Review comments on the submitted manuscript "Feedback mechanisms controlling Antarctic glacial cycle dynamics simulated with a coupled ice sheet–solid Earth model" by Albrecht et al.

Summary:

Growth or retreat of grounded ice sheets barystatically contributes to global sea level by exchanging mass between cryosphere and hydrosphere. In addition, it changes local relative sea level by causing deformation of the solid Earth and perturbing the Earth's gravitational field and rotation vectors, imprinting spatially varying sea-level changes.

In a little over the past decade, numerous modeling studies have shown the presence and significant impact of the feedback mechanisms between ice sheets, sea level and the solid Earth on ice-sheet modeling, highlighting the importance of coupled ice sheet – solid Earth/sea level modeling, particularly for the marine-based West Antarctic Ice Sheet (WAIS), which is situated upon reverse-sloped bedrock and under threat by MICI, and thus its stability is closely linked to the ocean depth at the grounding line and configuration of bed topography underneath. Moreover, the structure of the Earth across Antarctica is laterally heterogenous; it is characterized by thinner lithosphere and lower mantle viscosity in West Antarctica (WA), which causes faster viscous response of the solid Earth to ice loading/unloading, requiring a rather short coupling interval between the ice sheet model and solid Earth/sea-level model. Thus, capturing the interactions between ice sheets, solid Earth and sea level, as well as considering the 3D structure of the Earth and having a "short" coupling interval in the coupled modeling are warranted for accurate modeling of the Antarctic Ice Sheet.

In their paper, Albrecht and colleagues develop a newly coupled ice sheet-solid Earth/sea level model using the PISM and VILMA, which are a published ice-sheet model and viscoelastic solid Earth model, respectively. They explore Antarctic Ice Sheet dynamics over the last two glacial cycles in simulation experiments with varying Earth structure profiles, number of topographic corrections for the initial topography, coupling interval, and spatial resolution of the solid Earth model. They find a combination of these parameters according to the convergence of the AIS volume configuration and explore sensitivity of AIS to different 3D Earth Structure profiles. In addition to observing the negative sea-level feedback mechanism on grounding line retreat that has been seen by other modeling groups, the authors also observe what they call the "forebulge feedback", which helps the grounding line to advance by reducing the local ocean depth during the glacial buildup phase. The authors also explore and confirm the effect of far-field sea-level forcing from melting of the Northern Hemispheric Ice Sheet during the last deglaciation on the AIS dynamics, which has been previously verified by other studies.

The model development, simulation experiments and analysis done in this manuscript represent lots of hard work by the authors, and undoubtingly an important contribution to the ice sheet and GIA community. It is great to see independent groups around the world building the capability of modeling coupled ice sheet - solid Earth/sea level dynamics; together which will push for better representation of ice-sheet physics and better prediction of future sea level. While I support this topic material is suitable for publication in the journal *The Cryosphere*, I anticipate major revision needs to be done regarding several aspects including more thorough literature review and correct citation, more exhaustive discussion on outcomes and justifications on experimental choices, which might incur additional simulations.

General comments:

1. It would be helpful to clarify the definition of GIA, whether it solely addresses to the solid Earth deformation component or includes gravitational and rotational effects and use the precise/consistent term for the feedback mechanism. It becomes clear through the text that the authors take the former as the case, but then the term "ice sheet – GIA feedback" becomes imprecise as it misses the changes in the sea surface height.

2. It is great that the authors perform the topographic iteration to have the modeled present-day topography to match with the observed data. However, authors attribute all the differences in results to difference in the Earth Structure and omit the discussion on the role of having different initial bed topography, which could be quite important for ice dynamics. This issue could be addressed by discussing in detail the differences in 3D Earth results given the same initial topography (i.e. 1st iteration) and then putting the 3rd-iteration results into context.

3. The authors observe "forebulge effect" in their simulations, which helps the grounding line to advance during the build up phase. This however seems to warrant more rigorous testing and analyses. (If forebulge effect is real, then I would anticipate the `3D ref` curve should stay below the `3D min` curve in Figure 7a. And maybe that's what you are seeing in the 1st-iteration results, but not in the 3rd-iteration results, which might suggest the effects related to different initial topography?) You could look at how the local ocean depth changes at the grounding line and few cells from it (or along a transect) through time to check the location and size the forebulge formation. You could also run a simulation on a non-deformable (rigid) Earth for clearer comparison against the deformable Earth cases. Lastly, several other groups (e.g. DeBoer et al. 2014) have seen the destabilizing effect due increase of local water depth at grounding line when ice sheet advances, and putting your results in the context of literature would improve the discussion on this matter.

4. The authors claim that they have performed the convergence test on spatial resolution, along with coupling time step and topographic iteration. However, the spatial resolution has been explored only in the GIA model side, not on the PISM side. Clarifying the wording in the text is advised.

5. There is lack of proper literature review and giving credits to the studies that deserve citation.

6. Despite the high-fidelity of the model and the exhaustive model experiments (e.g. being able to run over glacial cycles and incorporating the 3D Earth Structure), there seems to be a lack of discussion on other phases of the glacial cycles and differences between West and East Antarctica, giving the impression that the discussion is under-utilizing the existing model results.

7. Given that the community is starting to call for clarification and agreement on the method of calculating "ice-sheet contribution to sea level", I recommend authors to provide more detail on how they calculate the sea-level equivalent ice volume: do you correct for all bedrock deformation, density difference between fresh and ocean water, external ocean forcing, or only some of them?

Specific comments:

L3: "In this study, we run coupled ice sheet–solid Earth simulations…"
"develop" than "run" might be more appropriate.

L36-39: "The redistribution of land ice, ocean water and mantle material induces changes in the Earth's gravity field (with the 'geoid' as equipotential surface), and therefore alters the sea level globally, which follows the geoid (Farrell and Clark, 1976); a correction, which usually is considered as being part of GIA."

This sentence is little awkward. Rephrase? Also, it's missing the direct he Earth's gravity field also gets perturbed by the direct interaction between landice and ocean water (i.e. weakening/strengthening of gravitational attraction) and that the pattern or perturbation is spatially non-uniform.

L39: "Geodetic investigations to… "
What about geophysical evidence? (i.e., seismic studies by Lloyd et al., for example.)

L41: "…a thinner lithosphere, "
The thinner lithosphere in West Antarctica needs to be introduced before referring here.

L34-47:
This paragraph can be further clarified. For now, GIA, geoid change, lateral variations in Antarctica and within West Antarctica are all mentioned here but without clear explanation of the connection between them.

L50: *"Goelzer et al., 2020; Adhikari et al., 2020) "*
These refs don't seem to be the right ones to cite here because 1) these two refs rather talk about a method to calculate ice-sheet contribution to sea level, and 2) the floatation criterion established in a very general sense.

L50: "GIA-induced viscoelastic bed deformation…"
GIA is already introduced as viscoelastic deformation (L36), so the expression "GIA-induced deformation" is double counting the effect. Either use GIA or solid Earth deformation. Also, in this sentence, gravitational effects need to be included to explain the water depth change. As mentioned in the general comment, please clarify the definition of GIA (gravitational and rotational effects included or excluded) and be consistent in using it throughout the text.

L51: "indirectly"
Why is this an indirect effect rather than direct?

L53: "(Gomez et al. 2010, 2012, 2015)"
There are studies that have specifically looked at the effect of gravitational attraction e.g., Coulon et al. 2021; Han et al. 2021 "

L56: "(lapse effect) "
lapse-rate effect

L61: "(Peltier, 2005)"
Is this a right reference for LGM timing? I think it's okay to not have one here unless there's more generic one.

L63: "Such inter-hemispheric "
The inter-hemispheric effect should be first clearly explained before saying "such" effect; the concept is not clear from the sentence before. You can make it clear that the "rapid sea level rise" since the LGM comes from the collapse of the ice sheet from the Northern Hemisphere.

L69: "(Coulson et al., 2021) "
This reference focuses on crustal deformation in response to prescribed ice sheets in the Northern and Southern Hemisphere, so it is doubtful to be an appropriate one to cite in talking about the state of Antarctic Ice Sheet.

L71-83: "Ice sheet models have been coupled to GIA models with different levels of complexity …"
This paragraph should provide more literature review on the coupled ice sheet - GIA models with the highest complexity in the field. For now, it mainly focuses on the models with the lowest level of complexity with ELRA and LC, which do not incorporate gravitational and rotational effects. It would be only fair to add more review on those that already incorporate both gravitational and rotational effects as well as the 3D earth structure.

L71: "(de Boer et al., 2017; Whitehouse, 2018). "
I would recommend citing the studies that have done the actual work of coupling an ice sheet model and a GIA model rather than citing review papers that describe the state of the field. Most of the references that can be cited here are already mentioned elsewhere (i.e. Gomez et al. 2012;2015, deBoer et al. 2017; Konrad et al. 2015, etc.)

L82: "(Goelzer et al., 2020) "
This ref introduces a method on how to correct for sea-level external forcing in calculating ice-sheet contribution to sea level, which happens at the post-processing part, rather than using GMSL as external forcing to drive ISMs?

L83: "with polar motion "
Rotational effects need to be introduced earlier in the introduction section.

L91: "presumably "
doesn't seem necessary.

L94: "Recently, a new generation of GIA models accounts for the 3D Earth structure (A et al., 2012; van der Wal et al., 2015; Nield et al., 2018; Powell et al., 2021; Blank et al., 2021) "
The sentence seems incomplete. Also, Latychev et al. (2005) developed a 3D GIA model, and Powell et al. 2021 used it. And how about the original references for VILMA?

L100-102: "Here, we present a set of new simulations of Antarctic Ice Sheet evolution over the last 246,000 years (i.e. two full glacial cycles) with the Parallel Ice Sheet Model (PISM) coupled to the VIscoelastic Lithosphere and MAntle model (VILMA) solving for GIA. "
It would be helpful to describe here in more detail how this study fits into the context of the work introduced above and in the field. For example, you mention above the interactive coupling scheme for 1D and 3D ISM-GIA models in the field, what do you similarly or differently? Is your goal to have a coupled model functionality that can iteratively converge towards an initial bed topography? Or come up with a coupling timestep that is the most appropriate to use over the glacial timescale/for a specific load response? What's the reason for doing your experiments for two glacial cycles?

L115: "interpolated accordingly "
According to what? Does this mean that basal melt is not calculated but rather taken from somehwere else predefined? Or do you mean the interpolation of basal melt at the ice draft?

L134: "sea level "
“sea-surface height” or “sea surface geoid” would be more precise term to use here.

L149: "rotational deformations "
Do you mean the effect of rotational feedbacks on viscoelastic deformation.

L150: "gravitational "
gravitationally?

L153: "Coast lines can freely migrate according to the local RSL, also accounting for floating ice shelves "
What does it mean that coastlines migrate accounting for floating ice?

L155: “redistribution of water mass”
“exchange of mass”?

L160:"condition, "

remove since core itself is a boundary rather than a condition?

L160: "we "
is?

L161: “n128”
describe what 'n' is.

L161: "256×512 "
If you are setting the spherical harmonics degree and order to be 170, shouldn't the grid resolution be 160X340?

L164: "gravity-consistent "
Have a consistent expression throughout the text - in Line 150, 'gravitational consistent' is used.

L165: "mass redistribution of water "
Remove “of water”.

L168: “…for flexible restarts, the original Fortran77 code was modernized to Fortran90…”
What do you mean by “flexible” restart? Did (or not) VILMA have a restart functionally before but it was restricted with some technical issue? And how was the code modernized? Please be more descriptive.

L169: "calculation of the left-hand side elastic problem and the right-hand side viscous solution "
Don’t see any left or right-hand side. Provide an equation or a reference to it?

L169: "For this, openMP4 functionality "
Isn't OPEN MP for parallelization? If so, how does this relate to the restart functionality?

L175:"PISM and VILMA are offline-coupled using a coupling time step that can be rather short. This is an advantage of the explicit time stepping and the weak formulation (in time-domain) of the solid-Earth dynamics (Martinec, 2000) in contrast to the normal-mode approach (in Laplace domain) used by most of the 1D Earth models"
The “coupling time step” in this sentence is the time interval after which PISM and VILMA call the other, and the explicit time stepping referred in this sentence is specifically for VILMA’s time stepping. It could be helpful for readers to make connection between the coupling timestep and VILMA’s timestep by explicitly mentioning that the coupling interval is constrained by the VILMA time stepping. In addition, the 1D normal mode approach also allows a short time stepping down to annual scale (e.g. Han et al. 2022). So the comparison made in this sentence might not be correct.

Figure 2: "coupling time step "
Include the 'delta t' symbol in the figure.

L179: "…VILMA integrates "
“runs”?

L181: "VILMA interpolates "
What does this mean? Why does ice history need to be interpolated temporally if it’s provided by PISM at every coupling interval?

L188: "and water load "
Remove or rephrase unless PISM actually knows about the global water loading distribution.

L190: "the PISM response always lacks behind the VILMA step by one coupling step "
Explain what this means? PISM runs between t0 and t1, and only after then VILMA will run from t0 and t1, and so on; I take you are trying to say that the RSL change from t0 and t1 calculated in VILMA is only reflected in ice dynamics in PISM for t1 and t2, not from t0 and t1.

L213: "(long) memory effects of the AIS to the climate history "
This is not the best reason for running two glacial cycles. Is there a better reason?

Figure 3 caption: "c) Ice volumes above flotation have been calculated considering cell-area weighting and density correction (Goelzer et al. 2020)… "
So you are correcting sea-level change with bedrock change correction term and density correction term from Goelzer et al. 2020? Any external sea-level forcing correction term?
Also, include descriptions for the legend in the figure caption.

Figure 3c:

Any idea what's the reason for a big difference in 3D min and max in panel c?

Figure 4.: Is this taking what is labeled as “3D ref” in Fig 3? State it in the caption. Also, please describe the black and grey contour lines.

L235: "previous PISM simulations”

Previous PISM studies?

L247: "The ‘3D min’ and the ‘3D max’… "
This introduction needs to come when Fig. 3 is first introduced. Also, by looking at the profile in the supplementary figure, these two profiles are laterally homogeneous, and more like 1D Earth profile? If so, would be helpful to state that.

L238 "…viscosity of 5×1020 Pa s). However, this approach is limited to a certain region (e.g. Antarctica) and is unable to solve self- consistently for sea level changes while considering a globally conserved water budget."

This information might be better combined with the part in Introduction where it talks about the limitations of solid earth models with simple structure and no gravitational and rotational effects. It sounds little redundant and out of place here.

261: "sea level equivalent ice volume (in units of mSLE) "
Please describe or provide an equation how this calculation is made. Is this based on VAF without correcting for bedrock elevation change and/or density difference between fresh water and ocean?

L265: "(blue) "
blue lines in Fig. 6

L266: "… 1000 yr delay. "
I might be confused - Why is this considered a 'delay'? Between t1 and t2, PISM is accounting for RSL changes between t0 and t1 calculated by VILMA, so at t1 PISM is not seeing any "delay" in RSL change that happened between t0 and t1?

L266-267:"This is mostly a consequence of the diagnostic calculation of the sea level equivalent ice volume (above flotation) that "
Why would the diagnostic calculation of the SLE VAF influence on the discontinuity, especially just for the 1000yr coupling interval? Even if so, would that be the entire reason? The timing of the appearance of the most pronounced discontinuity seems to coincide with the when Melt Water Pulse 1A event during which so much of the ice sheet melted from the Northern Hemisphere. I wonder if the AIS from PISM experienced a large far-field sea-level feedback from the Northern Hemisphere over each coupling interval, which might have influenced the ice sheet to be more unstable and thus affect the shape of the curve? I'm thinking of the mechanism introduced in Gomez et al. 2020. To check if this really an artifact of the SLE calculation, it would be helpful to show the plot just on ice volume or mass that is a raw output of PISM with no post-processing.

L267: "… also accounts for changes in bed elevation. "
I see. And density correction? Also, provide reference - Goelzer et al. 2020?

L269: "(blue dashed) "
blue-dashed line in Fig. 6

L269-270 & 278: "Comparing the ice sheet response for 100-yr (default) and 10-yr coupling time steps, we find only little differences, which we interpret as 'convergence' "
I think it is important to put the convergence your results within the context of spatial resolution of both PISM and VILMA, as it must depend on both.

L276: "accurate "
precise, rather than accurate.

Figure 6a:

What is 'dbdt' in blue dashed line in subplot a)?

L285: "blue and black dashed lines"
blue solid and black dashed lines

L284-286: "A coarser spatial resolution in the sea level equation, however, delays deglaciation during the Holocene (blue and black dashed lines for n64 and n128, respectively), with up to 2m SLE difference to the reference resolution. "
Any idea why would it be this way?

Figure 6:
The black solid line in panel b) and a) should be equivalent based on the description (3D n512_n128 & dt 100 yr), but they don't seem to be. Also the black solid line in panel b) shows some weird curvature around 11kyr BP, jumping away from convergence. Is this why is this happening? As it is no clear pattern of convergence towards the black solid line in Fig. 6b, I wonder what's the rational for the authors decision to take this run (3D n512 n128) to expand onto doing sensitivity test to coupling interval. I suggest authors to re-check their plots (including the consistency between the black solid lines in Fig. 6a and Fig.6b) and re-do the experiment if necessary.

L290: "ice-dynamical effects for variation of the underlying Earth structure, "
You mean to say "effects of lateral variations in the underlying Earth structure on ice dynamics"?

L295: "mio. "
Describe what this is.

L313: "e.g. by viscoelastic bedrock uplift"
Include the other effects too since sea level change happens combination of changes in the sea surface height and bedrock height.

L319: "light orange shading, "
the orange shading is not just far-field effects but combination with near-field effects, but it sounds like the light orange shading only represents far-field effects.

L320: "cf. Wan et al., 2022 "
Wan et al. explores only the GIA model, not coupled ISM-GIA model. The original coupled modeling studies that saw this effect would be the ones to cite here. Also, make the use of 'cf' consistent throughout the text.

L321:"rates of ice volume change and grounding line retreat remain comparably high until present "
But it's mentioned earlier the ice volume (SLE) has slower decline during the deglaciation phase.

L341:"'3D ref' shows a trajectory in between those two end members "

This does not seem to be true for the post-LGM deglaciation phase, and the reason need to be discussed clearly in the text.

L343:"characteristics due to the weak Earth structure (‘3D min’) seem to dominate the overall ice sheet response, … "
Describe what this means.

L366: "Antarctica"
Represented in the simpler Earth model.

L366: "which also alters the global mean sea level "
Little confusing what “which” refers to and what this sentence means as a whole, regardless.

L365: "7mSLE"
This seems rather a huge difference (around 10% of the total LGM SLE). The difference might be reduced if the 1D model incorporated a lower upper mantle viscosity ((10^18 or 10^19 PaS), which is a better approximation of the viscosity in the West Antarctic region.

L372: "In our coupled simulations we focus on the dynamics of the AIS in response to the northern hemisphere ice sheet’s decay.
Add “For this section,”.

L376"weak structure "
weak Earth structure

L381: "a synchronous coupling. "
If you mean “synchronous” as the ISM and GIA model communicate every coupling time step, Gomez et al. also uses synchronous coupling. Also, the coupling scheme presented in this work doesn't seem to be different in Gomez et al.'s.

L381: "we run iterations over the last two glacial cycles (246 kyr instead of 40 kyr). "
Given that the LGM deglaciation signal would be so dominant for the deglacial period (post-LGM), how might running over two glacial cycles improve your results compared to running one glacial cycle from the LGM peak?

L389: "As barystatic mean sea level change since LGM we find 120m (about 107m from the northern hemisphere in ICE-6G_C) "
It's already mentioned in Line 383 that 13m SLE is seen from your simulation.

L390: "we prescribe the present-day AIS configuration, "
do you mean when you “fix” the AIS through the GIA simulation and just see the fingerprint of the NH ICE6GC on Antarctica?

L378: "ICE-5G, "
Gomez et al. 2020 also did sensitivity test using different NH ice histories using ICE6GC.

L393: "viscoelastic "
viscoelastic deformational

Figure 10:
The figure captions are almost identical to the one from Gomez et al. I don't know the rule for when you are reproducing plots from other papers, but you should make sure you are not plagiarizing by accident.

Figure10:"analogous to Gomez et al. (2020) "
Include the figure number from Gomez et al. as well?

L395: "forebulge effect "
The last time this term appears is in the abstract, and it would be helpful to have a clear description on what this is before using this term.

L396: "This effect is presumably smaller "
Possibly because the ice mass loss in Gomez et al. is much smaller than the simulation shown in this work. (referring to the comparison made in L383).

Figure 8:
Describe the contour lines

L428: "present-day"
Remove "-"

L428: "lowest convergence rates "
you mean the "lowest number of iteration" required to converge modeled present-day topography to observation?

L430: "The maximum LGM extent is very similar to the ... "
Inferred from what?

L432: "then "
Than

L433:"GIA feecback"
sea-level feedback seems to be a more precise word to use because the definition of GIA in this work seems to only represent the deformational part.

L450:"…as sufficient for capturing the relevant dynamics and feedbacks in glacial cycle simulations."

Sufficient seems to be quite a vague word and maybe misleading. While it's true you are able to see differences in coupling vs. no-coupling with the coarse resolutions, it would be important to acknowledge the importance of high-resolution for capturing accurate ice dynamics at the grounding lines shown in many modeling studies.

L457: "higher resolutions are possible "
Is this true for the ice model as well?

L464: "slow response "
More appropriate expression would be something like “slows the retreat rate of the ice sheet.”

L467: "which can even cause grounding line re-advance. "\
But shouldn’t this be the other way around because on slow uplift on retrograde-sloped bed would leave the MISI to keep acting.

L451:"outer iteration "
This word has not been used in the text. I would suggest writing “topographic correction” or “iterative correction for initial topography” or some sort that is much more clear.

L477: "This provides confidence in the application of the PISM-VILMA coupling framework to future ice sheet and sea level projections and the investigation of tipping point characteristics "
This is a rather strong statement to make. Ice-sheet models that focus on predicting future sea level emphasize the importance of high-resolution and higher-order velocity solvers for capturing rapid evolution at grounding lines as well as small-scale peripheral glaciers. Plus, the work by Albrecht et al. 2020b. uses the same ice-sheet model, PISM, so I'm not sold by this argument. As mentioned in a comment above, the importance of high-resolution ice sheet modeling for grounding line dynamics and the short-coming of the model used here need to be acknowledged.

References:

Coulon et al 2021. Contrasting Response of West and East Antarctic Ice Sheets to Glacial Isostatic Adjustment

Latychev et al. 2005. Glacial isostatic adjustment on 3-D Earth models: a finite-volume formulation

Han et al. 2021. Modeling Northern Hemispheric Ice Sheet Dynamics, Sea Level Change, and Solid Earth Deformation Through the Last Glacial Cycle