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.

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.

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.

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 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. Also how is East Antarctica treated where the idealized unloading test is not performed? 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?

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.

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

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.

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.

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).

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?

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.

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.

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.

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.

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".

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.

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 (val 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)

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.

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.

13. L101: it is confusing to now refer ELRA and LVELRA to as "GIA models".

14. L153: what is the value of the lower mantle viscosity?

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?

16. L179: "... procedure as described for configuration 1.". Nothing has been described for configuration 1. More detail would be helpful.

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.

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?

19. L225: How are the "large" and "small" areas chosen based on what metric?

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.

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"?

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.

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."

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.

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.

26. L255: So which method do you take? High-resolution viscosity profile or smoothing the 2D maps?

27. L232: Specifically, how many timesteps of how many years?

28. L224: Table 1 is missing from Supplemental Material.

29. L231: Provide more information for the 40 simulations. Within what range of the mentioned parameters are varied?

30. L265: Do you mean Supplemental Material not Extended Data? Also, Table 1 is missing.

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.

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.

33. L314: It would be helpful to use a relative number (e.g., %) rather than absolute values. This comment applies 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.

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.

Technical comments:

1. ”Approximating ice sheet-bedrock interaction in Antarctic ice sheet projections”. I think the title can be improved by being more specific.

2. Antarctic ice sheet => Antarctic Ice Sheet (throughout the manuscript)

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

4. L23: sea level rise => sea-level rise

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.

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.

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

8. L103: “table 1” => Table 1

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).

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

11. L117: What does it mean by “realistic” sea-level projections?

12. L159: Earth structures => Earth structure

13. L169: Fig 1 => Fig. 1

14. Figure 1 legend: 1D Earth structures => 1D Earth structure profile?

15. L214: use consistent significant figures.

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”

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.

18. L229: “The uniform thickness of each load is taken to be 500m”. There needs to be

19. L285: ELRA model with uniform relaxation time?

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.

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.

22. L279: Unindent

23. L350: ELRA model with 2D laterally varying relaxation time?

24. Figure 6: Some lines go out of bound in the y-axis.

25. L380: The references cited here seem quite arbitrary and incorrect (Whitehouse et al., 2019 is a review paper, not an ice-sheet model.)

26. section => Section

27. configuration => Configuration

28. L408: accuracy in the metric of ice volume change.

29. L409: for two different emission scenarios from two different climate models.

References

1. deBoer et al., ice-sheet–sea-level model: algorithm and applications, GMD, 7, 2141–2156
2. Love et al., 2024., GMD. A fast surrogate model for 3D Earth glacial isostatic adjustment using Tensorflow (v2.8.0) artificial neural networks
3. Seroussi et al., 2024 ISMIP6 Antarctica 2300
4. Han et al., 2021. Modeling northern hemispheric ice sheet dynamics, sea level change and solid earth deformation through the last glacial cycle
5. Han et al., 2022. Capturing the interactions between ice sheets, sea level and the solid Earth on a range of timescales: a new "time window" algorithm
6. Jan Swierczek-Jereczek.,2024. GMD. FastIsostasy v1.0 – a regional, accelerated 2D glacial isostatic adjustment (GIA) model accounting for the lateral variability of the solid Earth
7. Gomez et al., 2015. Sea-level feedback lowers projections of future Antarctic Ice-Sheet mass loss
8. Gomez et al,. 2012. Evolution of a coupled marine ice sheet- sea level model
9. Gomez et al., 2010. Sea level as a stabilizing factor for marine-ice-sheet grounding lines.