Interactive comment on "Future Antarctic bed topography and its implications for ice sheet dynamics" *by* S. Adhikari et al.

Anonymous Referee #2

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General comments

The manuscript *Future Antarctic bed topography and its implications for ice sheet dynamics* by Adhikari et al. aims at investigating the influence of viscoelastic solid Earth response to ice loading on future ice dynamics in Antarctica. First, past and future loading are related by means of loading a two-layer solid Earth model with (a) a reconstructed ice load history from Last Glacial Maximum to Present Day (PD) and (b) ice dynamical projections from the SeaRISE experiment for the upcoming 500 years with the outcome that future loading will have a greater impact on the Antarctic bedrock topography. Second, the impact of these changes in bedrock topography on ice dynamics is evaluated in terms of gravitational driving stress, grounding line

(GL) migration, and surface velocity reduction. This analysis leads the authors to the conclusion that future bedrock uplift in Antarctica will stabilize the ice sheet.

The feedback mechanisms between ice bodies and the solid Earth are very a important issue for present and future research, and the investigation of these is clearly within the scope of Solid Earth in general and the Special Issue on 'Lithosphere-cryosphere interactions' in particular.

In my view, the first part of the manuscript, dealing with the impact of future ice loading in Antarctica, is interesting, relevant, and mostly well documented. The significance of a comparison between past and future loading impact on the topography is given without any doubt.
The analysis in the second part, however, that aims at investigating the influence of the bedrock uplift on future ice dynamics, and the conclusions drawn from this analysis might be strongly affected by the simplifications on which the analysis is based.
If it was only for the first part, I would recommend only minor revisions before publication in Solid Earth. But with my concerns about the second part, I am wavering between minor and major revisions.

The issue that concerns me most is the usage of the PD ice thickness distribution for the analysis of future gravitational driving stress, GL migration, and ice velocity field. If GL retreat occurs as a response to the climate forcing in the projections used as future loading scenarios, the bedrock uplift will also be associated to this GL retreat. If the GL advance, that is discussed in the manuscript, would be the result of the uplift following GL retreat, the results would be in clear contradiction to the input.
If I am not mistaken, there are no publicly available figures showing the individual model results of the R8 scenario in Bindschadler et al. (2013), which the authors are using, except for Fig S3 in the manuscript's supplement. Just from this Fig. S3, without

initial and final GL drawn and the colorscale probably not covering maximum/minimum values, it is hard to judge where GL retreat could have occured during the simulations. There might be GL retreat in the Amery Ice Shelf, for example, (all models show high losses here that might be accompanied by GL retreat). SICOPOLIS and AIF show mass loss at the Ross Ice Shelf GL, where also GL retreat might occur. The Amundsen Sea Sector might or might not be floating in PennState, PISM-PIK (possibly large area for these two), and SICOPOLIS.

So my very first suggestion in the context of the analysis of future ice dynamics is a more comprehensible description of the SeaRISE R8 model output, at least in Fig S3, so that it can be judged if there is a contradiction between GIA model input and the results for the impact of bedrock uplift on future ice dynamics in the present manuscript.

But even if all topographic corrections from viscolastic deformation were only based on inland thinning (as shown in Fig 1a and b), and no GL retreat occured in the projections, the thinning of the ice itsself should be taken into account when computing $\Delta\tau_d$ (changes in driving stress), GL migration according to the floating criterion, or – as a result of the two quantities – changes in surface velocity: $\Delta\tau_d$ relates to surface slopes and will therefore be sensitive to the consideration of spatio-temporally thinning inland ice. GL could be affected by future thinning of the ice in the ice shelfs.

I suggest that the authors undertake a different or an additional analysis, respectively, by assuming not PD ice thickness but ice thickness that is in some sense more consistent with the projections for a given time, for example by assuming an average ice thickness distribution from the four models, as it has also been done with the uplift patterns. Then an extraction of glacial isostatic adjustment (GIA) related effects could be performed by comparing this ice thickness distribution on PD and future bedrock.

With the previous two paragraph, I argue that, by the current analysis of PD ice thickness on the future bedrock, no conclusions can be drawn with respect to GIA stabilizing

the Antarctic Ice Sheet (AIS) in the future. Many destabilizing effects have acted on the way to computing the future bedrock uplift. These are then neglected for the analysis, although it could turn out that all effects related to GIA would be insignificant compared to the climatic destabilization. In this respect, the manuscript does not provide quantification of the stabilizing GIA effects as the authors state at several occasions in the text.

It is not fully clear to me, how the authors transfer the future ice thickness to a loading of the solid Earth in terms of differential ice height (DIH) and then to crustal uplift/subsidence. Please revise the description of the input and output of the GIA model and refer to the following questions:

- Do the authors only compute solid Earth response where the ice is grounded? For example, the phrase on P197 L18-20 is confusing: 'The GIA solutions perturb ice/bedrock contact surface within the area of ice sheet grounding.' Does that imply that GIA is not considered beyond the grounded ice sheet because there is no ice/bedrock interface?
- From the SICOPOLIS results in Ross and Ronne-Filchner Ice Shelf, for example, one can conclude that Fig. S3 shows DIHs for both grounded and floating ice. Do the authors assume that changes in floating ice have an impact on solid Earth deformation?
- Do the authors account for the ocean water that partially replaces the ice load if the GL retreats, or that is replaced by grounded ice if the GL advances, respectively, when forcing the GIA model?
- Does 'assuming the equilibrium sea level' (P195 L27/28) mean that sea level does not rise (or fall) in the experiments, be it due to ice melting inside or outside of Antarctica or any other process?

The model descriptions in Bindschadler et al. (2013) contain the information about

treatment of the solid Earth for some models at least. It has been discussed in Section 1.3 by the authors that the approximations in solid Earth physics in ice sheet models are mostly insufficient. Still, the feedbacks discussed in 1.1 and 1.2 are possibly treated by the individual authors of the SeaRISE study by such an approximation. Please comment on how stabilization by GIA might already be accounted for by the ice sheet models.

The authors opt for a specific 2-layer GIA model with 65 km lithosphere thickness and optimize their results in terms of uplift rates at sites where GPS stations are available. They comment on the applicability of this parameter choice beneath West Antarctica. However, any discussion about the solid Earth response probably being overestimated in East Antarctica by this choice is missing in the later parts. There should be respective notes in the later discussion and possibly also a quantification.

The authors use the expression 'first order estimates of Antarctic bedrock uplift' (P196, P202) several times, and finally explain that 'first order' means averaging the four individual future uplift patterns on P203. I suggest to either stress the meaning at the first mention, or to drop 'first order'.

Specific comments

Section 2.1: The label 'ISSM/GIA' somehow implies that the ice sheet model is capable of GIA modeling. If I understand it correctly, the current mutuality of ISSM and ISSM/GIA is the mesh on which they operate. If this is correct, I suggest to put more emphasis on this circumstance in the Introduction and when introducing ISSM/GIA in Section 2.1. Additionally, I suggest, that the authors include a figure in the supplement (for example, in the context of Fig S1a and b) that shows how the discs are situated in

the ISSM mesh and how by that the ISSM resolution directly affects the GIA resolution and accuracy.

Section 2.3: From Bindschadler et al. (2013), I understand, that the described scenario is labeled as 'R8'. Please refer to that label during the description.

P200 L13 / Fig 2: How have the GPS stations and their data been chosen from the set of Thomas et al. (2011)? The authors of the respective study offer viscoelastic uplift rates at many more sites. How are the misfits in case of higher/lower values for mantle viscosity? Please consider to include respective data in Fig 2b.

P200 L19: Please name the values for upper and mantle viscosity that have been found optimal by Ivins et al (2013) in case of the 65 km lithosphere.

P203 L13-P204 L03: It would be interesting to relate the numbers for uplift/subsidence with some average value of DIH in the respective regions.

P205 L18: '... by perturbing the steady-state response of solid Earth to present-day AIS loading through imposition of the future ice load changes.' I think, this is unnecessarily complicated. Please consider re-formulation with a focus on the difference to the previous experiments that included past loading or were restricted to past loading. For example, the description of the experiment with only past loading is much more straightforward: '... thus imposing $\Delta h(x, y, t) = 0$ for all $t \in [0,500]$' (P205 L08).

P207 L10-12: I do not think that changing slopes beneath floating ice at some distance from the GL will have an impact on GL migration.

P207 L22-23: What are these relevant boundary conditions?

P208 L09-10: It would become very clear how bedrock slopes alter the driving stress, if α_{si} was expressed as the sum of ice thickness gradient and bedrock slope.

P208 L13-15: 'Although the maximum changes in driving stress are about three orders of magnitude smaller than the driving stress itself, large changes are predicted at positions of larger bed uplift.' I do not see how the first part of the sentence relates to the second one. Please re-formulate.

P210 L02: '2800 flowlines': Would it be possible to include at least some of them in a figure?

P210 L12-25: 'Extensive observation of the GL advance ...'
'Observation' makes the reader think of a sudden switch to actual remote sensing data or something similar. Also, I find the expression 'reverse topography' confusing. Please consider re-formulation. I can see no evidence of GL migration in the Shackleton Ice Shelf in Fig. 7a and only minimal evidence in the Getz Ice Shelf. In general, a zoom to the respective parts in Fig 5a and c would help the reader to follow the discussion.

Fig. 4: Just a suggestion: Wouldn't it be more intuitive for the message (small effect of past load; large effect of future load) to exchange Fig S7 and Fig 4 in terms of the colorscaling. Then the small response to past loads would be obvious from the very first glimpse.

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Time conventions: I suggest to make the convention for temporal reference more uniform in the main text and in the Figures (omission of AD in several figures; varying reference to $t = 0$ and to years AD).

Technical corrections
P192 L24: I think it is worth writing 'Antarctic Ice Sheet' once in the main text; same for grounding line on P193 L05.
P194 L17: initially equilibrium GL → maybe 'initially stable GL'?
P194 L26: 'to ice sheet': ice sheets
P200 L03: 'Several **values for** lithosphere thickness'
P205 L17: Cancel 'model'? Or exchange 'model' and 'average'?
P208 L11: uplift
P208 L23: 'but in out of phase': Cancel 'in', or rather re-formulate without using 'phase' as the issue is only about positive and negative signs.
P210 L06: in **the** other two ice shelves
P214 L01: pervasive
P214 L06: greater → more