript to exclude any confusion about this term. We will clarify this sentence by stating that it is unfeasible to include a radially and laterally varying Earth structure in a large ensemble of sea level projections using a dynamic ice sheet model. Furthermore, we will discuss studies to overcome computational infeasibility that can be used to compute bedrock deformation coupled to an ice sheet model for projections, which are Han et al. (2022) and Swierczek-Jereczek et al. (2024).

11. L50-51: ELRA models are not only computationally cheap, but easier to implement because you don't need to "couple" a whole new model that is complex by itself. Also, I believe 1D GIA models are cheap enough to produce large ensembles. The claim that 1D GIA models are too expensive to produce ensembles for centennial time scale runs, needs evidence. We agree with the ease implementation as argument why ELRA models are commonly used and we will include this in the text. We also agree that 1D GIA model can be fast enough to be used in ensemble studies and our manuscript doesn't state that 1D GIA models are too expensive for centennial time runs. On the contrary, we studied which parameters can be used in the 1D coupled model to approximate a 3D coupled model because 1D GIA models are currently being used in ensemble studies. The manuscript already states the following in lines 384-385: "As 1D GIA models can also be considered intermediate in terms of computation time compared to ELRA and 3D GIA" and we will add

here that 1D models are thus suitable to be used in ensemble projections.

12. L79: The authors should cite the work of Jan Swierczek-Jereczek who developed the FastIsostasy model that captures lateral variations in lithosphere thickness and mantle viscosity. It is a critical reference that needs to be acknowledged. We acknowledge that there are different models to simulate bedrock deformation for the glacial cycle, of which FastIsostasy is one, but in this study we compare the currently used methods to include bedrock deformation in ice sheet models in projections, which are a rigid Earth, ELRA and 1D GIA, to the results of using a 3D GIA model. We will discuss this motivation more extensive in the introduction and place the various models in context of available methods by also discussing FastIsostasy . Please see our response to comment 10 for details on how we will discuss Swierczek-Jereczek et al. (2024).

13. L101: it is confusing to now refer ELRA and LVELRA to as "GIA models". We agree and will change it to "Earth model".

14. L153: what is the value of the lower mantle viscosity? We will add that the viscosity of the lower mantle is assumed to be $5*10^{21}$ Pa s.

15. L174: What is Configuration 1 like? How specifically is it different from Configuration 2 rather than the former is "variable resolution". Variable resolution in what? The variable resolution refers to the high resolution area over Antarctica. We understand the current wording in the manuscript is confusing. Configuration 1 is described in lines 149-155. We will add in line 155 that that configuration is referred to as configuration 1 and will remove this from line 174.

16. L179: "... procedure as described for configuration 1.". Nothing has been described for configuration 1. More detail would be helpful. The change suggested in point 15 of this review will clarify what is meant by configuration 1 and the details are described in lines 149-155.

17. L182-183: So, both bedrock deformation and sea surface height changes are calculated. Are you then only passing bedrock deformation back to the ice model? Whichever way, this needs to be made clear. This configuration of the 3D GIA model is used standalone with prescribed ice unloading. This was only mentioned later in the text and we will now clarify that here already.

18. L213: Mitrovica et al. 2011 shows laterally varying flexural rigidity has negligible effects on bedrock deformation? Also, is the citation order following the year of references or the alphabetical order of authors' last names? Mitrovica et al. 2011 state: "our predictions based on 3-D elastic earth models indicate that lateral variations in elastic Earth structure need not be included in future studies". We will make sure the in-text references are in chronological order consistently throughout the paper.

19. L225: How are the "large" and "small" areas chosen based on what metric? The small area is chosen to cover the main area of mass loss close to the present-day grounding line. The large area is chosen to cover the full basin of the embayment, and the full peninsula. We will clarify this in the text.

20. L226: "... such that the resulting empirical relationship between mantle viscosity and relaxation time accounts for the different mantle conditions…". What does it mean by the relationship accounts for the "different mantle conditions"? Isn't the mantle condition the same but the surface load size is being varied? Clarify the sentence. Because the GIA model has a laterally and radially varying structure the mantle conditions differ per region chosen. By having schematic ice loads in various locations, we sample the structure in different ways, obtaining a relationship that is supposedly valid over a large viscosity range. We will clarify this in the text.

21. L228: Do you mean "each surface load" as in surface load on each grid cell or of each basin or each small/large area? I presume the latter but clarify. And what does it mean by "surface load is controlled"? We will change this sentence to: "The resolution of the 2-degree finite element mesh that is used in this configuration of the 3D GIA model is relatively coarse and therefore determines the shape of each area of loading."

22. justification on why this thickness was chosen. Given that the marine-based portion of the West Antarctic Ice Sheet goes up to 2-km thick, and that most of the ice melting will happen in Amundsen Sea Embayment, 500m seems quite arbitrary and out of place. It would be also good to mention how sensitive the viscosity-relaxation time relationship would be to the choice of ice thickness. Please see our answer to general comment 3.

23. L223-224: "This implies that the relaxation time depends on the size of the ice sheet and that a single relaxation time cannot be derived". Clarify this sentence, as the current version is confusing as to what the difference is between "the relaxation time" and "a single relaxation time". Something like, "a single relaxation time" that best represents those integrated over all modes." To clarify, we will change this sentence to: This implies that the relaxation time that is experienced by a certain ice load depends on the size of the ice (un)loading. Thus, a single, uniform relaxation time fixed in time cannot be directly derived.

24. L241: how is "the Earth" different from "the mantle" in this context? Does it make sense to say "the region of the mantle….depends on the sensitivity of the Earth…"? The current sentence sounds like the mantle is sensitive to its own viscosity, which is confusing. To clarify, we will change this sentence to: "However, the actual region in the mantle that dominates the Earth's response depends on the sensitivity of the ice load to the viscosity in the sub-surface, which depends on the viscosity profile itself.

25. L250&251: Explicitly mention that these equations are derived based on your fit shown in Fig. 2b. Also, I think it'd be more helpful to provide a general equation and then say you get different values for the average-viscosity case and the lower-bound viscosity case. This is mentioned in lines 247-249. As suggested by the reviewer, we will combine equations 8 and 9 to the following general equation:

$\tau = a \cdot 10^{-b} \eta_{\mathrm{eff}}{}^{c}$,

where a is 2.3, b is 5, and c is 0.35 in case the average viscosity used. When the lower bound viscosity is used, a is 3.9, b is 2 and c is 0.20.

26. L255: So which method do you take? High-resolution viscosity profile or smoothing the 2D maps? We will add to the text that we apply smoothing to the 2D maps.

27. L232: Specifically, how many timesteps of how many years? We will add the following to the method section: "Each simulation contains 20 timesteps, of which the first time step is 15 years, increasing by a factor of 1.5 until the largest time step of 33.3 kyr."

28. L224: Table 1 is missing from Supplemental Material. We will add the table to the materials section.

29. L231: Provide more information for the 40 simulations. Within what range of the mentioned parameters are varied? We will change the sentence to: "A total of 40 simulations are conducted, using a grain size of 1, 4 and 10 mm, a water content of 0 and 1000 ppm and a small, medium and large of the region of loading (as shown in Fig. 2a)."

30. L265: Do you mean Supplemental Material not Extended Data? Also, Table 1 is missing. Yes, we mean supplemental materials and we will add the table to the materials.

31. L267: "... 3D-weaker and 3D-stronger Earth models described in section 2.1". Please provide more detailed description on these 3D models in Section 2.1. Lines 140-144 provide an explanation on the 3D Earth structures and we will add the following sentence: The 3D-weaker structure contains a water content of 400 ppm and a grain size of 2.5 mm, and the 3D-stronger structures contains a water content of 200 ppm and a grain size of 4.5 mm.

32. L301-303: It seems like these recommendations would work well for the low-emission scenario, but for the high-emission scenarios these choices seem hand-wavy. For example, how is tau = 300 years any better than 350 in Fig. 4b? More importantly, there needs to be a discussion as to how these recommended values can change when applying climate forcings from different climate models. In a similar context, there needs to be a discussion as to why the recommended values change depending on the period (before 2300 vs. after 2300) in relation to the region of ice melting. It looks like the dominant mass loss happens in West Antarctica until 2300, so relaxation time that best

represents bedrock response in West Antarctica will match better, and after 2300 ice in East Antarctica also goes away, and therefore higher relaxation time that reflects slower bedrock response, resembling East Antarctic response, will work better. Please see the response to general comment 1.

33. L314: It would be helpful to use a relative number (e.g., %) rather than absolute values. This comment applies throughout the manuscript. We will mention relative numbers throughout the manuscript.

34. Fig. 5 & Supp. Figs. 2,4,5: It would be helpful to see some comments regarding the strikingly similar grounding line contours across all GIA models. Also, it would be helpful to show these plots for year 2300 where there will be actual differences in both ice thickness and grounding line extent in West Antarctica. We will explain in the text that the grounding line is similar for most of Antarctica because most of the AIS is stable over time. GIA only has an effect where there is mass loss, which is, for example, mainly in the Amundsen Sea Embayment in the case of the IPSL climate model. Supplemental Figure 3 shows the difference in terms of grounding line position, ice thickness and bedrock elevation between the ELRA300 and the two 3D Earth structures for the year 2300 when using IPSL. We will add the same figure but for the CESM climate model.

35. It looks like most of the differences after 2300 across simulations come from Victoria Land and Mertz in East Antarctica. I'm curious why 2D ELRA models perform equally poorly compared to the 1D GIA and 1D ELRA models? This goes back to the question asked earlier on what kind of treatment is done for deriving relaxation time based on viscosity in East Antarctica given the ideal unloading tests were performed only in West Antarctica. As explained in the response to general comment 1, most of the difference in sea level change after 2300 between different Earth models does not come from East Antarctica but from West Antarctica. This is described in lines 307-311 and we will clarify that the most important effect occurs in West Antarctica. Furthermore, two of the 2D ELRA models perform much better than the uniform ELRA model for some cases, of which the best performing model is 2D Stronger 120 using the relation between the relaxation time and the average viscosity (brown dashed lines in Fig. 6). This is described in lines 364-370 stating "For the high emission scenario, the sea level rise is about 30-40 cm closer to 3D-Average at 2500 using 2D-stronger compared to using ELRA300 (Fig. 6a-b). The advantage of using 2D-stronger over ELRA300 is particularly great in the Amundsen Sea Embayment projections (IPSL) for scenarios longer than 400 years because the difference between 3D-Average and ELRA300 increases strongly after 2300, whereas the difference between 2D-stronger and 3D-Average is constant over time (Fig. 6a)."

Lines 393-396 describe that 1D19 performs better than the best fitting 2D ELRA model: "The largest improvement of 1D19 compared to ELRA300 and 2D-stronger is in the bedrock uplift. The bedrock elevation of 1D19 in 2500 differs by a maximum of 80 meters from the results of the 3D GIA modelling in the high emission scenario, which is significantly smaller than the difference of 250 m when 2D-stronger is compared with the 3D GIA model output (Supplemental Fig. 5).”

We will the following explanation of why that is: “This improved agreement is explained by the more complete representation of Earth rheology in the 1D GIA model compared to the ELRA approach. While ELRA prescribes a simplified elastic lithosphere and a purely local, exponential relaxation toward isostatic equilibrium, the 1D model captures the full viscoelastic response of the Earth, including both elastic and time-dependent viscous deformation. Although it does not account for lateral variations in Earth structure, the 1D model still resolves the mantle’s dynamic flow in response to loading, bringing it closer to the behavior captured in 3D GIA models.”

Technical comments:

1. ”Approximating ice sheet-bedrock interaction in Antarctic ice sheet projections”. I think the title can be improved by being more specific. We will change the title to: “Approximating bedrock deformation of a 3D Earth in an ice sheet model for Antarctic sea-level projections”.

2. Antarctic ice sheet => Antarctic Ice Sheet (throughout the manuscript) We will do so.

3. L20: conducted => conduct. (or make the tense consistent throughout) We will do so.

4. L23: sea level rise => sea-level rise We will do so.

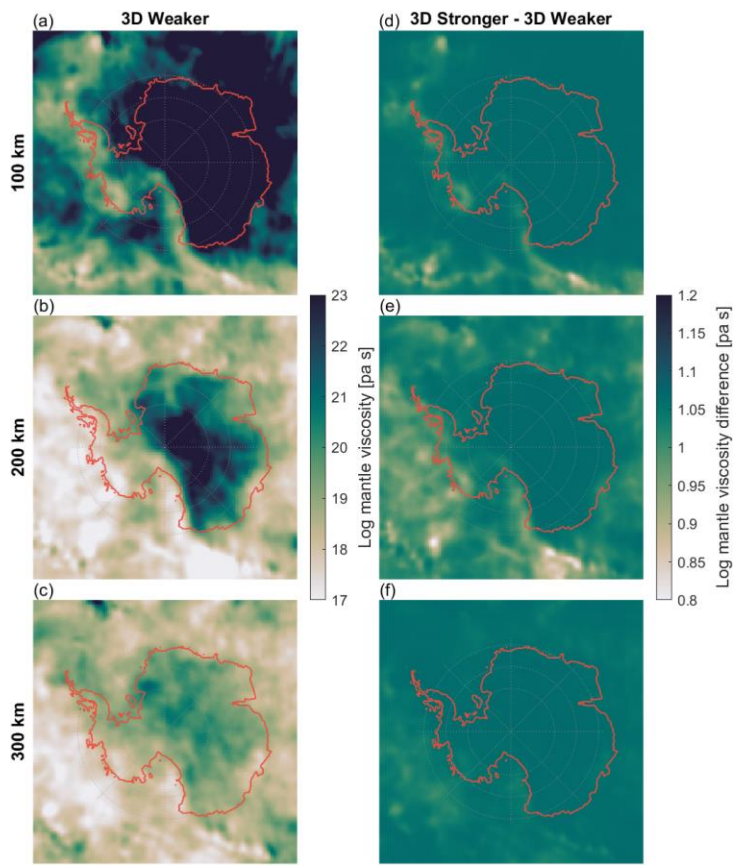
5. L56: “A GIA model can include the bedrock response to changes in ice loading…”. Remove “can”. GIA models by default should include bedrock response. We will do so.

6. Define GIA earlier in the text. Right now, it’s only introduced in L59 after the word has been used many times interchangeably with bedrock response. We will do so.

7. L84&86: “GIA model using a 3D Earth structure” -> 3D GIA model We will do so.

8. L103: “table 1” => Table 1 We will do so.

9. Table 1: explain the values and units used for the different Earth models. What maximum and minimum values do 3D-stronger and 3D-weaker models have? It would be helpful to describe those rather than referring readers to the other paper, van Calcar et al. (2024). We will add in the caption that the years described for the uniform ELRA model are the relaxation times and add the different flexural rigidity that we tested. We will also describe in the caption that 1D19 refer to an upper mantle viscosity of 10^{19} pa s, etc. We think that it would be useful to show spatial plots of the viscosity at multiple depths from which the maximum and minimum values can be read. We will the add the figure below, which is taken from van Calcar et al. (2024), to the supplemental material.



10. In Figure 1, you have a legend 1DASE, but in the table you have 1D18. We will adjust this.

11. L117: What does it mean by "realistic" sea-level projections? We understand that is can be interpret differently and will remove the word "realistic" from the sentence.

12. L159: Earth structures => Earth structure We will adjust this.

13. L169: Fig 1 => Fig. 1 We will do so.

14. Figure 1 legend: 1D Earth structures => 1D Earth structure profile? We will do so.

15. L214: use consistent significant figures. We will do so.

16. L217: I think it would be beneficial to have a more specific title for this section, something like, "Deriving 2D relaxation time maps from 3D viscosity profiles" We will change the title.

17. L223: "This implies that the relaxation time .....cannot be derived". This sentence feels redundant, as it should be very clear from the sentence before. But this is a minor comment. Please find our response in comment 23.

18. L229: "The uniform thickness of each load is taken to be 500m". There needs to be . The sentence of the reviewer is not finished but we assume the reviewer refers to the same question as comment 3 in the general comments section. Please find our response there.

19. L285: ELRA model with uniform relaxation time? We will adjust the title of this subsection.

20. L286-288: This sentence can be clarified. Strictly speaking, bedrock response is dependent on ice loading and viscosity. You could probably mention climate forcing then affects ice loading changes. We will change this sentence to: "The bedrock response depends indirectly on the climate model because varying ocean warming cause different regions of ice retreat, and the mantle viscosity differs in each region, and the sensitivity to the Earth structure depends on the size load."

21. L290: This whole paragraph is quite confusing and don't seem necessary. It is clear the coupled ice sheet – 3D GIA model results differ from ELRA models (or 1D GIA models), and the difference in the results will vary with climate forcings (which are in turn different for different climate models), which is also clear. This paragraph explains the indirect dependency of the impact of the GIA – ice sheet feedback on climate forcing and emission scenario, which is due to lateral variations in the mantle and due to different size of regions of ice mass loss. This information is necessary to understand the results and we therefore think it is important to keep this paragraph in the text for readers outside of the GIA community.

22. L279: Unindent We will do so.

23. L350: ELRA model with 2D laterally varying relaxation time? We will adjust the subtitle.

24. Figure 6: Some lines go out of bound in the y-axis. To maintain consistency across all figures, we have chosen to use the same y-axis limits, allowing for a more direct comparison between them. As a result, some lines in Fig. 6 extend beyond these limits. However, our primary focus is on the lines closest to y = 0, as they provide the most relevant insights. The lines that exceed the y-axis bounds already show significant deviations from y = 0 early in the simulation, indicating that certain parameter combinations do not perform well. Since these deviations are evident at earlier time steps, the loss of visibility at later stages does not compromise the key interpretation. Expanding the y-axis range to display the full extent of all lines would reduce clarity in the more relevant portions of the figure. Therefore, we have opted to maintain the current limits to prioritize visibility where it is most informative.

25. L380: The references cited here seem quite arbitrary and incorrect (Whitehouse et al., 2019 is a review paper, not an ice-sheet model.) The list of references was indeed incomplete, we will add Klose et al. (2014), Golledge et al. (2015), Gomez et al. (2015), and Rodehacke et al. (2020) to the list, and remove Whitehouse et al. (2019). The reference to Klose et al. (2014) was also missing in the introduction so we will add it there as well.

26. section => Section We will adjust this.

27. configuration => Configuration We will adjust this.

28. L408: accuracy in the metric of ice volume change. We will adjust this.

29. L409: for two different emission scenarios from two different climate models. The emission scenario is forced on the climate model and does not follow from the climate model. To improve clarity, we will add that we used two climate models and two scenarios and will change it to the following: "for two climate models under two emission scenario's".

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