Introduction

The authors made very clear that the target of the present article is to recommend:

1. a uniform relaxation time scale to use in ELRA,
2. a laterally-variable relaxation time scale to use in ELRA,
3. a radial viscosity profile to use in a 1D GIA model.

In a context where these GIA models are coupled to an ice-sheet model, these recommendations aim to reduce the discrepancies in sea level contribution from the Antarctic ice sheet compared to the use of a 3D GIA model (with 2 different plausible Earth structures, which is a well-appreciated detail of the work). This is specifically done within the frame of possible climate forcings until 2500, as computed by 2 GCMs (IPSL and CESM). The potential impact of the paper is therefore also very clear: offer a low-hanging fruit to ice-sheet modellers that want to improve their representation of GIA simply by adjusting the parameters that are used in their already-implemented GIA model. This effectively represents 1-10 seconds of work and is therefore very appealing. Additionally, the manuscript reads well and it is unambiguous that it required a lot of work, including many expensive simulations. After this praise, I unsurprisingly think that this is a very valuable endeavour and am generally in favour of seeing this article published. Nonetheless, I think that there are a series of important points that need to be addressed before this. I will now proceed to listing them with the following scheme:

- Primary comments: these are key points that need to be addressed in a very comprehensive and thorough manner before a possible publication.

- Secondary comments: these are points that are either curiosities of mine and/or easy improvements that can be made to the manuscript.

- Technical comments: these are related to phrasing, format, etc.

- Additional references that come up along the way are gathered at the end of the document.

I want to emphasise that the length of the upcoming points is motivated by the fact that I am eager to improve the quality of this article as much as I can, since it will likely be highly relied upon in future within the field of ice-sheet modelling.

I will assume that the authors are familiar with common abbreviations from ice-sheet and GIA modelling.

Disclaimer: I only read the editor's initial decision statement after writing down my comments.

Primary comments

1. The authors spend very little time explaining the details of the ice-sheet model that are of true importance for this work. For instance, mentioning the basal friction law (l. 110) is here of secondary importance compared to questions like:

    a. Is the potential of a marine ice-cliff instability included in the runs?

    b. Does IMAU-ICE accept a heterogeneous, time-varying sea level? For that matter: is the coupling to the 3D GIA model merely deformational or does it also include the effects of rotation and gravitation?

    c. What type of melt parametrization is applied at the grounding line (Leguy et al. 2014)? Is any flux imposed? Using a resolution of 16 km is in my opinion sufficient to go ahead with the present publication but it needs to be accompanied by such information on the subgrid parameterization.

2. In the present work, SSP8.5 gives an SL contribution of 3.5-4.5 metres by 2300 (depending on the GCM and the 3D Earth structure). This makes IMAU-ICE more sensitive than any other model included in Seroussi et al. (2024, c.f. Fig. 4, especially panels a and b), which were forced, among others, by the 2300 projections of CESM which offers a straightforward comparison to the present work. In Seroussi et al. (2024), the ensemble of SL contributions under SSP8.5 spans -/+ 0.2 m of SL rise by 2100, which is a substantial difference to the value of >0.5m obtained in the present work. Adding to this, Coulon et al. (2024) and Klose et al. (2024) present SL contributions of the AIS that are nominally less than 3 m by 2300, although you are using the same climate forcing.

    This is somewhat surprising, because IMAU-ICE is the only one of these models that is coupled to a 3D GIA model, which has stabilising effects on the grounding line and should therefore lead to a lesser sensitivity. I think this needs to be mentioned and, in the best case, explained comprehensively. Answering the points I raise in 1.a.-1.c might be a step in this direction.

3. I believe that the aforementioned high sensitivity of IMAU-ICE might slightly undermine the message of the article: if the positive feedbacks that are internal to (marine) ice sheets are particularly strong in IMAU-ICE, the negative feedback of GIA might be underestimated in comparison. I believe that the simulations you present show a few symptoms of this:

    a. Looking at Fig. 4, the error with respect to the 3D runs is almost the same for ELRA3000 and ELRA200 - although ELRA3000 is far from representing the reality in WAIS (Barletta et al. 2018, among many others). In essence, this means that the difference to the average 3D model is relatively insensitive to the choice of τ. Just so that you get me right: your optimal choice of τ would be the same with an ice-sheet model of lesser sensitivity and, in that sense, it does not change your final recommendation… but the impact of this optimal choice would be higher. I think you should consider this, since it might imply a larger reduction of the relative error in SL contribution thanks to your recommendations and could make your message stronger.

b. The large uplift discrepancy of 250 m that you mention at l. 369 has almost no impact on the result. Explaining the little impact of this difference on SL contribution by invoking the fact that it arises relatively late in the simulation is a partially albeit not fully satisfactory explanation to me. I believe the (potentially too) high nonlinearities that are internal to the ice sheet model contribute to it.

4. This large discrepancy of 250 metres makes me wonder how the change of the ocean mask and the associated change in load is treated in your implementation of ELRA. When ice melts in zones grounded below sea level, it is replaced by the ocean which largely compensates for the reduction in surface load due to ice melt. Is this accounted for here? If not, this might be an explanation for such high error values of ELRA – which arise even for a 2D relaxation time field! This point illustrates a deficiency of the article, which does not mention at all what is the coupling scheme between IMAU-ICE and the various GIA models. This can however be easily fixed with a couple of additional lines in the manuscript.

5. I know very little about IPSL but CESM is known as a quite high-sensitivity model. I think this is a rather good thing since it leads to ice-sheet configurations that show large differences to present-day, thus exploring more diverse AIS configurations. I think this could be highlighted in a discussion section, especially since it supports the overall message of the paper. I would very much appreciate more information about the GCM outputs in the supplemental material: e.g. their present-day bias compared to RACMO, MAR or ERA5, and their warming pattern (atmospheric and oceanic) by 2300.

6. In Fig. 4, IPSL gives a barystatic sea level contribution of 3 to 4 metres by 2500 under SSP2.6, which roughly corresponds to the total sea-level equivalent volume of the WAIS (i.e. only taking into account ice above floatation + corrections). In the corresponding maps shown in Fig. 5, the WAIS is however far from a total collapse, since the Ross and Filchner-Ronne grounding lines are essentially unaffected compared to present-day. I suspect two possible explanations for this discrepancy:

   a. The ice sheet has experienced a thinning throughout its domain, including EAIS (otherwise such values are impossible).

   b. The authors are showing the ice volume that was lost (including ice grounded below sea level) and NOT the actual contribution to barystatic sea-level. If this is the case, this might of course partly resolve the doubts I raise in 2. and 3.

7. Questions around the spatial resolution of models can easily become generic. I hope this does not apply to the one I will try to raise here. The “high-resolution” configuration of your 3D GIA model has a lateral resolution of 200 km, which is refined to 35 km in the WAIS. I believe this is more than sufficient if one wants to

study mechanisms in a qualitative way, e.g. “how does rheology affect the grounding line retreat?”. However, the goal of the present study is quite different, namely to make a recommendation of parameter choice to minimise the error of simpler GIA models compared to the averaged 3D results. This is a much more quantitative assessment, which essentially places your 3D GIA model as a “golden standard”. The recent work of Gomez et al. (2024), which uses a resolution of 3 km for the GIA model and 5 km for the WAIS (somewhat coarser outside), places a quite high target to what can be called a “golden standard” nowadays. I am not arguing that such a resolution is needed for the present study. However, if you want to keep the resolution as it is you need to show a convergence of some meaningful metric over (lower) resolutions. Alternatively, I’d be happy with a 16km-100km combination for the lateral resolution of your 3D GIA model or with any argument that shows that your resolution is high enough. I know this type of comment is the authors’ nightmare because it implies repeating runs that can become substantially longer. However, I hope that the authors understand that this expectation comes from the potential high impact of the paper.

8. Along the same idea, l. 149-150: “the GIA model is used with 9 vertical layers (0-35 km, 35-100 km, 100-150 km, 150-300 km, 300-420 km, 420-550 km, 550-670 km, 670-1171 km, and 1171-2890 km, and 2890-6371 km).” It seems like you used the same set-up in van Calcar et al. (2023) – which is fair in that case since you are not making direct parameter recommendations. Unless I am getting something wrong, this sounds very coarse to me, for instance compared with what was used already some time ago in Gomez et al. (2018), where the vertical layers have a thickness of 6–50 km (only down to the CMB). Another example with a different model can be found in Albrecht et al. (2024), where “Δz = 5 km down to 420 km depth, followed below by Δz = 10 km down to 670 km and Δz = 40 to 60 km down to the core–mantle boundary": The use of a much coarser resolution here presents two problems:

    a. A potentially inaccurate representation of the mechanics. For instance, even for an idealised constant viscosity over depth, you might obtain significant errors compared to the actual solution. In particular, does a layer from 2890 down to 6371 km depth make any sense in that context?

    b. An inaccurate representation of the parameter field. If you have fast variations of viscosities over depths, as it is the case in ASE, averaging over such thick layers might lead to an additional error when resolving the mechanics.

    Just as for the lateral resolution, I wish to see at least some convergence/refinement arguments to fully approve a paper that recommends parameters to the community.

9. The explored range of ice-sheet configurations is essentially limited to a collapse of the WAIS (and parts of EAIS basins?) compared to present-day. If a different configuration of the ice sheet is being studied, the results shown here might be leading to large errors. For instance, performing the same task with simulations in the distant future would lead to a higher optimal uniform relaxation time (or 1D viscosity profile) since the bulk of contribution to sea level would come from EAIS, where the viscosities are even higher. This is illustrated in many of your plots, where it is clear

that the error has a pronounced drift beyond 2500, giving an advantage to slower responding models. This does not undermine your message at all but needs to be discussed.

10. Swierczek-Jereczek et al. (2024) present FastIsostasy, a regional GIA model that (1) is computationally as efficient as ELRA, (2) directly takes laterally-variable viscosity and lithospheric thickness as input, (3) displays different relaxation time depending on the wavelength of the load, (4) includes a regional approximation of gravitational feedbacks on sea level and (5) displays errors that are low compared to a 3D GIA model, which was tested with idealised as well as realistic ice histories. Citing this paper was omitted, in spite of its direct relevance for the aim of the present paper. It however seems difficult to circumvent because it changes the emphasis of the message that is conveyed in the present article:

    a. **Computationally efficient ways to represent lateral variability with low error compared to a 3D GIA model already exist** which contrasts with the statement at l. 45-47. Besides FastIsostasy, 3D regional models like those presented in Weederstijn et al. (2023) and Nield et al. (2018) are computationally tractable when considering simulations only until 2500. **The main advantage of the present work is to avoid coupling to a different GIA model, which can sometimes require quite a lot of programming effort.** I insist that this is the main contribution of the article, which is very useful but should be stated more precisely.

    b. If provided with a new seismic data set, the process of fitting the relaxation time scales (summarised in Fig. 2 and in Supplemental Tab. 1) might need to be repeated. You are in the best position to know how time consuming this is and the ice-sheet community will be grateful that you made this effort. However, it should be emphasised that repeating this effort can be avoided by using approaches like FastIsostasy.

    c. You mention that the impact of a laterally variable lithospheric thickness has only a marginal effect on the bedrock deformation (l. 212-213). I think this depends on the ice-sheet configurations that are explored, especially for future projections where the West-East gradient might become more relevant. Of course, including a laterally variable LT is more complex to implement… but it turns out it was already done in FastIsostasy without noticeable increase in the computational cost. Since the software is open source (in fortran and julia), **the only practical obstacle to including this is the coupling effort.** I think it should be discussed that the latter is not more tedious than exploring different combinations of 2D relaxation times and constant lithospheric thicknesses (e.g. your work summarised in Fig. 6).

    d. As you mention at l. 223-224, ELRA displays exactly the same relaxation time, regardless of the extent of the mass anomaly. Because of this, it would be particularly important to discuss the geometry of the schematic load that you apply to the 3D model to derive the relaxation time. What horizontal extent does the load have? Did you vary this to see how it impacted the final

result? From Tab. 1 in the Supplement I gather you did that (small, large and LGM), but this is not mentioned explicitly although it makes the paper stronger!
As a side note, FastIsostasy partially fixes this since it does not present a flat relaxation spectrum.

11. You recommend a radial viscosity profile for the 1D GIA based on the behaviour of the AIS compared to the outcome with a 3D GIA model. The caveat of this is that this lower viscosity is then applied globally whereas we know that most regions have higher viscosities. In other words, your error metric is local, although your model is global... Imagine we select a random point at the coast of the Netherlands: the sea level rises because of AIS melting and floods the grid cell which subsides because of the new load. This response happens too fast with your viscosity recommendation! Again, this is not critical but needs to be discussed at least a bit.

12. The points I raised so far make me think that a discussion section between the results and the conclusions is absolutely necessary!

13. I acknowledge the intention of the authors to provide all the data that resulted from the study in the supplemental material. This is mostly raw, ~100Gb data, which has a value if anyone wants to reproduce the analysis, but which is also anything but visual and hence of little use to the average reader. I encourage the authors to provide supplemental material that is of importance for the present study while being directly digestible, such as maps of the initial state of the ice sheet with an error plot with respect to to present-day, as well as maps of the ice thickness anomaly at 2500 compared to present-day for SSP2.6 and SSP8.5 (for at least one of the 3D runs). For instance, the doubt I raise in 5. could be answered directly by such a plot.

Secondary comments

1. The word “bedrock” is used throughout the manuscript with a very “GIA perspective” on things. Bedrock response to ice-sheet forcing includes, for instance, erosion and sedimentation - which is obviously not your current object of study. I suggest you change this to something less ambiguous throughout the manuscript like “bedrock (vertical) deformation”. In particular I think this would make the title more specific. Something like “Approximating the coupled bedrock displacement in Antarctic ice sheet projections”. Since the main result of your work is the parameter recommendation, you could even include this idea in the title.

2. L. 23-24: You mention a difference in SL rise that “deviates less than 40cm”. Without knowing what is the total contribution to SL rise, this number is not very meaningful. In particular, it might sound like a huge amount (which it is in terms of policy advising!) if not cited as relative value.

3. L. 29: Citing the IPCC is of course pretty reliable but it is not the most up-to-date information. I believe you should at least add Seroussi et al. (2024), Coulon et al.

(2024) and Klose et al. (2024) here.

4. L. 30 “atmospheric and oceanic feedbacks”. I would replace “feedbacks” by “processes” since it is more general. For instance, the warm water intrusion observed over the last decades in ASE is an oceanic process, not a feedback.

5. L. 43-45: the context of the numbers you are mentioning could be more precise. Over the next centuries = until 2500?

6. L. 46: I believe you can at least partially do that with the model presented in Swierczek-Jereczek et al. (2024).

7. L. 61: You are omitting Gomez et al. (2024) which contrasts with your statement.

8. L. 82 - 88: you repeatedly use “approximate the ice sheet evolution resulting from a coupled ice sheet - GIA model using a 3D Earth structure” in the three questions. I think it would read better if you define this before as your “baseline” and refer to it as such later.

   I believe the three questions you pose are erroneously formulated. This appears particularly in your conclusions: “Research question 1 was: How well can a uniform relaxation time approximate the ice sheet evolution resulting from 3D Earth structures? We recommend to use a uniform relaxation time of 300 years with a lithospheric thickness of 100 km to replicate the sea level rise predicted by a model that includes 3D Earth structure.” The answer and the question don’t match! You ask “how well?” and you answer “the best choice is”. Your question is actually: what is the best parameter choice for the near future? Of course, this comes with an error analysis (“how well?”), but the final outcome you want to convey to the community is the improved choice of relaxation time for ELRA and viscosity profile for 1D GIA models.

9. L. 82 and following: do you have any reference that describes the GRD effects as implemented in your 3D GIA model? I feel like the description of dislocation and diffusion creep parameterization is actually less important than this – although admittedly much more concise to write down.

10. L. 90: you introduce IMAU-ICE but previous work of yours mentions ANICE and the code provided in the supplemental material also uses this name. Is there any difference between both apart from the name?

11. L. 95: “and one based on a constraint in the Weddell Sea Embayment and Palmer land in the Antarctic Peninsula.” I don’t really understand why you use the Weddell Sea Embayment and Palmer land as a constraint, since they are not particularly relevant for future sea level rise. Because we have better GNSS constraints in this region?

12. L. 145-147: do you have a reference where it is shown that both things are equivalent? As an analogy from ice-sheet modelling to support my question: tuning

the friction law or tuning the friction coefficient field can have similar impacts but are actually quite different conceptually and may lead to different sensitivities.

13. L. 182: I understand this means that the rotational feedback and the shoreline migration are not taken into account? This should be stated explicitly.

14. L. 186: ELRA does not “depend” on a point load. The displacement is just obtained by a convolution of the response to a point load with the actual load.

15. L. 203: I suspect using a uniform 60 or 100 km lithospheric thickness for any experiment that implies a large-scale retreat in East Antarctica would give a pretty erroneous displacement pattern, since the lithosphere is much thicker in this region. This should be mentioned at some point in the discussion to prevent ice-sheet modellers from using it in such cases.

16. L. 301 and following: I feel like recommending a parameter value until 2400 and a different one if running until 2500 is a very odd thing to do – especially because it depends on your ice sheet model + climate forcing and is therefore not particularly robust. I think recommending a single nominal value for 2500 accompanied by an uncertainty range is a much more useful information for ice-sheet modellers, who can then choose to sample the uncertainties or not. This would be nicely illustrated if you provide some integrated error metric (as box or scatter plot) on the right y-axis of Figure 4 b, c, e, f, Figure 6 and Figure 7.

Technical comments

1. Some of your section titles have points after the number e.g. “5. Conclusions and outlook” and some not e.g. “4 Projections using different approaches to bedrock response”. Also their indentation is not consistent.

2. The table and figure captions are all in bold fonts, which I believe is a formatting error.

3. I think that throughout the Copernicus journals, abbreviated names for figures, tables and equations should be used when placed in the middle of a sentence but they should be spelled out if at the beginning or at the end of a sentence. If I am not mistaken in this regard, you should correct l. 188, l. 234, l. 240, l. 297 (maybe I am missing some).

4. L. 19: I reckon that when spelling out ELRA a comma, a dash or a slash should separate “elastic lithosphere” from “relaxed asthenosphere”. Apply also later in the manuscript.

5. I believe “ELRA” can be written without specifying “the ELRA model”, since models are generally referred to by their sole name once introduced (e.g. “CESM shows high

equilibrium climate sensitivity” and not “the CESM model shows…”).

6. L. 26: you use “AIS” without introducing the acronym… and then you inconsistently use “Antarctic ice sheet” and “AIS” throughout the manuscript.

7. L. 32: giving a numeric value for the uncertainties arising from comparison projects could support your argument very nicely here (e.g. Seroussi et al., 2024, Fig. 15).

8. L. 42: “specific region where the loss occurs” is a bit bit vague. I think it would be clearer if replaced by “the solid Earth properties of the region where the ice loss occurs”

9. L. 52: I would add some reference to the statement – at the very least LeMeur and Huybrechts (1996).

10. L. 56: I would delete “(the elastic modulus)”, since “stiffness” is clear enough.

11. L. 69: “However, using…” should be reformulated to make clear that it refers to the work presented in Pollard et al. (2017).

12. L. 103-105: this is a bit tedious to read. I suggest the following rephrasing: “We use the projections of two climate models, CESM and IPSL, under a low emission scenario (SSP1.2-6) and a high emission scenario (SSP5.8.5). In CESM, the warming mainly occurs in the Weddell sea, whereas in IPSL, the warming mainly occurs in the Amundsen Sea.”

13. L. 108: I would replace “ice dynamical” by “thermomechanically coupled ice model”, which is more accurate.

14. L. 110: I guess that you use the nonlocal formulation of the melt law proposed in Favier et al. (2019)? I think this should be written down explicitly to not confuse it with the local formulation.

15. Equation 1 and 2 (and rest of the manuscript): it is unclear to me which logic is adopted by the authors when it comes to italic vs. normal font for mathematical symbols and indices. For instance incoherent notation of D and L_r between l. 187-188 and Equation 3 and 4. Also Equation 6 and following text…

16. Equation 2: I think T(x, y) makes much more sense than the subscript notation you use, since the temperature is a function of the space coordinates. Mentioning what T(x,y) is is redundant between l. 133 and l. 136 (but as you wish… sometimes nice to repeat).

17. L. 131-134: you give numerical values for some parameters and for others not. Why not for all?

18. L. 169: this very short sentence could be fused with the former one.

19. L. 174: you introduce the two configurations. One is introduced with quotation marks and the other without. This should be coherent. Moreover, writing out “low resolution configuration” and “high-resolution configuration” is almost just as long as “configuration 1 / 2” and it has the advantage that the reader does not need to remember which is which.

20. Figure 1: pa → Pa in the x label

21. Equation 5: The subscript notation “$w_{i,j}$”, “$b_{i,j}$” makes more sense than using parentheses here since it refers to matrix indices, as stated in the final part of the sentence.

22. L. 231-232: I don’t get what the sentence “For each simulation, the displacement over time for each surface load/Earth model combination is computed for multiple time steps” should convey. That you don’t have a single time step as output? Isn’t that a bit obvious?

23. Figure 2 is great. You probably can leave out the distracting axis ticks and labels of the Antarctic map, since it is clear what is represented but I leave that to your choice.

24. L. 237: I believe “log of the viscosity” should be replaced by “logarithm of the viscosity” or “logarithmic viscosity”.

25. L. 268-270: this sentence reads pretty bad. Why invoke the ice sheet model here if the only thing you say is that it evolves the ice thickness in time?

26. L. 280-284: the two sentences begin with exactly the same structure, which reads a bit odd.

27. L. 284: a small technicality but… isn’t the barystatic contribution to sea level actually calculated within your GIA model based on the ice thickness?

28. L. 295: 0.44-07 m does not correspond to the percentage of 8-20%. Also, I would suggest giving the error separately for each scenario since it changes the value quite significantly.

29. I would suggest giving references, at some point, about IPSL and CESM and potentially about any publication describing the simulations you are using. I reckon the refs can be found in the Coulon and Klose (et al., 2024).

30. L. 301-304: you begin with the conclusion on your results. I would first begin with the detailed analysis you make at l. 307-330 and then conclude (what you actually do partly already and hence 301-304 is redundant).

31. L. 304: I could not find Supplemental Fig. 1.

32. Figure 4: “km·m2 /s2” is a weird unit to use when you can simply use “N m” as done before in the manuscript.

33. L. 366: the IPSL projections are not ASE projections. They are just projections, with more warming in ASE. I think this is not formulated clearly here.

34. L. 307, 368: I would just write “IPSL-SSP2.6” and “IPSL-SSP8.5” instead of lengthy formulations like “In the high emission scenario driven by the IPSL climate model” (which is wrong anyway because it is the high emission scenario that is used as input for IPSL).

35. L. 408: this sentence reads pretty bad. How about “Using forcing fields from 2 different GCMs under low and high emission scenarios, we investigated the accuracy of common implementations of bedrock displacement when coupled to an ice sheet model.” Or something similar…

36. L. 432: 2D stronger 120 looks more like 20 cm error by 2500 as far as I can tell from Fig. 6, and not 10 cm as you state.

37. L. 454 and following: I wonder if “doi” should be replaced with “DOI” since it’s an acronym.

I hope these comments will make the paper stronger and I wish the best of luck to the authors for the rest of the publication process.

Sincerely,

Jan Swierczek-Jereczek

Additional references

1. Leguy, G. R., Asay-Davis, X. S., and Lipscomb, W. H.: Parameterization of basal friction near grounding lines in a one-dimensional ice sheet model, The Cryosphere, 8, 1239–1259, https://doi.org/10.5194/tc-8-1239-2014, 2014.

2. Seroussi, H., Pelle, T., Lipscomb et al.: Evolution of theAntarctic Ice Sheet over the next three centuries from an ISMIP6 model ensemble, Earth's Future, 12, e2024EF004561, https://doi.org/10.1029/2024EF004561, 2024.

3. Klose, A. , Coulon, V., Pattyn, F., and Winkelmann, R.: The long-term sea-level commitment from Antarctica, The Cryosphere, 18, 4463–4492, https://doi.org/10.5194/tc-18-4463-2024, 2024.